ELECTRODYNAMICS FROM

AMPÈRE TO EINSTEIN

Olivier Darrigol

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For Dennis Dang

Preface

Electrodynamics, as Ampère defined it in the early 1820s, is the science of the forces exerted by electricity in motion.[1] It emerged as an important field of study soon after Oersted's discovery of electromagnetism. The present book follows the evolution of the subject from its beginnings to Einstein's theory of relativity. This is not, however, a purely internal history. Proper understanding of some central episodes requires excursions into other domains of physics, and even beyond physics: into chemistry in Faraday's case, engineering in Thomson's, and physiology in Helmholtz's. Conversely, the history of electrodynamics illuminates the general history of nineteenth century physics and its relations with other disciplines.

In 1910, Edmund Whittaker published the first volume of his great *History of Aether and Electricity*, which includes a remarkably clear account of nineteenth century electrodynamic theories. Whittaker is most insightful when dealing with the British tradition in which he himself was trained. By contrast, his descriptions of continental electrodynamics are often modernized; pay little attention to broader methodological issues; and largely ignore experimental activity. These flaws have been partly corrected by more recent historiography on the subject, yet the newer studies tend to be local and confined to one actor, to a narrow period of time, or to a given tradition.

There is then clearly a need for an up-to-date synthetic history of electrodynamics. Studies limited to a short time period inevitably lose sight of long-term resources and constraints that shape the physicists' activity. This is particularly true when the time span of the historical account is shorter than the memory of the main actors. For example, the available histories of relativity generally ignore crucial aspects of nineteenth century electrodynamics of which Einstein was himself aware. Longer-term history can correct such defects. It also helps perceive large-scale changes in methods and disciplinary boundaries. For example, the present study documents the increasing quantification of physics, the evolution of the relationship between theoretical and experimental practices, and the merging of theoretical optics and electromagnetism. Taking a bird's eye view, we can better appreciate the continuities, variations, and interplay of various activities and traditions.

[1] For an explicit definition, see Ampère 1826b: 97.

The sheer number and variety of nineteenth century publications on electrodynamics makes impossible an exhaustive history of the kind given in John Heilbron's admirable *Electricity in the 17th and 18th Centuries.* To narrow my task, I have confined myself to works on the forefront of fundamental electrodynamics. I have focused on concept formation and methodological innovation, and have neglected the more conservative, derivative, or isolated contributions. In particular, I have left aside technological applications of electricity, unless there was a feedback effect on the conceptual and instrumental equipment of fundamental electrodynamics. As a consequence of these choices, the present work ascribes a prominent role to the few actors who transformed the foundations of electrodynamics by their experimental, conceptual, and institutional efforts. I have nonetheless described the spread and stabilization of the main innovations, with a special emphasis on those which had broader significance in the evolution of nineteenth century physics.

Three epistemological themes underly my narrative. The first is the relation between experimental and theoretical practice. Until the 1860s, the chief electrodynamicists were as much experimenters as they were theorists. Their conceptual innovations depended on harmonious blends of experimental and theoretical procedures. In order to show how the kind of blend depended on local or individual circumstances, I have adopted a comparative approach, opposing for instance Faraday to Ampère, or Weber to Neumann. The second theme is electrodynamics as a testing ground for various forms of mechanical reductionism. Essential innovations in electrodynamic theory depended on attempted reductions to mechanical systems. Conversely, the mechanistic ideal evolved according to the specific needs of electrodynamics. The third theme is the communication between different traditions. A well-known characteristic of the history of electrodynamics is the long coexistence of field-based and distance-action approaches. Less known are the various strategies that physicists of these two traditions developed in order to communicate with one another. For example, Maxwell distinguished a more phenomenological level of electrodynamic theory that could be shared by continental physicists; and Helmholtz reinterpreted Maxwell's theory in terms of the continental concept of polarization.

This thematic structuring reveals new aspects of the history of electrodynamics, and of nineteenth century physics more generally. First, it is shown that the coordination of experimental and theoretical practice by the same actor involved methodological principles that guided both experiment and theory. For example, Faraday followed a principle of contiguity according to which both the exploration and the representation of phenomena were about 'placing facts closely together'; Ampère based both his theory and his experiments on the decomposition of electrodynamic systems into current elements. When such transverse principles operate, historians can no longer separate the experimental and theoretical activities of a given actor; and philosophers can no longer regard one activity as simply controlling the other.[2]

[2] For a general discussion of transverse methodological principles, cf. Darrigol 1999.

The theme of mechanical reductionism would bring little historiographical novelty if mechanical reduction was regarded as a pure ideal referring to the actors' metaphysics. In this book, however, the emphasis is on the illustrative or algorithmic procedures that concretize this ideal. These procedures are more variable, more context-dependent, and less personal than the idealistic view would imply. Proponents of the mechanical world-view, like Thomson, Maxwell, and Helmholtz, adjusted their reductionist practices according to the evolving needs of theory construction and communication. Later opponents of the mechanistic ideal questioned not only its Kantian underpinning, but also its effectiveness for building and expressing theories.

My third theme, the communication between different traditions, is the most likely to disturb historiographical and epistemological habits. Previous studies of nineteenth century physics have oscillated between two extremes. In the more traditional studies, differences between traditions are meant to be decorative, and communication unproblematic. In the more recent, post-Kuhnian, studies, differences between traditions are often taken to be so radical that communication is nearly impossible among them; knowledge becomes essentially local. An intermediate picture emerges from the present study. Several pairs of traditions are identified (British/Continental, Weberian/Neumannian, Thomsonian/Maxwellian, etc.) in which deep differences existed at various levels, ranging from ontological commitments to socio-institutional, experimental, and theoretical practices. Yet representatives of these antagonistic traditions communicated in ways that permitted comparisons, adaptations, and cross-fertilizations. In fact, the most creative actors desired and planned this interaction. The variety of communication devices described in this study should inform discussions of the objectifying and uniformizing goals of science.

The main text of this book is organized as follows. Chapter 1 recounts Ampère's and Faraday's reactions to Oersted's discovery of electromagnetism in the 1820s, and how they founded a new science of electrodynamics. Chapter 2 shows how in the 1840s two important research traditions emerged in Germany from quantitative studies of magnetism and electrodynamics, the leaders being Gauss and Weber on the one hand, and Neumann and Kirchhoff on the other. Chapter 3 is devoted to two systematic ways of introducing entities in the space between electric and magnetic sources: Faraday's in the 1830/40s and William Thomson's in the 1840s. Chapter 4 describes the formation of Maxwell's theory until the *Treatise* of 1873, while Chapter 5 recounts the British elaborations of this theory in the 1880s. Chapter 6 shows how Helmholtz provided a general framework for comparing the predictions of the existing theories of electrodynamics; how Hertz, working in this framework, produced and detected electromagnetic waves; and how German physicists then read Maxwell. Chapters 7 and 8 recount two ways in which ions or electrons were injected into Maxwell's theory: in connection with empirical studies of electric conduction through solutions and gases, and in connection with the difficulties of electromagnetic optics. Lastly, Chapter 9 deals with various approaches to the electrodynamics of moving bodies at the beginning of the twentieth century, including Einstein's relativity theory.

In the more theoretical sections, I show how in some cases the available mathematics constrained the conceptual developments, while in some others new physical pictures called for new mathematics. In the main text, however, I have kept formalism to a minimum. A series of appendices provide more of the mathematical apparatus. There I freely use anachronistic methods and notations, because my only point is to show briefly the consistency, completeness, and interrelations of the corresponding theories. In the main text, I have carefully respected the original styles of demonstration. My only liberty has been to replace Cartesian coordinate notation with modern vector notation, for the latter can be to a large extent regarded as an abbreviation of the former. Sections devoted to the origins of the vector notation should correct any resulting misconception.

My study of the vast primary literature over the past few years has been greatly aided by the abundance and excellence of more focused histories of electrodynamics. On Ampère, I have often followed Christine Blondel's elegant, authoritative account. On Faraday, I owe much to Friedrich Steinle's deep and systematic studies, and to earlier works by Pearce Williams, David Gooding, and Manuel Doncel. On Gauss and Weber, and their geomagnetic program, my guides have been Christa Jungnickel and Russel McCormmach. Their monumental history of the rise of theoretical physics in Germany has provided much of the background for the German side of my story. On Franz Neumann, both his experimental style and his institutional role, I have relied on Kathryn Olesko's impressively thorough study. On William Thomson (Lord Kelvin), I owe much to the important biography by Crosbie Smith and Norton Wise. These scholars highlight the role of Thomson as a cultural mediator and bring out major shifts of British physics in the nineteenth century. On Maxwell, my main sources have been Peter Harman's excellent edition of his letters and papers, Norton Wise's incisive commentary of the earliest steps to field theory, Daniel Siegel's lucid account of the vortex model, and the descriptions that Jed Buchwald and Peter Harman provide of the basic concepts and program of the *Treatise*. On the spread and evolution of Maxwell's theory in Britain, I have used Bruce Hunt's admirably rich and well-written book, as well as Buchwald's earlier insights into the phenomenological and dynamical aspects of Maxwellianism. On the crucial role of the Faraday effect through the history of British field theory, I have frequently referred to Ole Knudsen's illuminating study. On Helmholtz's and Hertz's physics, I profited greatly from Buchwald's latest book, with its acute scrutiny of laboratory work and the connections he reveals between experimental and theoretical styles. For some aspects of the history of conduction in gases, I have relied on valuable studies by John Heilbron, Isobel Falconer, Stuart Feffer, and Benoît Lelong. On electron theories, my main sources have been again Buchwald and Hunt, but also the earlier, insightful studies by Hirosige Tetu. To which I must add, for the later evolution of the electrodynamics of moving bodies, the competent edition of Einstein's papers under John Stachel's lead (for the two first volumes), and the authoritative studies by Gerald Holton, Arthur Miller, Michel Paty, and Jürgen Renn.

No matter how rich these sources and how strong my efforts to synthetize and complement them, I do not pretend to have closed a chapter of the history of science.

On the contrary, I hope to stimulate further studies and reflections beyond the self-imposed limitations of my own work and into the gaps of which I am still unconscious. The lofty summits of the history of electrodynamics will no doubt attract new climbers. I shall be happy if I have marked out a few convenient trails in this magnificent scenery.

The research on which this book is based required access to well-equipped institutes, libraries, and archives. I was fortunate to belong to the REHSEIS group of the Centre National de la Recherche Scientifique and to receive the warm support and competent advice of its director, Michel Paty. Most of my reading and writing was done in wonderful Berkeley, thanks to John Heilbron's and Roger Hahn's hospitality at the Office for History of Science and Technology. Even after his retirement from Berkeley, John's help and advice have been instrumental in bringing this project to completion. I also remember a fruitful year spent at UCLA, in the inspiring company of Mario Biagioli. Most recently, I have benefitted from the exceptional facilities of the Max Planck Institut für Wissenschaftsgeschichte in Berlin, thanks to Jürgen Renn's regard for in my work.

When I came to the history of electrodynamics, I contacted Jed Buchwald, to whose penetrating studies I owed much of my interest in this subject. At every stage of my project, he offered generously of his time to discuss historical puzzles and to help sharpen my results and methods. Another leading historian of electrodynamics, Bruce Hunt, has patiently read the whole manuscript of this book and provided much incisive commentary. This exchange has been exceptionally fruitful and pleasurable. I have also received valuable suggestions from two anonymous reviewers, and technical advice from a prominent physicist, Jean-Michel Raimond. My highly competent editor at Oxford University Press, Sönke Adlung, is partly responsible for these fruitful exchanges.

Some friends and scholars have personally contributed to improve individual chapters of this book. Friedrich Steinle offered valuable comments on the first chapter. Matthias Dörries clarified obscurities of the second. Françoise Balibar helped me reshape the three first chapters. Norton Wise discussed with me some mysterious aspects of Thomson's fluid analogies in Chapter 3. Bruce Hunt helped me refine some of the arguments in Chapters 4 and 5. Andy Warwick showed me a chapter of his forthcoming book that illuminates the reception of Maxwell's theory in Cambridge. Jed Buchwald recommended alterations in Chapter 6. Edward Jurkowitz suggested the characterization of Helmholtz's approach in terms of frameworks. He and Jordi Cat helped me formulate the arguments of Chapter 9.

To these colleagues and friends, I express my deepest gratitude, and my apologies for having sometimes failed to follow their suggestions. I am of course responsible for any remaining imperfections.

Paris O. D.
May 1999

Contents

Conventions and notations

- For two vectors **A** and **B**, $\mathbf{A} \cdot \mathbf{B}$ denotes their scalar product, and $\mathbf{A} \times \mathbf{B}$ their vector product.
- The symbol ∇ ('nabla') denotes the gradient operator. Hence, for a vector field **A**, $\nabla \times \mathbf{A}$ denotes the curl of this field, and $\nabla \cdot \mathbf{A}$ its divergence.
- The symbol Δ denotes the Laplacian operator.
- The symbol d**l** denotes an element of length, ds an element of curvilinear abcissae, d**S** a surface element, dτ a volume element, δ a variation, $\partial/\partial x$ or ∂_x the partial derivative with respect to x, D/Dt a convective derivative (see Appendix 5).
- The notations of the various electric quantities have been made uniform through the book (exceptions will be clear from the context), as follows:

A : vector potential
B : magnetic induction
c : [electromagnetic unit of charge]/[electrostatic unit of charge] (which equals the velocity of light in Maxwell's theory)
C : relative velocity for which the Weber force between two uniformly moving electric particles vanishes ($C = c\sqrt{2}$)
D : electric displacement
e : electrolytic quantum of charge
E : electric force (on a unit point charge)
ε : dielectric permittivity (except in Chapter 2, where it denotes Neumann's constant for electromagnetic induction)
f : mechanical force
ϕ : electric potential
H : magnetic force (on a unit point charge)
h : Hall's constant
i : intensity of an electric current
j : density of the electric conduction current
k : basic constant of Helmholtz's electrodynamics

κ : electric polarizability
χ : magnetic polarizability
$\mathbf{J}$: density of the total electric current (including Maxwell's displacement current)
L : Lagrangian
m : mass
$\mathbf{M}$: magnetic moment
n : optical index
μ : magnetic permeability
P : Neumann's potential
$\mathbf{P}$: dielectric polarization
$\mathbf{\Pi}$: Poynting's vector
q : electric charge
$\mathbf{r}$: position vector
ρ : charge density
σ : conductivity
σ_{ij} : Maxwell's stress system
t : time
T : kinetic energy
$\mathbf{u}$: velocity of the Earth
$\mathbf{v}$: velocity
U : energy
V : potential or potential energy
x, y, z : Cartesian coordinates

- Four systems of units are used: electrostatic, electrodynamic, electromagnetic, and rationalized electromagnetic units. The first three systems are defined in Appendix 2. The fourth derives from the third by eliminating the 4π factor in the source terms of the field equations. Applied to Maxwell's theory, the rationalization gives $\nabla \cdot (\varepsilon \mathbf{E}) = \rho$ and $\nabla \times \mathbf{H} = \mathbf{J}$. The corresponding potentials and the resulting expressions of Coulomb's and Ampère's force laws involve a divisor 4π (the mathematical cause of the 4π being the identity $\Delta(1/r) + 4\pi\delta(\mathbf{r}) = 0$). In general, for a given theory the unit system is used for which the fundamental equations are simplest: electrodynamic system for Ampère's theory, electrostatic for Weber; electromagnetic for Neumann's and Helmholtz's, rationalized electromagnetic for Maxwell's, Heaviside's, and Hertz's. This usage sometimes contradicts the inventor's choice: Helmholtz preferred electrostatic units, and Maxwell used *partially* rationalized units.

- Citations of sources are in the author–date format and refer to works listed in one of the two bibliographies (primary or secondary literature). Abbreviations used in citations and in the bibliographies are explained on pp. 443–4 below. When a reprint is mentioned (by 'Also in . . .') for a bibliographical item, page numbers refer to it. Square brackets enclosing a date indicate that the work in question is an unpublished manuscript. The symbol # indicates a paragraph number.

- When a given bibliographical entry indicates several publications of the same text, page numbers in a citation of this entry refer to the last of these publications.
- Translations are mine, unless I am quoting from a source which is, or includes a translation.
- Figures from Faraday's diary are reproduced by permission of the Royal Institution.

1

Foundations

1.1 Introduction

In the early nineteenth century electricity was already a wide research field, with diverse methods and multiple disciplinary connections. The oldest and best understood part of the subject was frictional electricity, especially its distribution over conductors and its mechanical effects. In his celebrated memoirs of the 1780s, Charles Coulomb, a military engineer, had founded quantitative electrostatics (later named so by Ampère). He posited two electric fluids, positive and negative, asserted the inverse square law by means of his celebrated torsion balance, and developed its consequences for the equilibrium of conductors in simple configurations. In 1812 Siméon Denis Poisson, one of the first *polytechniciens*, completed the mathematical apparatus of Coulomb's theory. He borrowed from Lagrange's and Laplace's works on gravitation what we now call the potential (V), wrote the corresponding differential equation ($\Delta V + 4\pi\rho = 0$, where ρ is the charge density), solved it in simple cases, and improved the agreement of the theory with Coulomb's experimental results.[1]

Coulomb and Poisson's electrostatics fitted excellently the Laplacian scheme which then dominated French physics. Laplace and his disciples sought to reduce every physical phenomenon to central forces acting between the particles of ponderable and imponderable fluids, in analogy with gravitation theory. In other countries, the number, function, and reality of the electric fluids were controversial issues. The British and the Italians preferred Benjamin Franklin's single-fluid hypothesis, which lent itself equally well to quantitative analysis, as Henry Cavendish had shown before Coulomb. Some of them preferred no fluid at all, or at least avoided direct action at a distance with notions reminiscent of eighteenth century electric 'atmospheres.'[2]

[1] Coulomb 1784–1788; Poisson 1811, 1813. Cf. Whittaker 1951: 57–9, 60–2; Heilbron 1979, 1982: 225–8, 236–40; Blondel 1982: 13–16; Gillmor 1971 (on Coulomb); Blondel and Dörries 1994 (on Coulomb's balance); Grattan-Guinness 1990, Vol. 1: 496–513 (on Poisson).

[2] On Laplacian physics, cf. Crosland 1967; Fox 1974; Heilbron 1993; Grattan-Guinness 1990, Vol. 1: 436–517. On singlism/dualism, cf. Heilbron 1982: 213–18, 228–34 (Cavendish); Blondel 1982: 14–15. On alternative views, cf. Heilbron 1981.

In Germany, the few marginal followers of Friedrich von Schelling's *Naturphilosophie* criticized the general notion of fluids acting at a distance, and sought a deeper unity of nature that would relate apparently disconnected phenomena. They favored a dynamistic, anti-Newtonian view of physical interactions in which matter and force were not to be distinguished: matter was only a balance of two opposite forces, and every action at a distance was to be reduced to a propagating disturbance, or polarity, of this balance. Although these romantic speculations at times bore fruit, they contradicted the basic empiricism of contemporary German physics. For quantitative studies of electricity, the Newtonian fluid theories were the only suitable basis.[3]

The same can be said of magnetism. The chief quantitative theory of this subject was again Coulomb's, based on the assumption of two fluids (austral and boreal) obeying the inverse square law. Most ingeniously, Coulomb explained the impossibility of isolating a magnetic pole by assuming that the magnetic fluids were permanently imprisoned within the molecules of magnetic bodies. His magnetic measurements, however, seemed less reliable than his electric ones, and the arguments in favor of the magnetic fluids were less direct than in the electric case. Hence Coulomb's magnetic theory met more skepticism than his theory of electricity. Yet the analogy between the two theories appealed to Laplace's disciples. Well after Ampère had proposed a contradictory view of magnetism, Poisson applied his mathematical arsenal to Coulomb's view of magnets.[4]

The most popular electric topic was galvanism. It suddenly blossomed in 1800, with Alessandro Volta's discovery of the electric pile. Volta himself regarded the tension and discharge of the pile as an electric phenomenon, therefore belonging to physics. However, other disciplines capitalized on this astonishing device. Its physiological effects and medical applications were intensively pursued, in line with the frog's contribution to Luigi Galvani's discovery. The British discovery of electrolysis attracted the chemists' attention, so that electricity was commonly regarded as a part of chemistry.[5]

In conformity with Volta's original intuition, the electrical, thermal, physiological, and chemical effects of the pile turned out to be the same as those of frictional electricity. It was usually agreed that Volta's device behaved like a battery of Leyden jars that had the mysterious ability to spontaneously recharge itself. When the poles of the pile were connected by a conductor, the discharge unceasingly repeated itself, so that its effects were permanent. In this picture only the state or the pile before discharge seemed amenable to quantitative studies. This may in part explain why quantitative studies of the galvanic current were so scarce before the 1820s.[6]

[3] On *Naturphilosophie*, cf. Caneva 1978; Blondel 1982: 29–30; and Jungnickel and McCormmach 1986, Vol. 1: 27–8 for German rejection.

[4] Coulomb 1785; Poisson 1826. Cf. Whittaker 1951: 59–60, 62–5; Blondel 1982: 16–18; Heilbron 1982: 87–8; Grattan-Guinness 1990, Vol. 2: 948–53 (on Poisson).

[5] Cf. Whittaker 1951: 67–75; Heilbron 1982: 233–6; Blondel 1982: 19–22.

[6] Cf. Brown 1969: 64; Blondel 1982: 21–2; Heilbron 1982: 196.

Beyond the Leyden jar analogy, there were deep disagreements on the cause and nature of the pile's activity. Volta proposed that the electric tension originated in the contact between two different metals. In a series Cu/Zn/mp/Cu/Zn/mp/Cu/Zn . . . (Cu = copper, Zn = zinc, mp = moist paper), the role of the moist paper was simply to avoid the contact Zn/Cu—which would cancel the effect of the previous Cu/Zn contact—without preventing the passage of electricity. Volta verified this assumption by showing that two insulated disks of copper and zinc exhibited opposite electric charges after having been brought in temporary contact. French mathematicians approved Volta's view, in which they saw an opportunity to reduce galvanism to electrostatics. The Swedish chemist Jöns Jacob Berzelius founded his popular doctrine of chemical combination on intramolecular Volta-forces.[7]

The contact theory was less fortunate in England. The leading chemist Humphry Davy found many reasons to assume that chemical changes were responsible for the electric power of the pile. Not only were the pile's effects always accompanied by chemical processes, but the force of the pile appeared to be related to the affinities of the involved chemicals. Davy exploited the latter finding to construct new kinds of pile. He also proposed a mechanism for electrolysis, and suggested, before Berzelius, that chemical forces were of electrical origin.[8]

Altogether, the new science of galvanism offered a striking contrast with electrostatics and magnetism. The latter subjects had reached a state of perfection and were proudly displayed by the French as major achievements of their mathematical physics. On the contrary, galvanism was a rich, disorganized field, growing in multiple directions (physical, chemical, physiological, and medical), but mostly escaping mathematical analysis. In 1820 a radical change occurred: the discovery of electromagnetism suddenly brought galvanism and magnetism in contact, and blurred the methodological and socio-professional borders that separated the two topics. After a summary of Oersted's discovery, the present chapter offers an analysis of Ampère's and Faraday's resulting works that founded electrodynamics.

1.1.1 Electromagnetism

Despite the mathematical analogy of their fundamental laws of equilibrium, electricity and magnetism were generally thought of as completely disconnected phenomena. Their causes and their effects were utterly different: electrification required a violent action and implied violent effects such as sparks and thunder, whereas magnetism seemed a very quiet force. The magnetizing effect of thunder, which had long been known, was regarded as a secondary effect of mechanical or thermal origin. Yet in 1804 an illuminated *Naturphilosopher*, Johann Ritter, believed that he had found an action of the open pile on the magnet, and even

[7] Cf. Whittaker 1951: 71–2; Brown 1969: 76–82 (on the French theory); Blondel 1982: 22–3; Whittaker 1951: 78–9 (on Berzelius).

[8] Cf. Whittaker 1951: 74–6; Blondel 1982: 25–7.

announced the electrolysis of water by magnets. He was soon ridiculed by the French demolition of his claims. Anyone who knew of this episode and assumed distinct fluids for electricity and magnetism was naturally predisposed against similar attempts.[9]

In July 1820, Hans Christian Oersted, a Danish Professor and a friend of Ritter, sent to the leading European physicists a Latin manuscript with the stunning title: *Experimenta circa effectum conflictus electrici in acum magneticam*. Immersed in the depths of German *Naturphilosophie*, he had long expected a connection between electricity and magnetism. He understood the galvanic current as a propagating alternation of decompositions and recompositions of the two electricities, and made this 'electric conflict' the source of heat, light, and possibly magnetism. No more needs to be said of Oersted's philosophy, given that the leading explorers of electromagnetism did not bother to investigate it further.[10]

Most of Oersted's fundamental text was a precise description of a number of experiments performed with a galvanic source, a connecting wire, and a rotating magnetic needle. For the galvanic apparatus, he followed a recipe by Berzelius: 20 copper–zinc cells filled with a sulfo-nitric mixture. He made sure that the wire turned red when connected to the apparatus, as a test of strong electric conflict. He suspended the magnetic needle as is usually done in a compass, let it assume its equilibrium position along the magnetic meridian, approached the wire and connected it to the pile.[11]

In the first of Oersted's experiments, the wire is above the needle and parallel to it. If the Northern extremity of the wire is connected to the negative pole of the pile, the North pole of the needle moves toward the West.

Next, Oersted displaced the wire toward the East or the West, and observed the same action, though a little weaker. He commented: 'The observed effect cannot be attributed to an attraction, because if the deviation of the needle depended on attractions or repulsions, the same pole should move toward the wire whether the latter be on the East side or on the West side.'[12]

Oersted then varied the respective orientations of needle, wire, and magnetic meridian. Two of the resulting experiments deserve special mention, because of their resemblance to later observations by Ampère and Faraday. In the first, the wire is vertical with its lower extremity connected to the positive pole of the pile, and it faces the North pole of the needle. Then this pole moves toward the East. If instead the wire, being still vertical, faces one side of the needle (East or West), between the North pole and the center of the needle, the North pole moves toward the West. In the other interesting experiment, the wire is bent to a vertical U-shape. Then each face of the U attracts or repels the poles of the needle.[13]

From his observations Oersted drew three essential conclusions:

[9] Cf. Blondel 1982: 27–30.

[10] Oersted 1820; 1812, 1813 for the electric conflict. Cf. Meyer 1920; Stauffer 1957; Caneva 1980; Heilbron 1981: 198–9.

[11] Oersted 1820: 215. [12] Oersted 1820: 216. [13] Oersted 1820: 217.

1. The electric conflict acts on magnetic poles.
2. The electric conflict is not confined within the conductor, but also acts in the vicinity of the conductor.
3. 'The electric conflict forms a vortex around the wire.'

To justify the third point, Oersted argued:[14]

Otherwise one could not understand how the same portion of the wire drives the magnetic pole toward the East when placed above it and drives it toward the West when placed under it. An opposite action at the ends of the same diameter is the distinctive feature of vortices.

Finally, Oersted proposed to complete the picture of the electric conflict in accordance with the vorticity of the magnetic action:

All the effects we have observed and described on a North pole are easily explained by assuming that the negative electric force or matter follows a *dextrorsum* spiral and acts on the North pole without acting on the South pole. The effects on a South pole are explained in a similar manner by assuming that the positive electric matter moves in the opposite direction and acts on the South pole without acting on the North pole.

The botanic term *dextrorsum* (defining the helicity of climbing plants) did not survive the competition of Ampère's *bonhomme* or Maxwell's cork-screw. But it was the first of the mnemonic devices that physicists proposed for the polarity of the electromagnetic action. From the beginning, Oersted placed the circle–axis duality at the heart of electromagnetism.[15]

In retrospect, Oersted's observations were accurate and his conclusions insightful. He understood the impossibility of reducing electromagnetism to magnetic attractions or repulsions, and yet saw how to mimic such interactions by curving the conjunctive wire. Most important, he perceived that the action of a rectilinear wire on a magnetic pole was a circular one, centered on the wire. Some features of his memoir, however, hindered a full grasp of its contents. He did not provide any figures or diagrams. He operated in conditions for which the electromagnetic effect is comparable to the magnetic action of the Earth, and therefore reached his general conclusions indirectly, by mentally subtracting the effect of the Earth. He formulated these conclusions in terms of a specific picture of galvanic currents, although his description of individual experiments was purely operational. The essential idea of a circular action appeared only in the context of the electric conflict, an alien notion for most of Oersted's readers.

Despite these obscurities, the astonishing claim of an action between a galvanic current and a magnet was easy to confirm. Within a few weeks, the world's best philosophers entered the attractive lands of electromagnetism. Most of them tried to reduce the new phenomenon to a temporary magnetism of the wire. In this way, they could ignore Oersted's dubious speculations on the electric conflict

[14] Oersted 1820: 218.
[15] Oersted 1820: 218. For a philosophical analysis of the role of axis–loop duality, cf Châtelet 1993.

and apply their previous knowledge of magnetic forces. Yet the two men who most influenced the subsequent history of electromagnetism did not follow this natural course.[16]

1.2 Ampère's attractions

The first exception was André-Marie Ampère, a mathematician with an interest in theoretical chemistry and a passion for philosophy. For physics he had done little, save his early unpublished questioning of the principles of electricity and magnetism. The news of Oersted's discovery changed his fate at age 45. In the Summer of 1820 he launched himself into frenetical researches that would make him, according to Clerk Maxwell's judgment, 'the Newton of electricity.'[17]

1.2.1 Undoing the magnet

Ampère first noted the complication of Oersted's experiments due to the magnetic action of the Earth. He conceived what is now called an astatic needle, that is, a magnetic needle whose rotation plane can be made perpendicular to the action of the Earth. In this configuration the orientation of the needle depends only on the action of the wire. Ampère found the needle to be at a right angle to the shortest line joining the center of the needle to the wire. Here was a simple fact of electromagnetism from which Oersted's more complex observations could be derived.[18]

Then Ampère looked for a similar effect produced by the voltaic battery itself. The experiment was by no means superfluous, because of the lack of consensus on the workings of the battery: the existence of a current within the battery was an open question. Ampère thus formed the concept of a 'circuit' in which 'the electric current' was closed. At the same time, he turned the suspended magnetic needle into a universal current detector, which he soon named a 'galvanometer.' [19]

At that stage Ampère reflected:

> Granted that the order in which two facts have been discovered does not make any difference in the available analogies, we could suppose that before we knew about the South–North orientation of a magnetic needle, we already knew the needle's property of taking a perpendicular position to an electric current [. . .]. Then, for one who tries to explain the South–North orientation, would not it be the simplest idea to assume in the Earth an electric current?

In this view the Earth's magnetic property was reduced to an electric current circulating along the parallels of the Earth. Ampère further imagined that the

[16] For the early reception of Oersted's discovery, cf. Meyer 1920: 101–8; Heilbron 1981: 199–204; Blondel 1982: 44–8.

[17] Maxwell 1873a: #528. On Ampère's biography, and for an accurate bibliography, cf. Hofmann 1995.

[18] Ampère 1820a, 1820b. Cf. Blondel 1982: 69–70; Hofmann 1995: 236–8; Steinle 1998: note 20.

[19] Ampère, 1820a, 1820b. Cf. Blondel 1982: 72–3.

heterogenous composition of the Earth along a parallel made a natural electric pile closed on itself, a device of which he had just proved the magnetic activity.[20]

Ampère then reverted to the analogy between the Earth and a magnet to deduce that every magnet owed its properties to the existence of closed currents in its mass. As a corollary, electric currents had to possess all the properties of a magnet. In particular, an electric current had to attract or repel a magnetic needle. Presumably, a current running in a flat spiral or in a helix would present a North pole and a South pole. Ampère reported these reflections to the French Academy on 18 September, only a few days after Oersted's effect had been demonstrated there, and before he had proven anything but the magnetic action of the current in a battery and the power of an electric current to attract a magnetic needle hung by a thread.[21]

Ampère's new theory of magnetism matched the philosophy of his early unpublished attempts at reforming electricity and magnetism.[22] He believed that a theory based on different kinds of fluids lacked the unity that should be found in God's plans of the universe. There had to be a single fundamental force, preferably one excluding direct action at a distance. The new concept of the magnet was a first step in the right direction, since it eliminated the magnetic fluids. This opinion contradicted Laplacian orthodoxy. Ampère strove, however, to meet other criteria of French mathematical physics. He wished to establish his theory on firm experimental grounds and to cast it in an irreproachable mathematical form.

On 25 September, Ampère showed to the skeptical Academicians that flat helical currents attracted each other and responded to a bar magnet. He had ordered the rather sophisticated apparatus from a competent mechanician. The essential difficulty was to feed the current into the helix without impeding its mobility. Ampère's universal expedient consisted of small mercury cups, in which the extremities of the mobile part of the circuit could rotate and the contact with the battery wires was simultaneously made. With his rotating helices, Ampère believed he had given a 'definitive proof' of the equivalence between magnets and current. Later in the month, he obtained a better imitation of a bar magnet with a helix of current suspended in its middle (Fig. 1.1).[23]

1.2.2 The physical current elements

Ampère's investigations then took a more analytical turn. From the beginning of his researches he expected the interaction between two currents to be analyzable in terms of current elements. Experimentally, this involved the attraction (repulsion) between two portions of parallel (antiparallel), rectilinear currents, demonstrated in October 1820. His device is represented in Fig. 1.2. Except for the mercury cups (R, S, T, U, X, Y) and the surrounding glass box, the construction of the device was entirely dictated by the necessity of isolating the interaction of two current elements, here AB and CD, from the action of the rest of the circuit to which they belong. The

[20] Ampère, 1820b: 238. [21] Ampère 1820a, 1820b. [22] Cf. Ampère [1801].
[23] Ampère 1820a, 1820b. Cf. Blondel 1982: 75–6; Hofmann 1995: 242–4.

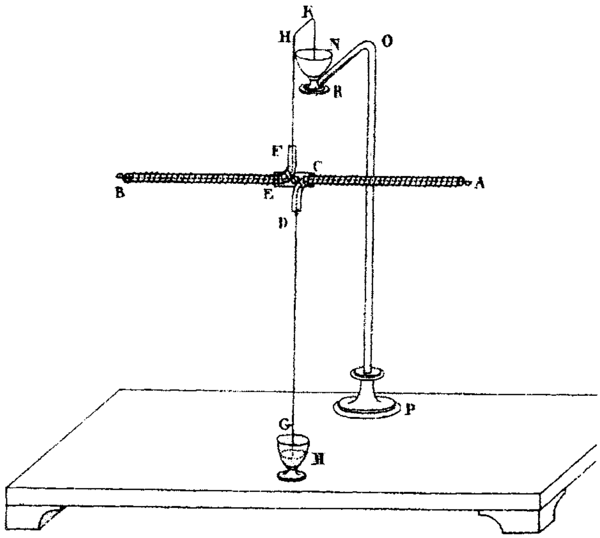

FIG. 1.1. Apparatus for showing the equivalence between a helical current and a bar magnet (Ampère 1820b).

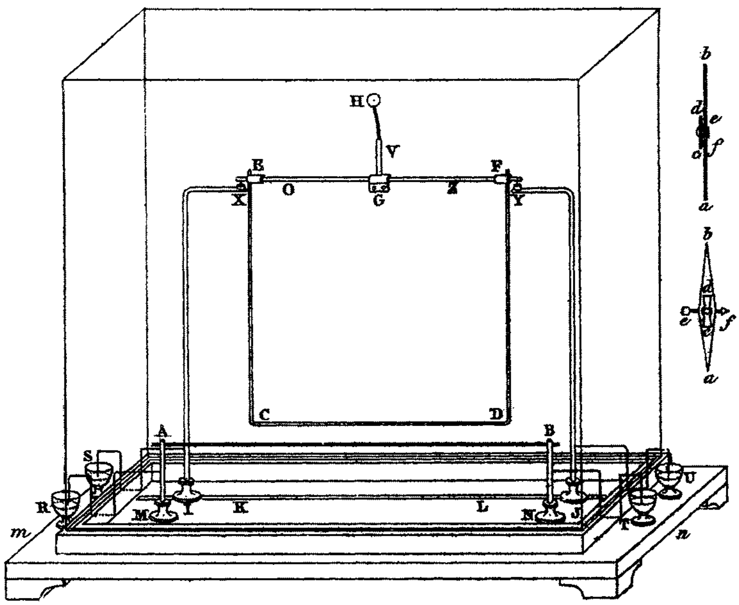

FIG. 1.2. Apparatus for showing the action between two parallel rectilinear currents (Ampère 1820b).

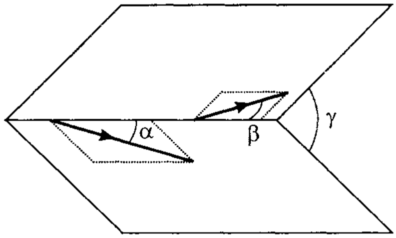

FIG. 1.3. Angles determining the relative orientation of two current elements.

segment AB is longer than CD, and the counterweight H is adjusted so that at the equilibrium position (without currents) CD is very close to AB. Then the action of AB on CD dominates all other electrical actions, and determines the rotation of CDEF around the (non-conducting) axis EF.[24]

In conformity with this concrete possibility of isolating two portions of current, Ampère ascribed a separate physical existence to the force between two current elements. Consequently, he made this force comply with the equality of action and reaction, and he had it lie on the line joining the elements.[25] For information on the angular dependence, he used a device in which the two rectilinear current were free to rotate in planes perpendicular to the line joining their centers. In October he guessed that in the most general configuration the force between two current elements was proportional to

$$\frac{\cos\gamma\sin\alpha\sin\beta}{r^2}, \tag{1.1}$$

the three angles being defined in Fig. 1.3. Analogy with gravitational forces dictated the dependence on the distance r of the two elements, the central character of the forces, and the exclusion of elementary torques. Simplicity, the need to retrieve the properties of magnets, and the two experiments on rectilinear currents suggested the angular dependence.[26]

In the same month, Ampère designed a torsion balance that could measure the force between two current elements in any geometrical configuration, and thus test his conjectured formula. He soon gave up the project, presumably because the variability of his battery and the friction in the mercury cups prevented sufficient precision.[27]

[24] Ampère 1820a (mémoire du 9 octobre), 1820b. The electric forces acting on EC and FG have, to a sufficient approximation, no influence on the motion of the moving part ECDF of the circuit, because the corresponding torque is negligible (assuming with Ampère that the forces between two elements are parallel to the line joining the elements).

[25] Ampère later explained that the forces between two elements had to be central, because if they were not, a perpetual motion could be obtained by rigidly connecting the two elements (Ampère 1826b: 1–2).

[26] Ampère 1820a: 247–8 (mémoire du 9 octobre) has only a brief summary of his analysis. The full version is in Ampère [1820c]. Cf. Blondel 1982: 83–5; Hofmann 1995: 239–41.

[27] Ampère 1820b for the description of two devices of this kind, the first of which was shown to Biot and Arago on 17 October. Cf. Blondel 1982: 84–5; Hofmann 1995: 245–6.

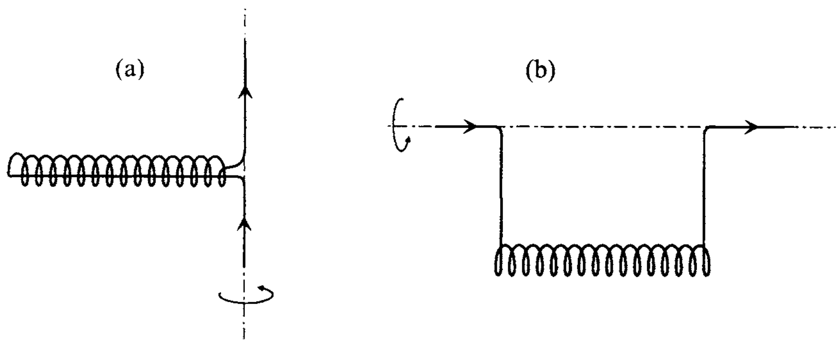

FIG. 1.4. Two kinds of helical current.

Ampère needed another way to justify his fundamental formula. Ironically, he found it in the failure of a less ambitious experiment in which he tested the interactions of two parallel helices. To his surprise, the helices acted like parallel wires instead of imitating parallel bar magnets. He soon recognized the source of the anomaly. In his earlier experiments with a helix, the current was brought to the helix in the manner of Fig. 1.4(a) that permitted rotation around the vertical axis. In the new experiment, it was brought in the manner of Fig. 1.4(b), which permitted rotation around the horizontal axis. The current in a turn of the helix, Ampère surmised, can be regarded as the superposition of a circular current around the axis and a linear current along the axis. Assuming that the action of the composed current is equal to the resultant of the actions of the partial currents, then only the helix of Fig. 1.4(a) can be compared to the parallel circular currents of a magnet; the helix of Fig. 1.4(b) involves the additional action of a linear current, which dominates the former action when the radius of the helix is small.[28]

Ampère detected here a more general principle, according to which any two infinitely short currents with the same extremities were equivalent, no matter how contorted they might be. The principle severely constrained the angular dependence of the force between two current elements. Ampère showed this as follows.[29]

The two elements AG and BH represented in Fig. 1.5 can be decomposed into the elements AM and MG on the one hand, and BP, PQ, and QH on the other. According to the principle and in an obvious notation, the force (AG → BH) is equal to the sum of the forces (AM → BP), (AM → PQ), (AM → QH), (MG → BP), (MG → PQ), and (MG → QH). Call m the force acting between two parallel unit elements of current when they are perpendicular to the line joining their center, and n the similar force when they are on this line. Then, the force (AM, BP) is proportional

[28] Ampère 1820b: 174–6 (mémoire du 6 novembre). Cf. Blondel 1982: 87–8; Hofmann 1995: 246–50.

[29] Ampère [1820d], 1820e, 1820g. In anachronistic terms, we would say that the force is a linear function of each current element regarded as a vector.

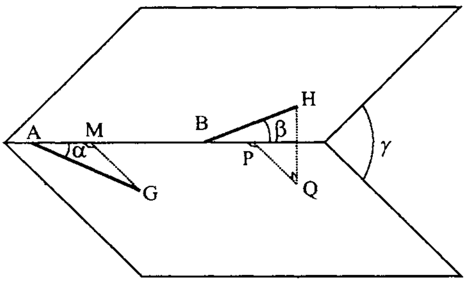

FIG. 1.5. Orthogonal decomposition of two current elements.

to $n\cos\alpha\cos\beta$, and the force (MG, PQ) to $m\sin\alpha\sin\beta\cos\gamma$. All other forces vanish due to symmetry reasons: the geometrical configuration of the corresponding elements is invariant by inversion of one of the currents (neglecting second-order infinitesimals). Consequently, the force acting between two arbitrary current elements has the angular dependence

$$\sin\alpha\sin\beta\cos\gamma + k\cos\alpha\cos\beta, \qquad (1.2)$$

where k is equal to n/m. When he announced this result in December 1820, Ampère believed that he could take k equals *zero* 'without inconvenience.'[30]

To consolidate this beautiful reasoning, Ampère conceived experiments that would directly prove the underlying principle. His first idea was to compare the successive actions of a rectilinear and a sinuous current on a magnet. Again, the instability of the galvanic source hampered the project. In the end Ampère had the rectilinear current and the sinuous current act simultaneously on a third mobile current placed at equal distances. Thus was born his famous *méthode de zéro*. As was usual for him, Ampère described the apparatus and the expected results before they were made. On the drawing he provided (Fig. 1.6), SR and PQ are the two currents to be compared, and GH is the test current. Note that Ampère carefully eliminated the effects of the connecting wires. For example, mn and de are placed at equal distance of the test current; the leaders fg and hi to the test current are very close to each other, so that their effects mutually cancel according to a previous experiment. Ampère avoided the magnetic action of the Earth by including the test current in a double loop GFHI, BCDE.[31]

This experiment ended a first phase of Ampère's researches in which Oersted's discovery was the only external stimulus, except for a few remarks by Laplace and by his friends Augustin Fresnel and François Arago. By Christmas 1820,

[30] Ampère 1820e: 229. Cf Blondel 1982: 92–5; Hofmann 1995: 250–2. The symmetry argument for the nullity of the force between perpendicular elements appears for the first time in a note of Ampère 1822a: 209n.

[31] Ampère 1820f (memoire du le 26 décembre), 1822: 162. Cf. Blondel 1982: 96–8; Hofmann 1995: 252–61.

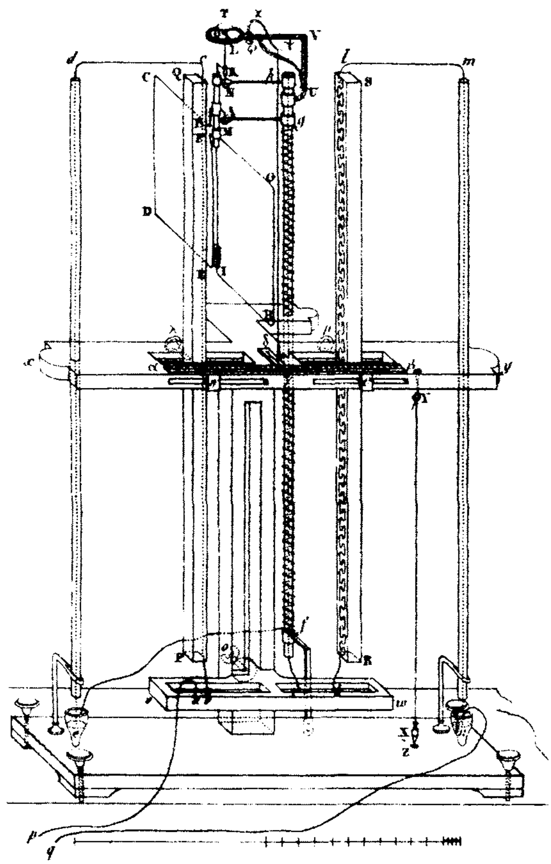

FIG. 1.6. Apparatus for proving the law of sinuous currents (Ampère 1822c).

Ampère had in hand the main elements of his electrodynamics: experimental devices and a mathematical formula for the interaction between two currents, the null method, and the reduction of magnetism to the motion of electricity. Some uncertainty remained on the precise expression of the force between two current elements, and a systematic derivation of the consequences was still missing. However, the main characteritics of Ampère's electric philosophy were already apparent.

1.2.3 Reified theorems

For the most part, Ampère's experiments were planned according to preconceived theoretical ideas. Only the very first experiments had an exploratory value. The more definitive devices were a direct expression of his theoretical beliefs within material constraints such as the compatibility of mobility with current feeding. They served a unique function and could not be transformed to answer new questions. This rigidity was increased by the fact that Ampère, lacking manual skills, always had the apparatus made for him. In general he knew the results of his experiments in advance. Material constraints could, however, lead to instructive surprises, as we saw for the setup with parallel helices.

Ampère's experiments did not yield numbers. In one class of experiments, he showed the qualitative similarity of spirals or helices to magnets. In another class, he examined the more fundamental action between rectilinear currents. There he wished to obtain quantitative results. He failed, however, because the corresponding forces were too small and the voltaic source too unstable. He then switched to the null method, which he believed to provide precision and generality without yielding any number but zero.

To the extent that they reified preconceived ideas, Ampère's experiments played little role in the development of his theory. More instrumental was his critical attitude toward the multiplication of imponderable fluids, which he shared with his friends Fresnel and Arago. He also benefitted from the Newtonian analogy and relevant mathematical techniques, which he learned from the Laplacian circle. At the origin of his intuition that every magnetic action could be reduced to interactions among currents, Ampère saw a virtual history that placed Oersted's discovery before the invention of the compass. His first guesses about the forces between two currents were inspired by his new conception of magnets and by the analogy with gravitational forces.

Yet some of Ampère's experiments contributed to his original intuitions. Most importantly, the failed experiment on parallel helices permitted a basic change of method. From the combination of theoretical conjecture and experimental confirmation, Ampère turned to a more axiomatic method in which the whole theory derived from a few experimentally established principles. The infinitesimal equivalence of rectilinear and sinuous currents was the first of these principles.

Ampère wanted to give his theory a non-speculative outlook. On the nature of the electric current, he followed the French idea of a double flow of negative and positive electic fluids, only adding that the intensity of the flow was the same in all parts of the circuit. He insisted that his deductions did not depend on any particular picture of the electric current, and he kept his speculations on underlying ether processes mostly to himself. He did not regard the new conception of magnets as a speculation: he confused its possibility, demonstrated with helical currents, with a necessity, and he regarded the opposite view as a 'gratuitous supposition.'[32] Lastly, he did not regard the central character of the forces acting between current elements as hypothetical: his rectilinear current apparatus seemed to grant the physical existence of these forces.

[32] Ampère [1820d]: 133.

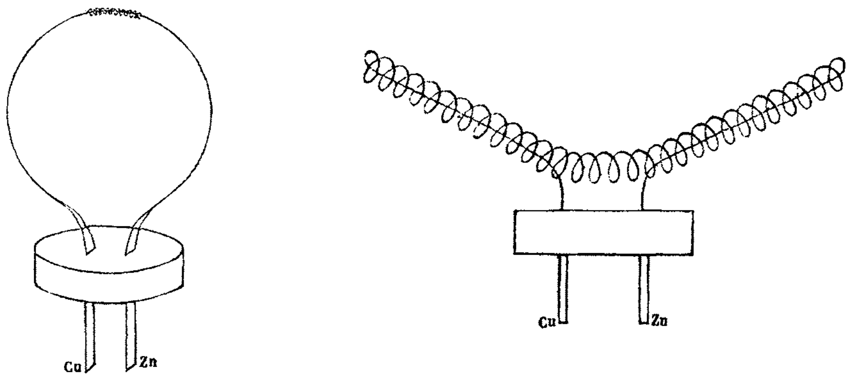

FIG. 1.7. De la Rive's floating devices (G. de la Rive 1821).

1.2.4 Antipathies

In these first months of feverish activity, Ampère's results received more attention than praise. His friends Arago and Fresnel at home, and Gaspard de la Rive in Geneva, seem to have been his only active supporters. Ampère's numerous hurried publications created an impression of confusion, the 'facts' not being clearly distinguished from the theory. The experiments were impressive on paper, but much less so when demonstrated by the inventor. 'Monsieur Ampère is so clumsy,' Laplace said, 'that when his apparatus does not move, he reportedly pushes to shift it.' Oersted was equally unimpressed:

I was at Ampère's by appointment to see his experiments [. . .] He had three considerable galvanic apparatus ready; his instruments for showing the experiments are very complex, but what happened? Hardly any of his experiments succeeded [. . .]. He is dreadfully confused and is equally unskillful as an experimenter and as a debater.[33]

To make it worse, Ampère's devices were much harder to duplicate than Oersted's. His 'ingenious instruments,' de la Rive deplored, 'required skilled workers and fairly high expenses.' As a cheap and easy substitute, de la Rive proposed floating devices made with an acid bath, a cork, a zinc blade, a copper blade, and a piece of wire (Fig. 1.7). With the recipe, he offered the wisdom:[34]

In my opinion we do a favor to Science when we try to diminish the material obstacles that we encounter in our researches and make it possible for a great number of people to study a new experiment: we thus give better chances to new discoveries.

With or without de la Rive's help, physicists quickly accepted Ampère's main 'facts': the attraction between two parallel currents and the analogous behaviors of

[33] Laplace's comment from Colladon 1893: 121; Oersted 1920, Vol. 1: CXIV, both quoted in Blondel 1982: 167n.

[34] G. de la Rive 1821: 201.

a helix and a bar magnet. Yet Ampère's theory met much skepticism or hostility. Foreign physicists were not likely to follow Ampère's mathematical reasoning, and they preferred to see the action between two currents as a consequence of temporary magnetism. The French *savants* were more at ease with Ampère's calculus, but they condemned his departure from Laplacian orthodoxy. Being the heirs of Coulomb's theory of magnetism, they judged Ampère's conception of magnets unclear and questioned the introduction of trigonometric lines in a fundamental force law.[35] They even denied the originality of Ampère's discovery of the interaction between two currents. If, their argument went, a current acted on a magnet and a magnet acted on a current, then a current obviously had to act on an other current. Defending Ampère, Arago objected that two iron keys did not attract each other, although each of them interacted with a magnet.[36]

Ampère's most dangerous critic was the politically and intellectually conservative Jean-Baptiste Biot. More Laplacian than Laplace himself, Biot applied standard French techniques to the determination of the force between a current element and a magnetic pole (that is, the extremity of a long, uniformly magnetized needle). With the help of Felix Savart, he first established by Coulomb's method of the oscillating magnetic needle that the force between a pole and a long rectilinear wire varied as the inverse of their distance. As Laplace told him, this implied a $1/r^2$ dependence for the contribution of a current element to the force. For the angular dependence, he measured the force between a V-shaped current and a pole, varying the aperture of the V. With uncontrolled precision and flawed calculus, he derived the sine we all know. On 18 December 1820, he announced the complete law at the Academy of Sciences.[37]

After one of Ampère's students had pointed to Biot's mathematical mistake,[38] Biot consolidated his proof and argued as follows against Ampère's theory. Ampère's forces between moving electric fluids were 'completely outside the analogies offered by all other laws of attraction.' His interpretation of magnets was a complicated regression to Descartes' vortices. The true course was the Biot–Savart law from which every electromagnetic fact could be deduced without contradicting Coulomb's theory of magnets. Ampère's attractions were nothing but a consequence of the temporary magnetic virtue of the wires carrying the currents.[39]

Biot's criticism could hardly be honest. His own law bore little resemblance to known attraction laws. His explanation of the force between two currents was purely qualitative and depended on a bizarre, if not impossible, distribution of magnetism within the wire, whereas Ampère's description of magnets could be made as precise and quantitative as Coulomb's. Ampère could have used such arguments against Biot. He did not, because he found a more powerful defense in a fact discovered by a British newcomer to the field of electricity.

[35] Cf., e.g., Biot 1824, Vol. 2: 771–2. [36] Cf. Arago 1854, Vol. 2: 58–9.

[37] Biot and Savart 1820, 1821; Biot 1824, Vol. 2: 706–74. Cf. Frankel 1972; Grattan-Guinness 1990, Vol. 2: 923–25.

[38] Savary 1823: 364.

[39] Biot 1824, Vol. 2: 704–74 ('Sur l'aimantation imprimée aux métaux par l'électricité en mouvement'); *Ibid.*: 769–71 (explanation of Ampère's attractions).

1.3 Faraday's rotations

1.3.1 Davy's admirer

When Faraday entered the field of electromagnetism, he was known as the discoverer of a chloride of carbon. His published work was in chemistry, pure and applied. His first interest in science had risen during his apprenticeship in a bookbinder's shop, as he read the books at hand or as he attended popular lectures. The newborn field of galvanism captivated him. With whatever tools and chemicals he could gather, he improvised his own electrochemical experiments.[40]

In his early twenties, Faraday caught Humphry Davy's attention, and became his amanuensis and assistant at the Royal Institution. Founded in 1899, this institute had the official aim of 'diffusing the knowledge, and facilitating the general introduction of useful mechanical inventions and improvements; and teaching, by courses of philosophical lectures and experiments, the application of science to the common purposes of life.' Under Davy's influence, it also became a center for chemical research and popular expositions of science. When Faraday entered the Institution, Davy was a heroic figure both to his peers and to the layman. He was the man who had isolated chlorine and disproved Lavoisier's principle that oxygen was the cause of acidity. Upon Volta's discovery of the pile, he had demonstrated the essential role of chemical reactions in galvanic sources, against Volta's contact theory. He believed in an intimate relation between chemical forces and electricity, and had a critical attitude toward the electric fluids.[41]

Under Davy's lead, Faraday soon became an outstanding chemist in both fundamental and applied matters. He discovered a new compound of carbone and chlorine, a new oil now called benzene, new steels, and new optical glasses.[42] His first excursion beyond pure chemistry occurred in 1820, as a consequence of Oersted's discovery. In the fall of that year he assisted Davy in a series of electromagnetic experiments.

Davy used a much stronger battery than Oersted (100 plates of 4 square inches) and observed that one of the poles of a magnetic needle placed under the wire was 'strongly attracted' by the wire and remained in contact with it. In his opinion, this could be explained only by supposing that the wire itself became magnetic. In order to prove this assumption, he sprayed iron filings on the wire and observed their massive sticking to the wire. He also obtained the magnetization of small pieces of steel. He first interpreted these effects in terms of four magnetic poles in the wire (as Berzelius also did), but soon adopted the idea of 'a sort of revolution of the magnetism around the wire,' which William Wollaston had introduced under the name of 'vertiginous magnetism.' In conformity with this view, small steel needles placed along a circle centered on the wire became magnetized.[43]

[40] Cf. Williams 1965: Ch. 1.
[41] Rumford 1870–1875, Vol. 4: 755. On the Royal Institution, cf. Berman 1978. On Davy, cf. Williams 1965: Ch. 1; Knight 1996.
[42] Cf. Williams 1965: 120–3, 107–8, 109–15, 115–20.
[43] Davy 1821 (read 16 November 1820).

FIG. 1.8. Wollaston's diagram for the attraction of two parallel currents (left) and the repulsion of antiparallel ones (right).

The highly respected Wollaston never published his views in full. In a short published note, he spoke of 'an electromagnetic current' around the axis of the wire, and he provided two drawings explaining the action between two parallel wires (Fig. 1.8). For the same direction of the two currents 'the North and South powers meet' and therefore attract each other. For opposite currents 'similar powers meet' and repulsion results. No one—perhaps not even Wollaston himself—saw clearly what Wollaston had in mind. He seems to have modified Oersted's idea of a helical current to confine it within the conductor. Oersted himself soon interpreted Ampère's forces between two currents in terms of a sympathy, or antipathy, between the corresponding helical motions.[44]

Having assisted Davy in his experiments, Faraday knew the results and Wollaston's speculations. In this period he published an anonymous 'Historical sketch of electro-magnetism,' in which he reviewed the state of experimental and theoretical knowledge in this field. Worth noting are his agnosticism about the electrical current, his fidelity to Davy's conception of electromagnetism in terms of magnetic attractions, and his criticism of Ampère's views. In the first chapter he wrote in unison with Davy:[45]

There are many arguments in favour of the materiality of electricity, and but few against it; but still it is only a supposition; and it will be well to remember, while focusing on the subject of electro-magnetism, that we have no proof of the materiality of electricity, or of the existence of any current through the wire.

On Oersted's experiment, Faraday repeated Davy's conclusions, insisting on the orientation of the needle across the wire when the effect of the Earth could be neglected, and phrasing everything in terms of *attractions* or *repulsions* of the magnetic needle by one side or the other of the wire. He described Oersted's two spiralling forces with the introduction: 'I have little to say on M. Oersted's theory, for I must confess that I do not quite understand it.' He condemned Berzelius's hasty interpretation of Oersted's experiment in terms of four magnetic poles, and reproached Ampère with lack of clarity. Ampère could not pretend, Faraday went on, to provide an electric explanation of magnetism, for his theory lacked a precise

[44] Wollaston 1821: 363; Oersted 1821: 235–6.

[45] Faraday 1821: 196. Very early, Faraday had read the entry 'Electricity' of the *Encyclopedia Britannica*, in which a certain James Tytler defended a vibrational theory of electricity. Cf. Williams 1965: 14–15.

picture of the electric current. As he confided to de la Rive, Ampère's methods were alien to him:[46]

> With regard to [Ampère's] experiments I hope and trust that due weight is allowed to them but these you know are few and theory makes up the great part of what M. Ampère has published and theory in great many points unsupported by experiments when they ought to have been adduced. At the same time M. Ampère's experiments are excellent and his theory ingenious and for myself I had thought very little about it before your letter came simply because being naturally skeptical on philosophical theories I thought there was a great want of experimental evidence.

1.3.2 Rotations and powers

In September 1821 Faraday experimented with a vertical wire and a suspended magnetic needle. Presumably, he did not trust Oersted's results and did not pay much attention to the details of Oersted's account. Also, Davy's repetitions must have been too rough for his taste. According to Davy, the wire attracted one side of the needle and repelled the other. With a large-plate galvanic source, and meticulous variation of the position of the vertical wire, Faraday observed that on a given side of the needle the attraction turned into a repulsion, or vice versa, when the wire passed a certain point located between the center and the extremity of the needle (Fig. 1.9(a)). He concluded that the true poles of the needle were not at its extremities. Most important, the observation excluded his and Davy's view that the motions of the needle resulted from attractions or repulsions between poles and wire. Faraday extrapolated in his mind the motion of a free wire around a fixed magnetic pole (circles of Fig. 1.9(b)), and suspected it to be circular. By trial and error, he soon found a geometrical configuration of a wire and a magnet for which a continuous rotation of the wire occurred (Fig. 1.9(c), (d)).[47]

In Faraday's subsequent elaboration of electromagnetism, this experiment was most basic. All attractions and repulsions observed by Oersted, Davy, and Ampère derived from the simple fact of rotation of a pole around a wire. The action of any wire system on a magnetic pole (concretely, on the extremity of a uniformly magnetized needle) could be traced to the combined circular effects of the different portions of wire. For example, with the drawing of Fig. 1.10 Faraday explained that a system of two parallel wires 'in the same state' (i.e. with the same direction of the current), attracted a pole on one side of their plane, and repelled it on the other. The rotations, real or imagined, connected the various facts of electromagnetism.[48]

The concept of a pole here played a central role. Faraday systematically avoided any reference to the magnetic fluids, and defined the pole as a centre of action. With

[46] Faraday 1822a: 107, 109; Faraday to G. de la Rive, 12 September 1821, *CMF* 1.

[47] *FD* 1: 49–50 (3 Septemper 1821); Faraday 1821, *FER* 2: 127–8. Cf. Gooding 1985.

[48] *FD* 1: 52, #17, #22 (4 September 1821); Faraday 1822, *FER* 2: 133–6, 139–42. Cf. Steinle 1995.

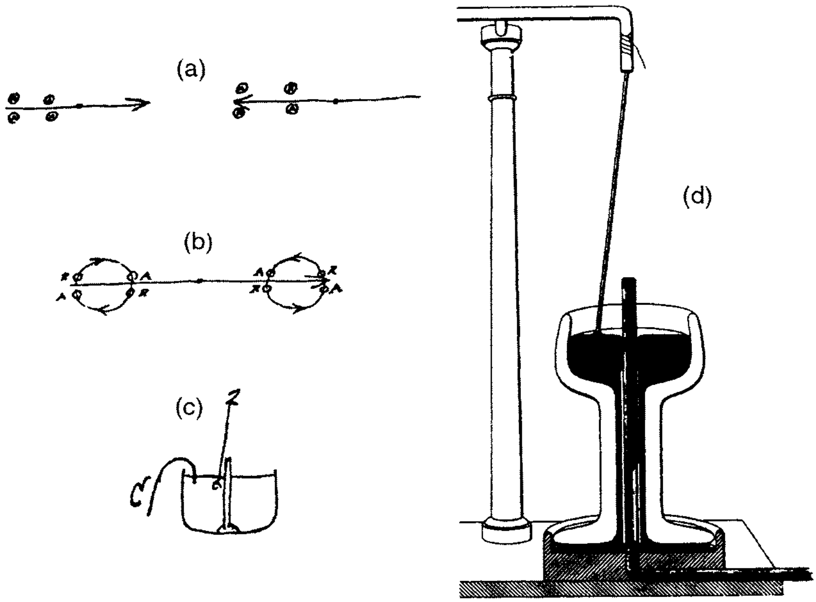

FIG. 1.9. Faraday's steps toward electromagnetic rotations: (a) attractions and repulsions of a wire by a magnetic needle (*FD* 1: 49), (b) imagined rotations (*ibid.*), (c) first rotation device (*FD* 1: 50), (d) classroom version (*FER* 2: plate 4).

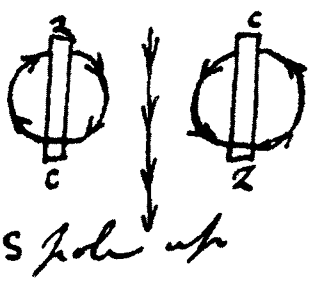

FIG. 1.10. Motion of a magnetic pole between two rectilinear currents (*FD* 1: 51).

this definition, poles were no longer specific to magnets; they could also be produced by electric currents, for example at the extremities of helices. A broader unifying concept was that of 'power,' of which Faraday made abundant use without defining it. Apparently, powers referred to portions of space from which specific actions emanated. Powers could equally belong to a magnet or to the sides of a wire, and they could attract or repel each other:

It has been allowed, I believe, by all who have experimented on these phenomena, that the similar powers repel and the dissimilar powers attract each other; and that, whether they exist in the poles of the magnets or in the opposite sides of conducting wires.[49]

The language of powers was not as universal as Faraday thought it was. But Davy and Wollaston used it, probably to avoid the magnetic fluids, and perhaps out of sympathy for the dynamistic denial of the matter/force dualism. In this language, Faraday explained, the simplest case of magnetic action was that of two centers of concentrated power, that is, the rectilinear attraction or repulsion of two poles. Next came the case of a wire and a pole, which involved three powers: that of the pole, and those of the two sides of the wire.

The pole is at once attracted and repelled by equal powers, and therefore neither recedes nor approaches; but the powers being from opposite sides of the wire, the pole in its double effort to recede from one side and approach the other revolves in a circle.

Then came the case of two parallel wires, which involved four powers, two for each wire. The powers of the facing sides of the wire, Faraday explained, were of the opposite kind when the wires were in the same state and of the same kind for opposite states of the wires, in conformity with the attraction and repulsion observed by Ampère.[50]

The notion of two different powers at the opposite sides of the wire was a bit confusing, as Faraday himself realized:

With regard to the opposite sides of the connecting wire, and the powers emanating from them, I have merely spoken of them as two, to distinguish the one set of effects from the other. The high authority of Dr. Wollaston is attached to the opinion that a single electro-magnetic current passing round the axis of the wire [. . .] is sufficient to explain the phenomena.

However, Faraday needed the notion to explain the actions between currents, and also to express the unity of magnetism and electromagnetism: 'The pole of a magnetic needle presents us with the properties of one side of the wire.'[51]

Faraday thus filled the space around wires and magnets with powers, virtual rotations, and eventually iron filings. In contrast, he left the internal state of wires and magnet undetermined:

I have not intended to adopt any theory of the cause of magnetism, nor to oppose any. It appears very probable that in the regular bar magnet, the steel, or iron is in the same state as the copper wire of the helix magnet; and perhaps, as M. Ampère supports in his theory, by the same means, namely currents of electricity; but still other proofs are wanting of the presence of a power like electricity, than the magnetic effects only.

Note that Amperean currents were not incompatible with Faraday's views. What Faraday rejected was the idea that the attractions and repulsions of two currents were

[49] Faraday 1822, *FER* 2: 128; *Ibid.*: 136.
[50] Faraday 1822, *FER* 2: 136–7, 132–3.
[51] Faraday 1822, *FER* 2: 146, 132.

primitive facts. They were a consequence of the distribution of powers demonstrated in electromagnetic rotations.[52]

1.3.3 Opposite styles

Many other differences existed between Faraday's and Ampère's investigations. Whereas Ampère had reached the basic fact of his electrodynamics by speculative reasoning involving virtual history and analogy, Faraday discovered the continuous rotations by patiently exploring the details of the interaction between a magnet and a wire, which others took for granted. In later stages of their research, Ampère and Faraday both used theory, but theory of a very different kind. Ampère exploited the analogy with the theory of gravitation and reasoned in mathematical terms. Faraday knew no mathematics and thought in terms of vaguely defined powers and concretely imaginable actions. For the first, theoretical unity depended on mathematical deduction from a small number of axioms; for the second, theory was always open, and unity derived from the connexity of the various known experimental facts. As Faraday explained to Ampère: 'I am unfortunate in a want of mathematical knowledge and the power of entering with facility any abstract reasoning. I am obliged to feel my way by facts closely placed together.'[53]

Different kinds of theory implied different styles of experiment. Ampère's rigid, professionally designed apparatus was completely at odds with Faraday's improvised, quickly built devices. Whereas Ampère had to give a few days to his mechanician, Faraday managed proper arrangements of wires and needles in a few minutes. He found de la Rive's little floating contrivances 'very simple, easily made, and effectual' and used them abundantly. He wanted to be able to modify and combine the geometrical configurations as easily and quickly as possible, in part to multiply the possibilities of unexpected effects, in part to provide connections between known facts.[54]

With these differences in mind, one easily understands why Ampère could not discover the continuous rotations, although his theory implicitly contained them. Ampère's devices aimed at proving consequences of his theory that he could predict. He did not foresee the continuous rotations, presumably because the analogy with other theories of attractions hid this phenomenon. For a mechanical system moved by gravitational, electrostatic, or magnetostatic forces, it was well known that these forces, being central, could not compensate for the frictional loss of living force (kinetic energy) during a cycle of the system. In other words, the theorem of living forces forbade perpetual motion. Ampère naturally overlooked that his forces, with their angular dependence, contradicted the premisses of the theorem. Having reduced magnets to currents, he could not imagine that electric currents would allow the perpetual motion that no one had ever succeeded in producing with magnets.

[52] Faraday 1822, *FER* 2: 145–146. Faraday used iron filings to show 'the path the pole would follow' (*ibid.*: 140).

[53] Faraday to Ampère, 3 September 1822, *CMF* 1. [54] Faraday 1821: 288.

1.3.4 How original?

Faraday's discovery of continuous rotations attracted much attention from his British colleagues, though not the kind of attention he expected. The rumor swelled that he had stolen the idea. Wollaston had indeed invoked the possibility of making a wire rotate around its own axis under the influence of a magnet, and had tried—without success—such an experiment with Davy. Faraday defended himself as follows. He was aware of Wollaston's idea and trial, but originally disagreed and interpreted Oersted's phenomenon in terms of attractions and repulsions (as can be verified from the sketch). What led him to the rotations was his closer investigation of the action of a vertical wire on a magnetic needle. And the rotation he produced differed in kind from that anticipated by Wollaston.[55]

There is no reason to doubt Faraday's sincerity. Wollaston himself accepted his explanations. Faraday did not make clear, however, to which extent his discovery was a rediscovery. Were the rotations entirely new? Did previous conceptions of electromagnetism play a role in Faraday's crucial step from the attractions to the rotations?

On the first point, the answer is certainly negative. Oersted's spiralling conflict and Wollaston's electromagnetic current both indicated a circular motion of a magnetic pole around the wire. Moreover, Faraday's observations with the vertical wire added nothing to Oersted's previous observations of the same kind, save the distinction between the poles and the extremities of a magnetic needle. Oersted himself insisted that his observations could not be explained in terms of attractions and repulsions. Faraday innovated in concretizing the rotations, not in imagining them.

If we read Faraday's defense literally, he imagined the rotations only by contemplating the various apparent attractions and repulsions of the needle by the wire. This is possible, but not necessary. Faraday could have unconsciously drawn on Oersted's or Wollaston's idea of a circular action.[56] The essential originality of Faraday lies in the way he could pass from the imagined to the actual rotation. When Wollaston and Oersted understood the circular character of the electromagnetic action, their first impulse was to invent a theoretical cause for it. Wollaston then predicted a case of wire rotation that had little to do with Oersted's original arrangement. In contrast, Faraday avoided speculating on the cause of the circular action. He modified the concrete device on which he conceived the rotation until he could display it in all its splendor.[57]

Imitating de la Rive's advertizing strategy, in September 1821 Faraday mailed to his foreign correspondents a small kit of the rotation device (Fig. 1.11). He provided the cork, the wire, and the glass tube. The happy recipients just had to pour some mercury into the tube and connect it to a galvanic battery. One of them, Jean Hachette, reproduced the wonderful phenomenon in Ampère's company.[58]

[55] Faraday 1823. Cf. Williams 1965: 157–60.

[56] The day he obtained rotations, Faraday noted the absence of rotations of Wollaston's kind: *FD* 1: 50, #7 (3 September 1821).

[57] Cf. Gooding 1985.

[58] Cf. Faraday 1822c: 150–1; Faraday to G. de la Rive, 16 November 1821, *CMF* 1; Hachette to Faraday, 19 Nov 1821, *CMF* 1; Gooding 1985; Blondel 1982: 110.

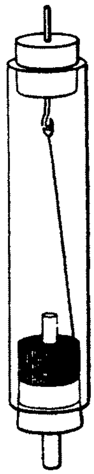

FIG. 1.11. Pocket version of the rotation apparatus (*FER* 2: plate 4).

1.4 *Electro-dynamique*

1.4.1 Awakening

A lung disease and metaphysical broodings had long interrupted Ampère's electromagnetic researches, when he received Faraday's memoir on electromagnetic rotations. Recovery followed magically: 'Metaphysics was filling my head. However, since Faraday's memoir has appeared, all my dreams are about electric currents.' The dreamer soon crafted a number of continuous rotation devices in his own style: with straight wires, mercury cups, and acid baths. The contrivances had less friction than Faraday's, they permitted the substitution of a coil for the magnet, and they were more easily amenable to calculation. On one point Ampère surpassed Faraday: he obtained the rotations of a magnet and a wire around their own axis (Fig. 1.12).[59]

The more theoretical aspects of Faraday's work failed to disturb Ampère: 'This memoir contains very singular electromagnetic facts which perfectly confirm my theory, although the author tries to fight it by opposing one of his invention.' Ampère announced that proper calculations, which he did not provide, explained the rotation in Faraday's original device. More qualitatively, he showed that in his own rotation devices the motion resulted from the forces between the various currents involved. Despite the temporary lack of rigor, Ampère had no doubt: 'These facts comply with the general laws of physics, and one does not have to admit as *a simple primitive fact, a revolutive action* of which nature gives no other example and which we find it difficult to consider as such.' Ampère had other reasons to dislike Faraday's

[59] Ampère to Bredin, 3 December 1821, *CA* 2: 576; Ampère 1821a: 329–33; 1821b, 1822a. Cf. Blondel 1982: 109–16.

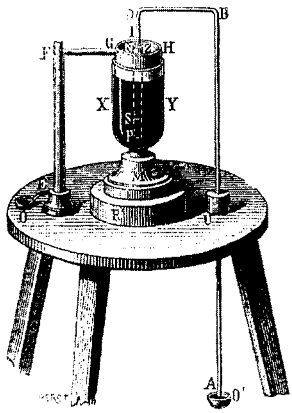

FIG. 1.12. Apparatus for the rotation of a magnet (NS) around its axis (Ampère 1822b). The current enters the magnet through the tip of the vertical wire DI and leaves it through the mercury bath XY. The magnet floats vertically in the bath thanks to the loading SP.

primitive revolutions. They did not provide a sufficient basis for calculation, they involved heterogenous entities (pole and current), and they contradicted the principle of the equality of action and reaction by having a net torque act on the pole–current system. In short, they betrayed every principle of French Newtonian physics.[60]

The lack of understanding was reciprocal. 'I regret that my deficiency in mathematical theory,' Faraday wrote to Ampère, 'makes me dull in comprehending these subjects. I am naturally sceptical in the matter of theories and therefore you must not be angry with me for not admitting the one which you have advanced immediately.' Such statements should not be read as an admission of inferiority. In several occasions Faraday appeared to be proud of his ignorance of mathematics. Upon his later discovery of electromagnetic induction he commented: 'It is quite comfortable to find that experiment needs not quail before mathematics but is quite competent to rival it in discovery.'[61]

Although Ampère misrepresented and rejected Faraday's theoretical ideas, he did not neglect the theoretical consequences of the new fact of continuous rotations. Most strikingly, the rotations offered an apparent exception to the impossibility of perpetual motion. Ampère explained that the continuous supply of living force to the rotating wire came from the electric current. Having thus emphasized the dynamical nature of voltaic electricity, he decided to call 'électro-dynamique' the new science of the interaction of currents. Most important, he used the argument to banish any theory of the temporary magnetism of wires. An arrangement of magnets, no

[60] Ampère to Bredin, 3 December 1821, *CA* 2: 576 (quotation); Ampère 1821b: 370, 374 (quotation); Ampère to A. de la Rive, 14 October 1822, *CA* 2: 605 (against primitive revolutions).

[61] Faraday to Ampère, 2 February 1821, *CMF* 1; Faraday to Phillips, 29 November 1831, *CMF* 1.

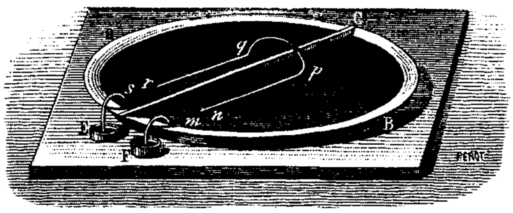

FIG. 1.13. Device for showing the mutual repulsion of the parts of a rectilinear current (Ampère 1822c). The currents in the segments qr and np of the floating wire repel the currents rs and mn in the mercury bath.

matter how complex, could not yield the continuous rotations, since magnetic actions were known to obey the theorem of living forces.[62]

The rotation experiments also played an important role in the determination of the force formula for two current elements. While experimenting on his own rotation devices, Ampère noted that the phenomenon disappeared whenever both wire ends were on the axis of the magnet. Yet calculations with the simple formula (1.1) indicated a positive result in this case. Ampère then returned to the more general formula (1.2), and sought the value of k for which the rotation did not occur. In June 1822 he found $k = -1/2$, which gives

$$\mathrm{d}^2 f = ii' \frac{\mathrm{d}s\mathrm{d}s'}{r^2}\left(\sin\alpha \sin\beta \cos\gamma - \frac{1}{2}\cos\alpha\cos\beta\right) \tag{1.3}$$

for the force d^2f acting between the elements $\mathrm{d}s$ and $\mathrm{d}s'$ of the currents i and i' (an attraction being reckoned positively). This formula implies that, contrary to Ampère's early guess, two current elements on the same straight line and with the same orientation repel each other. Ampère soon confirmed this effect in Geneva, with a device which is now familiar to every student of electrodynamics (Fig. 1.13).[63]

The analytical calculations performed in this context are of special interest. Ampère replaced the magnet with a simple circular current, and required that the total torque impressed by an element of this current on any current starting and ending on the axis of the circle should be zero.[64] For Ampère the mathematician, this meant that the torque impressed on any element of current had to be an exact differential with respect to the distance of this element from the axis. With this prop-

[62] Ampère 1822a: 66; Ampère 1822b, 1826b: 97 for 'électro-dynamique'; Ampère 1822a: 65–6; 1826b: 96n.

[63] Ampère 1822c: 235, and 1822d: 418; Ampère [1822e]: 331, and 1826b: 28. Cf. Blondel 1982: 127–8, 132–3; Hofmann 1995: 293–308.

[64] The latter condition does not rigorously result from the similar condition with the whole circular current. Presumably for this reason, Ampère later preferred another equilibrium case. Cf. Blondel 1982: 127–8.

erty in mind, Ampère transformed his trigonometric force formula into another that involved derivatives of the mutual distance of the two elements with respect to the curvilinear abcissae s and s' of the two linear currents to which they belonged (see Appendix 1):

$$d^2 f = -ii' \frac{ds ds'}{r^2} \left(r \frac{\partial^2 r}{\partial s \partial s'} - \frac{1}{2} \frac{\partial r}{\partial s} \frac{\partial r}{\partial s'} \right). \tag{1.4}$$

Although it was born from the consideration of a special class of failed experiments, this formula had a prosperous future.[65]

The new technique proved highly adequate in the important case of closed circuits. Ampère thus proved that the force impressed on a current element by a closed circuit was perpendicular to the element. A few months later, a former student of his harvested other essential results. With a Polytechnician's skills, Félix Savary integrated the force formula over a circular loop of current, and then over a dense pile of such currents—which Ampère later called solenoid, after the Greek σωλεν for canal. When the radius of the circles was much smaller than the length of the canal, the solenoid behaved like two magnetic poles located at its extremities. The force between an extremity and another current obeyed the Biot–Savart law, and the force between two extremities satisfied Coulomb's law (see Appendix 1). Ampère congratulated Savary for having reduced the three basic actions of magnetism under the same law of his, thus proving the validity of his conception of magnets.[66]

1.4.2 The Newton of electricity

By that time, early 1823, Ampère's electrodynamics had reached maturity. With the perfected Ampère law, the Amperean currents, and proper analytical tools, one could calculate every known magnetic or electromagnetic effect. However, a systematic account of the theory was still wanting. This Ampère gave in 1826 with his masterful 'Mémoire sur la théorie mathématique des phénomènes électro-dynamiques, uniquement déduite de l'expérience.'[67]

Imitating the rhetorics of Newton's *Principia* or Fourier's *Théorie Analytique de la Chaleur*, Ampère presented his results as the plain expression of experimental truths: 'I have solely consulted experiment to establish the laws of these phenomena, and I have deduced the only formula that can represent the forces to which they are due.' Later commentators have had no difficulty detecting a few unwarranted hypotheses in Ampère's theory, for example the central character of elementary forces, the absence of elementary torque, and the Amperean currents. There is no reason, however, to doubt Ampère's sincerity. As was mentioned, the concept of *physical* current elements, on which the character of the action between current elements depended, seemed to be materialized in his apparatus. The currents in magnets

[65] Cf. Grattan-Guinness 1990, Vol. 2: 930–33. I use Kirchhoff's notation for the partial differentials.

[66] Ampère 1822d: 419–20; Savary 1823. Cf. Grattan-Guinness 1990, Vol. 2: 934–9. Savary starts with a closed solenoid, motivated by an unpublished experiment of Gay Lussac and Welter.

[67] Ampère 1826b.

were not a hypothesis, as far as they were the only consistent way to unify magnetism and electromagnetism: 'The proofs on which I base [my theory] mostly result from the fact that they reduce to a single principle three sorts of actions which all phenomena prove to depend on a common cause, and which cannot be reduced in a different manner.'[68]

Most important, Ampère's formula for the force between two current elements did not depend on any assumption regarding the nature of the electric current and connected mechanisms: 'Whatever be the physical cause to which we may wish to relate the phenomena produced by this action, the formula obtained will always remain the expression of facts.' As we shall see, this turned out to be largely true, since Ampère's formula (at least its consequences for closed currents) remained an essential basis for the construction of all later theories of electrodynamics. Ampère again compared himself to Fourier, whose equations for heat propagation had survived Fresnel's wave theory of light and heat. Extending the parallel, Ampère did not exclude the search for physical causes. He himself speculated on various mechanisms for the production of electrodynamic forces, as will be seen in a moment. But he required a clean separation between laws and causes.[69]

For the determination of the force between two current elements, Ampère offered a polished version of the null method, which was 'more direct, simpler, and susceptible of great precision.' The first equilibrium case concerned the lack of action of two contiguous opposite currents. The second established the equivalence of rectilinear and sinuous currents, in the manner of 1821. The third replaced the no-rotation devices of 1822 and proved that the force acting from a closed circuit on a current element was perpendicular to the element. The fourth established the scale invariance of the electrodynamic action.[70]

Ampère assumed, as self-evident, that the action between two current elements resulted in equal and opposed forces directed along the line joining the elements and decreasing as the nth power of their distance. Then he used the first case of equilibrium to prove that the force between two orthogonal elements vanished. The second case, as before, determined the angular dependence of the force, up to the constant k. The third and fourth cases gave two relations between k and n, from which $k=-1/2$ and $n=2$ resulted. The complete expression of the force still involved obvious factors: the lengths of the elements and the intensities of the currents. In Ampère's mind the latter factor constituted a quantitative *definition* of the intensity of a current, including a definite current unit as soon as the unit of force was defined.[71]

The experiments and reasonings of the null method had an air of great systematism. A closer look at them, however, reveals serious flaws. Ampère did not quantify the precision of his apparatus, as if measuring a zero quantity required zero efforts at error analysis. Even worse, his third case of equilibrium was utterly

[68] Ampère 1826b: 2, 83–4. [69] Ampère 1826b: 4. [70] Ampère 1826b: 6, 9–18.
[71] Ampère 1826b: 18–44, and 18–19 for the definition of intensity.

unstable and hardly observable, and the apparatus for the fourth one was never built, on Ampère's own admission.[72] Could it be that Ampère's law rested on paper evidence? Certainly not: Ampère knew that the equivalence between magnets and systems of currents completely determined the values of n and k.[73] For the sake of a reductionist rhetoric, however, he preferred an ideal justification of his formula that would not depend on the complicated physics of magnets.

In the bulk of his memoir, Ampère developed the consequences of his formula for closed currents, Savary's solenoids, and magnets. The diversity of his mathematical techniques must be emphasized. In some reasonings he used the original expression of the force in terms of trigonometric lines, but in most he started with the 'very simple'

$$\mathrm{d}^2 f = -ii'\mathrm{d}s\mathrm{d}s'\frac{2}{\sqrt{r}}\frac{\partial^2\sqrt{r}}{\partial s\partial s'}. \tag{1.5}$$

Occasionally, he turned to Cartesian coordinates. For example, he wrote the force (X, Y, Z) acting from a closed circuit on a current element $(\mathrm{d}x, \mathrm{d}y, \mathrm{d}z)$ in the form

$$X = \frac{1}{2}ii'(C\mathrm{d}y - B\mathrm{d}z), \quad etc. \tag{1.6}$$

with

$$A = \int\frac{(y'-y)\mathrm{d}z' - (z'-z)\mathrm{d}y'}{r^3}, \quad etc. \tag{1.7}$$

where (x', y', z') are the coordinates of the points of the closed circuit (see Appendix 1). This expression exhibits the perpendicularity of the force and the current element. Moreover, it shows that the force is perpendicular to the direction of the vector (A, B, C), which Ampère called the 'directrice' since it depended only on external circumstances. Modern readers should resist the temptation to identify the *directrice* with a magnetic field concept: Ampère considered only the direction, and he did not include the intensity i' in the vector (A, B, C).[74]

As a special case of a closed current, Ampère considered a single infinitesimal loop of current, and showed that it was equivalent to a magnetic dipole. A finite closed current, he went on, could be replaced by a net of infinitesimal current loops

[72] Ampère 1826b, 1st edn: 205 (third case); 2nd edn: 151 (fourth case). In the edition for the Mémoires de l'Académie Royale des Sciences (1827), Ampère omitted the criticism of the third case of equilibrium. Cf. Blondel 1982: 147–8.

[73] Cf., e.g., the remarks in Ampère 1826b: 17, 151, indicating that the properties of the action between current and magnets can be used instead of the fourth case of equilibrium.

[74] Ampère 1826b: 30–1. Cf. Grattan-Guinness 1991; Hofmann 1995: 341–3. Up to a normalization factor, the formulas correspond to the modern $\mathbf{f} = i\mathrm{d}\mathbf{l} \times \mathbf{B}$, with $\mathbf{B} = \int i'\mathrm{d}\mathbf{l}' \times \mathbf{r}/r^3$.

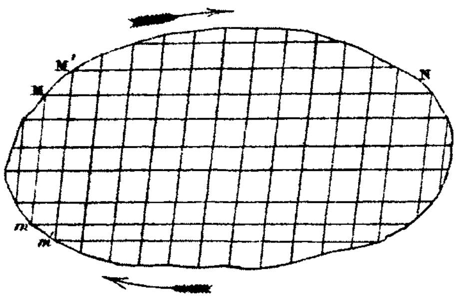

FIG. 1.14. Net of current (Ampère 1826b: plate 1). The same current runs clockwise around each little square, so that the net current in the common side of two square vanishes.

(Fig. 1.14), and was therefore mathematically equivalent to a double sheet of boreal and austral fluid. The ingenious equivalence played little role in Ampère's deductions, save for a proof that the continuous rotations were impossible for closed rigid circuits. Yet it could be very helpful to any one who, unlike Ampère, wished to derive the law of electrodynamics from those of magnetism.[75]

Toward the end of his memoir, Ampère relaxed his severe attitude and indulged in speculations on the cause and nature of electric motions. In his previous researches he had repeatedly tried to understand electrodynamic forces in terms of a propagated action in a medium. In his youth he condemned 'the supposition of an action between bodies that do not touch each other.' In the early 1820s, the success of Fresnel's optical ether revived his desire to reduce all physics to the local motions of a medium. When he discovered the equivalence of rectilinear and sinuous current, he imagined a corresponding superposition of ether motions. Later, the equivalence between a closed circuit and a net of infinitesimal current loops suggested to him a rotary motion in the medium. In each case, the fact preceded the intuition, and Ampère remained very discreet about his ether.[76]

Ampère was more open about his conception of the electric current. In 1821, he gave up Volta's idea of an electric motion of which the substratum of the conductor was the only obstacle. He adopted instead Oersted's idea of a series of compositions and decompositions of the two electricities starting in the battery and propagating along the conductor. In lengthy speculations, he combined this view with the atomistic conception of matter to explain contact tension and electrolysis. More succinctly, he imagined an ether made of the neutral fluid resulting from the combination of negative and positive electricity.[77]

In the memoir of 1826 Ampère expounded his view of the electric current, and mentioned the related conception of the ether. He briefly suggested a propagation of

[75] Ampère 1826b: 41, 101, 145–6; Ampère 1826a. Cf. Blondel 1982: 150–3; Grattan-Guinness 1990, Vol. 2: 956–9. In Chapter 2 it will be shown how Franz Neumann exploited the equivalence.

[76] Ampère [1801]: 175; 1820a: 257; 1826a: 47. Cf. Blondel 1982, 88–9, 152–3; also Caneva 1980.

[77] Ampère 1821c, *MRP* 2: 216 (Oersted's idea); 1822a, *MRP* 2: 249 (ether); [1824a], 1824b (electrochemistry). Cf. Blondel 1982: 155–7 (current), 161–5 (ether).

electromagnetic actions through this ether, but favored a more conservative approach in which Coulomb's electrostatic law remained basic. The idea was to take the average of the Coulomb forces between the separated fluids in the interacting currents. Since the separation was a temporary, spatially directed process, the angular dependence of the net forces could perhaps emerge in this manner.[78]

In sum, Ampère's influential memoir of 1826 was not just the reunion of the equilibrium cases, the Ampère formula, and the Amperean currents in magnets. It also involved a store of mathematical techniques from which successors could borrow, and it prefigured two ways of deepening our understanding of electrodynamic forces: by reducing them to motions in the ether or by summing the direct actions of the electric fluids running in conductors.[79]

The magnificent architecture of the memoir rested on a fictitious three-stage history. In the first stage, fundamental experiments established general properties of electrodynamic forces. In the second, a general force formula was inferred from these properties. In the third, all known phenomena of electrodynamics and magnetism were deduced from the force law and the assumption of Amperean currents. This architecture helped clarify the subject and convince Ampère's readers. At the same time, it obscured the dynamical interplay of experiment, mathematical techniques, and theoretical ideas in the actual genesis of electrodynamics.

Oersted's new effect, Newtonian analogy, and the principle of unity were the sources of Ampère's initial theoretical convictions. Then Ampère conceived, ordered, and used apparatus intended to support these convictions. The infinitesimal analysis of the theory conditioned the structure of the apparatus. Reciprocally, this structure suggested the notion of a physical current element as a separable entity with regard to the principles of mechanics. In general, the experiments confirmed the original intuitions. However, the few failed experiments played a crucial role. They removed previous indeterminations of the theory, they redirected Ampère toward the null method, and they prompted the development of new mathematical techniques. In turn, these techniques permitted a confirmation of the more qualitative components of Ampère's theory, and suggested more fundamental explanations of electrodynamic forces.

This complex history and Ampère's simple reconstruction of electrodynamics share a common trait: the mathematics is rigorous and adaptable, while the experiments lack precision and flexibility. This asymmetry, later regarded as a basic defect of the otherwise impressive French physics, has a natural explanation: the experiments were intended to found the theory at the simplest level of analysis, for which effects are small and geometrical configurations highly constrained. There were two obvious ways of avoiding the difficulty: to deny the control of mathematical theory over experiment, as Faraday did, or to relocate the control at the level of more complex, but still computable systems, as Weber later did.

[78] Ampère 1826b: 87, 97–9.
[79] Faraday's field conception is akin to the first approach, Weber's theory to the second.

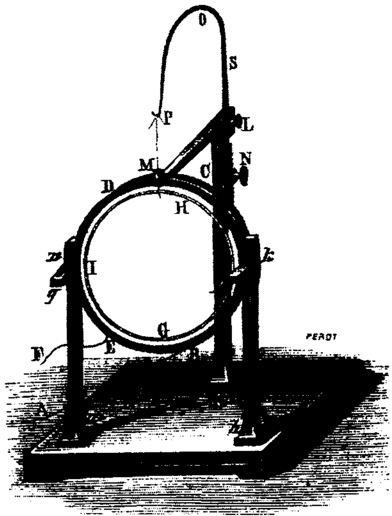

FIG. 1.15. Device for Ampère's induction experiment (Ampère 1821c: 448).

1.5 Electromagnetic induction

1.5.1 First tries

No mention has yet been made of a couple of Ampère's experiments that had nothing to do with the mathematics of current elements. As the hypothesis of molecular currents in magnets played a central role in his theory, Ampère wanted to determine whether these currents preexisted in unmagnetized iron or were created during the magnetization process. For this purpose, in July 1821 he imagined the device of Fig. 1.15, in which the copper ring HIG hangs within the fixed coil BCDE. He placed a magnet on the sticks nq and pk, and fed the coil with a battery. The ring did not move, and therefore did not seem to be the seat of induced currents. Ampère inferred that randomly oriented molecular currents existed in unmagnetized iron, and explained magnetization by an orientation of these currents.[80]

In September 1822 he repeated the experiment in Geneva with a powerful magnet and this time obtained 'alternatively an attraction and a repulsion of the ring.' With this positive result, the experiment could no longer serve to support the existence of molecular currents in iron. Nor was it related to the fundamental law of electrodynamics. Hence Ampère had no theoretical motivation to pursue the subject. He did not even specify whether the effect was permanent or transient, left its direction undetermined, and abandoned the publication to Auguste de la Rive.[81]

[80] Ampère 1821b: 377; 1821c: 448. Cf. Blondel 1982: 118–19; Hofmann 1995: 310–15.
[81] Ampère [1822e]: 333–4; A. de la Rive 1822: 48; G. de la Rive to Faraday, 24 September 1822, *CMF* 1: 291. Cf. Ross 1965; Williams 1986; Hofmann 1987b; Romo and Doncel 1994: 299. For a modern repetition and interpretation of Ampère's experiment, cf. Mendoza 1985.

For Faraday, the induction of currents was not an indifferent matter. If, he might have reasoned, the current-carrying state of a conductor implied magnetic power, the reciprocal effect was likely to exist: magnetic power had to induce electric currents in conductors. In November 1825 he investigated the case of two linear conductors. The first conductor being connected to a battery, its magnetic power could perhaps induce a current in the second conductor. Faraday tried three geometrical configurations: a pair of parallel wires, a straight inducing wire within a helicoidal collecting wire, and a helicoidal inducing wire around a straight collecting wire. In each case his galvanometer gave no deviation.[82]

Perhaps the effect was just too small to be detected, Faraday must have thought. In August 1831, probably impressed by Joseph Henry's and Gerritt Moll's experiments with electromagnets, Faraday imagined a new device that exploited the multiplying effect of coils and the concentrating effect of iron. He had long copper wires coiled around two opposite sides of an iron ring, with proper insulation of the turns. The iron conducted the strong magnetic power of the primary coil to the secondary coil. Three feet of wires connected the latter coil to a primitive galvanometer made of a suspended magnetic needle and a parallel wire. Worth noting is Faraday's distrust of ready-made meters in his search for new effects.[83]

Faraday connected the primary coil to the battery and reported: 'Immediately a sensible effect on needle. It oscillated and settled at last in original position. On *breaking* connection [. . .] with battery again a disturbance of the needle' (Faraday's emphasis). The effect was therefore clear but transient. Faraday spoke of a 'wave of electricity,' meaning a current short and intense as a breaker on the shore.[84]

Faraday did not expect a transient phenomenon. Available analogies, especially that with electrostatic induction, suggested a permanent induced current. Fortunately, he did not need to look for a transient effect in order to see the induced current, for a simple reason: his galvanometric needle had little damping and could perform 'a few oscillations' before it returned to equilibrium. He could therefore observe the perturbation of the needle even if he looked at it well after he had closed the primary circuit. Over previous attempts at detecting induced currents, Faraday's crucial improvement was the amplification of the effect by the coils and the iron core. With the primitive device of November 1825, his galvanometer was too insensitive to show the least disturbance, even transient.[85]

[82] *FD* 1: 279 (28 November 1825). In April 1828 (*FD* 1: 310) Faraday explored a case of induction similar to Ampère's: he placed a magnet within a delicately suspended copper ring, and tried and failed to move the ring by approaching the poles of another, powerful magnet. For his motivations to expect induced currents, cf. *FER* 1: 1–2.

[83] *FD* 1: 367, ##1–5 (29 August 1831). On Henry's and Moll's experiments, cf. Moll to Faraday, 7, 9, 10 June 1831, *CMF* 1.

[84] *FD* 1: 367, #3; *FD* 1: 369, #14 ('wave of electricity'). A careful examination of occurrences of the expression 'wave of electricity' in the diary shows that Faraday did not mean a waving motion (In French one could say he meant *vague*, not *onde*).

[85] *FD* 1: #7. Several commentators have attributed Faraday's success to his supposed anticipation that the effect should be transient, and speculated on various reasons for such an anticipation.

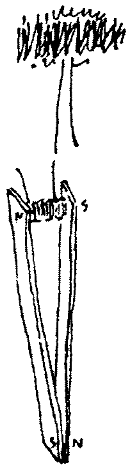

FIG. 1.16. Device for magneto-electric induction (*FD* 1: 372).

1.5.2 From weed to fish

Another key to Faraday's success was his willingness to investigate what looked at first glance like a parasitic phenomenon and thus to transform a crude observation into a full-blown discovery. In the day of the first observation he improved the detection by replacing the straight wire above the compass needle with a flat spiral, and showed that an iron cylinder could be used instead of the iron ring (though less efficiently). He failed, however, to produce two expected effects of the induced current: spark and electrolysis. Three weeks later, on the verge of starting a new series of experiments, he wrote to his friend Richard Phillips: 'I am busy again on electromagnetism and think I have got hold of a good thing but can't say; it may be a weed instead of a fish that after all my labor I may at last pull up.'[86]

The following day Faraday tried induction from coil to coil without iron core, and also induction from moving magnet to spiral. This failed. He then returned to his earlier iron cylinder, made the surrounding wire into a single helix connected to the galvanometric device, and arranged two bar magnets and the iron cylinder in a triangular magnetic circuit (Fig. 1.16). Whenever the magnetic contact was made or broken, the magnetic needle moved. Faraday concluded: 'Distinct conversion of Magnetism into Electricity.'[87]

In October, with a refreshed battery and improved coils, Faraday obtained direct induction from coil to coil, though very weakly. He also managed to produce a spark with the original iron ring and coils. Lastly, he obtained a galvanometric deflection by thusting a bar magnet into a hollow coil and recorded: 'A wave of Electricity was

[86] *FD* 1: #6, #7, #18, #15, #11; Faraday to Phillips, 23 September 1831, *CMF* 1.
[87] *FD* 1: #21, ##25–7, #33 (24 September 1831).

so produced from *mere approximation of a magnet* and not from its formation *in situ.*'[88]

By that time Faraday could realize all cases of electromagnetic induction, except for that given by the relative motion of two circuits, which he obtained at the end of the year.[89] His explorative strategy involved amplifications of the effects with improved coils, batteries, and detectors; and simple mutations of the devices, suggested by the equivalence of currents and magnets with respect to magnetic power. According to this equivalence, the iron in the first induction experiment could not be essential, since it merely channelled and amplified the magnetic power of the primary coil. Conversely, the primary coil could be replaced with a magnet, as Faraday did in the triangular device of Fig. 1.16. Lastly, as the formation and the approximation of a magnet brougth the same change of magnetic power, Faraday conceived induction by a moving magnet.

1.5.3 The electro-tonic state

This series of observations did not depend on a particular view of the induction process. Yet Faraday had one very early on. In his mind, the transient character of the induced current was too surprising to be left unexplained. A transient induced current was conceivable when *closing* the primary circuit: intuitively, the sudden increase of the primary current could be more efficient than a steady current. But the occurence of a transient current of comparable intensity when *breaking* the primary circuit puzzled Faraday. How could a dying current have inductive effects when a steady current had none?

In his first report of the latter effect Faraday underlined the word 'breaking.' In the same day he wrote: 'Recurrence on breaking the connection shews an equilibrium somewhere that must be able of being rendered more distinct.' In this view, which Faraday later exposed at the Royal Society, the conductor assumes a 'tonic' state during the initial transient current and maintains it as long as the primary current exists. When the primary circuit is broken, this state relaxes and an inverse transient current results. In Faraday's own words:

> Whilst the wire is subject to either volta-electric or magneto-electric induction, it appears to be in a peculiar state; for it resists the formation of an electrical current in it, whereas, if in its common condition, such a current would be produced; and when left uninfluenced it has the power of originating a current, a power which the wire does not possess under common circumstances. This electrical condition of matter has not hitherto been recognised, but it probably exerts a very important influence in many if not most of the phenomena produced by currents of electricity. [. . .] I have, after advising with several learned friends, ventured to designate it as the *electro-tonic* state.

In brief, the new state had three essential virtues: it explained the current induced during the suppression of the inducing device, it extended the idea of states induced

[88] *FD* 1: ##36–9, #46, #57. [89] *FD* 1: ##250–1 (26 December 1831).

by states, and it offered a new possibility for developing a picture of the electic current and its effects.[90]

If, Faraday reasoned, the new state of matter truly existed, independent evidence needed to be brought. For example, Faraday sought magnetic actions from conductors in this state, or variations of their conducting power. All attempts were negative. Besides, Faraday soon developed another description of the induction phenomenon, as will be seen in a moment. To his published account of the electrotonic state, he added a footnote mentioning that the notion had become superfluous. He was reluctant to give press to a speculation, but retained his faith in the tonic state for the rest of his life. In 1835 he wrote to Whewell:

> I have given up this electrotonic state for the times as an experimental result (remember, my researches are *experimental*) because I could find no fact to prove it but I cling to it in fancy or hypothesis from general impressions produced by the whole series of researches.

Among Faraday's later tentative proofs of the tonic state we find self-induction in 1834, a quickly discarded difference between the inducing powers of voltaic and magneto-electric currents in 1840, and diamagnetism in 1845. This obstinacy reveals Faraday's commitment to the gradation of cause and effect: he could not admit that an effect would be larger than its direct cause. The interrupted current in a given circuit could not be the cause of a larger current in another circuit without 'a link in the chain of effects, a wheel in the physical mechanism of the action, as yet unrecognized.'[91]

1.5.4 Cut magnetic curves

Nonetheless, the electro-tonic state played little role in Faraday's early experiments on electromagnetic induction. After proving voltaic and magnetic induction, he rather explored the link he suspected between Arago's effect and the new phenomenon. In 1822, while measuring the magnetic force of the Earth near Greenwich, Arago had noticed the damping effect of non-magnetic metals placed in the vicinity of the compass needle. Two years later, he examined and published the reverse effect: the slowing down of a rotating copper disk by a nearby magnet. This new kind of magnetic action attracted much attention, and even triggered a priority quarrel between Arago and David Brewster. Notwithstanding Arago's initial reserve, several assumptions were made about the cause of the new effect, the most popular being a temporary magnetization of the rotating disk.[92]

In the very first day of his induction experiments, Faraday queried: 'May not these transient effects be connected with causes of difference between power of metals in rest and in motion in Arago's expts.?' Faraday had in mind that the force between

[90] *FD* 1: #3, #8; *FER* 1: series 1 (November 1831): #60.

[91] *FER* 1: 16n (footnote); Faraday to Whewell, 19 September 1835, *CMF* 1; *FER* 1, series 9 (December 1834): #1114 (self-induction and quote). On the difference between voltaic and magneto-electric currents, cf. *FD* 4: ##6081–6187 (August 1840). On tonic state and magnetic polarization, cf. *FER* 1: #1729.

[92] Arago 1825. Cf. Arago 1826; Williams 1965: 170–172; Romo and Doncel 1994: 302–303.

the magnet and the rotating plate could be due to currents induced in the plate. Two months later, he proceeded to check this assumption with a copper disk rotating between the jaws of the 'great magnet of the Royal Society.' He placed two collecting blades at two points of the disk, connected them to a galvanometer, and observed a distinct deviation. A new electric machine was born.[93]

In the absence of any precise theory, Faraday assumed that the configuration of the induced currents would imitate the configuration of the inducing current. Accordingly, in his early experiments on Volta-electric induction he judged that a growing current induced currents in the same direction.[94] It took him no less than three months to become aware of this sign mistake. In the rotating-disk experiment, he expected a semi-vortex of currents in the part of the disk situated between the poles of the magnet, in conformity with the configuration of the inducing Amperean currents. Fortunately, this prejudice did not prevent further exploration. Varying the position of the sliding contacts, Faraday soon found that the assumed vortex did not exist. The currents were induced radially, that is, in a direction perpendicular to the motion.[95]

Having in mind a more direct proof of this law, Faraday passed rectangular blades and wires between the jaws of an electromagnet, and by the pole of a cylinder magnet. He concluded: 'The current of electricity which is excited in a metal when moving in the neighbourhood of a magnet depends for its direction altogether upon the relation of the metal to the resultant of magnetic action, or to the magnetic curves.' Not knowing the vector product, Faraday found this direction 'rather difficult to express,' and took three paragraphs to explain it with diagrams, a knife-blade, and a wood-and-threads model. The basic idea was to consider the way the wire cuts the 'magnetic curves,' defined as 'the lines of magnetic forces [. . .] which would be depicted by iron filings or those to which a very small magnetic needle would form a tangent.'[96]

In later experiments regarding the induction under the magnetic action of the Earth and the induction by a rotating cylinder magnet, Faraday proved that the mere cutting of magnetic curves, without change of magnetic intensity, was sufficient to induce a current. He also compared (by opposition) induction in different metals, and found that 'the tendency to generate a current' was the same for all metals. In early 1832 he condensed his results in a single law: 'If a terminated wire moves so as to cut a magnetic curve, a power is called into action which tends to urge an electric current through it.'[97]

[93] *FD* 1: #17; *FD* 1: #85, ##99–109 (28 October 1831).

[94] *FD* 1: #9. Faraday gave the correct direction of the induced currents on 8 December, *FD* 1: 190–208. The manuscript of series 1 read before the Royal Society on 15 December contained the sign mistake. Cf. Romo and Doncel 1994; Doncel 1996.

[95] *FD* 1: #77 (24 October 1831) for the semi-vortex; *FD* 1: ##110–19 (28 October 1831). Cf. Steinle 1994; Romo and Doncel 1994; Doncel 1996.

[96] *FD* 1: ##130–42 (4 November 1831), ##194–213 (8–9 December); *FER* 1, series 1 (November 1831): 33, ##114–16, #114n (magnetic curves).

[97] *FD* 1: ##232–9 (21 December) for terrestrial induction, ##255–7 (26 December) for the rotating magnet; *FD* 1: ##283–87 (26 December) and *FER* 1: series 2 (January 1832): 62 for different metals. Originally, Faraday regarded the equality of the induction in different metals as contradicting the dependency of the Arago effect on the metal. It took him several weeks and Christie's help to understand the

Originally, this statement concerned only the induction produced by the relative motion of a magnet and a conductor. Faraday later included the case of two conductors in relative motion, and finally that of induction by a varying current. In the latter case magnetic curves must be seen as developing during the growth of the primary current and thereby cutting the conductor of the secondary circuit. With this ultimate extension, the law of the cut lines of force became complete and self-sufficient: 'By rendering a perfect reason for the effect produced [the law seems to] take away any for supposing that peculiar condition, which I ventured to call the electro-tonic state.'[98]

Magnetic curves had been widely used in Faraday's circle, including Davy, Sturgeon, and Moll, to represent the magnetic power of magnets, electric currents, and electromagnets. With this geometrical representation of magnetic power, Faraday could bridge his two essential discoveries: electromagnetic rotation and induction. He perceived a basic axis–loop duality that applied to both phenomena: 'The power of inducing electric currents is circumferentially exerted by a magnetic resultant or axis of power, just as circumferential magnetism is dependent upon and is exhibited by an electric current.'[99]

1.5.5 The ambiguities of success

Faraday's extraordinary discovery prompted high excitement among his peers. Without the author's permission, Jean Hachette read to the French Academy a private letter in which Faraday summarized his main findings. Through a French magazine the news reached two distinguished Italian physicists, Leodolfo Nobili and V. Antinori, who immediately experimented on the subject and published their findings. The false rumor of their priority soon circulated, even though they had included the text of Faraday's letter in their paper. Furthermore, an article in *Le lycée* dwelt on French anticipations of Faraday's discovery. Faraday had no difficulty straightening the facts.[100]

However, he hurt Ampère's feelings by attributing to him 'the erroneous result' that induced currents were in the same direction as the inducing currents. In a long, tormented letter to Faraday, Ampère proved that he had never made any pronouncement on the direction of the electric current. Implicitly, he regarded his experiment with the suspended copper ring as an anticipation of Faraday's discovery. Yet this observation had little historical importance, whether or not it had something to do with electromagnetic induction. Where Ampère had done a single,

role of the conducting power of the metal. In terms of Ohm's law, which Faraday did not know, the induced electromotive force does not depend on the metal, but the current does. Cf. Steinle 1996.

98 *FER* 1, series 2 (January 1832): #232, #238, #231 (quotation). Cf. Steinle 1996.

99 *FER* 1, series 1 (November 1831): 118. On previous uses of magnetic curves, cf. Simpson 1968: 80–86; Heilbron 1981: 202; Gooding 1985, 1990: Ch. 4.

100 Faraday to Hachette (lost), extract pub. in *ACP* 48 (1831): 402, and in *Le temps*, 28 December 1831 (read by Nobili); Nobili and Antinori 1831 (dated 31 January 1832, but pub. in Vol. dated November 1831); Rumor in *Literary gazette* (1832): 185; *Le lycée* 36, 1 January 1832; *FER* 1, series 1: 40–41.

doubtful experiment, Faraday offered a long systematic series of researches and gradually constructed the fact of electromagnetic induction.[101]

The key to Faraday's success may be seen in his ability at methodic exploration. He gave to his devices the optimal flexibility, and was attentive to the parts that could be modified according to the opportunities offered by his laboratory. He thus constructed chains or trees of experiments, feeling his way 'by facts closely placed together.' He kept in memory a large stock of previous experiments, to be explained by the new facts (in the case of the Arago effect) or instead to be used in the explanation of the new facts (in the case of the motion of magnetic poles). He settled his views and his experimental activity when a simple, coherent network of actual and virtual experiments was reached.[102]

Faraday avoided two ways of blocking the exploratory function of experiments. First, he did not divert his energies into developing practical applications. He was 'rather desirous of discovering new facts and new relations than of exalting the force of those already obtained.' He was satisfied as soon as the new effects were clear and easily reproduced (eventually in the classroom), and left to others the conception of efficient electric motors and dynamos. Second, Faraday did not let theory invade his researches. Although theoretical prejudices, such as the existence of induced currents, the electro-tonic state, or the vortices in Arago's disk, played a role in orienting his research, they were easily correctible. Faraday was proud and eager of this flexibility, and denounced the sterility of closed mathematical theories:[103]

I do not remember that Math. have *predicted* much. Perhaps in Ampère's theory one or at most two independent facts. I am doubtful of two. Facts have preceded the math. or where they have not the facts have remained unsuspected though the calculations were ready as in electromagnetic rotation and magneto-electricity generally; and sometimes when the fact was present as in Arago's phenomenon the calculations were insufficient to illustrate its true nature until other facts came into help.

Only at the end of his experimental series on electromagnetic rotation and induction did Faraday offer a synthetic view of the explored field. We may call this view a theory because of its ability at ordering the complex. Yet it was very different from what Ampère (and Faraday) would have called a theory. It was not mathematical and it was not even quantitative.[104] It was an open scheme, in which the nature of the electric current remained undetermined. The central concept, that of magnetic power and its lines of action, had an ambiguous status. Was it a simple convenience of expression, or was it a physical entity? Faraday's operational definition of the magnetic curves suggests the first alternative. But his explanation of the attraction

[101] *FER* 1, series 1 (Nov 1831): #78n; Ampère to Faraday, 13 April 1833, *CMF* 2. Faraday was misled by the description of Ampère's experiment that he found in Demonferrand 1823 (cf. Romo and Doncel 1994: 301). He apologized in *FER* 1, series 3 (Jan 33): 107–9 (note of 29 April 1833).

[102] Faraday to Ampère, 3 September 22, *CMF* 1.

[103] *FER* 1, series 2 (January 1832): ##159; Faraday to Somerville, November 1833, *CMF* 2.

[104] The quantitative concept of the density of lines of force appeared only in 1851: *FER* 3, series 28 (October 1851): #3115, #3122.

between two currents in terms of the corresponding magnetic powers points to the second.[105]

Historians have tried in vain to eliminate the ambiguity. It may instead be seen as an essential characteristic of Faraday's investigations, from the beginning of his interest in electricity and magnetism to his latest works. On the one hand, he wished to keep his researches experimental and to secure a purely instrumental meaning of the lines of force, as a clear and efficient way to formulate the rules of electromagnetism. On the other, he strongly suspected that the magnetic curves and the tonic state were real 'links in the chain of effects' produced by magnets and electric currents. Accordingly, he expected that the magnetic and electric actions were 'progressive and required time,' as the propagation of sound and light already did. This speculation of March 1832 ended up in the safe of the Royal Society. Faraday spent twenty more years of exploration until he made it public.[106]

1.6 Conclusions

Ampère and Faraday co-founded the new science of electrodynamics. In harvesting experimental facts, their contributions were complementary: Ampère demonstrated the forces between two currents and proved the equivalence between magnets and distributions of current (not to be confused with the hypothesis of Amperean currents), while Faraday discovered continuous rotations and electromagnetic induction. However, their experimental and theoretical methods were so different that they could hardly exchange more than uninterpreted facts.

Ampère's physics was dominated by theory. Despite the inductive rhetoric of his chief memoir, he constructed his theory mostly from theoretical resources, including analogy, virtual history, and mathematical unification in neo-Newtonian style. Most of his experiments verified theoretical predictions, or decided between a preconceived range of possibilities. They were highly rigid in their construction, and reflected the mathematical structure of the theory.

In contrast, Faraday knew no mathematics and tried to minimize theoretical prejudice. He regarded the Newtonian notions of electric and magnetic fluids as unproven, and enhanced the exploratory function of experiment by systematically evolving his experimental devices. He did not aim at a closed mathematical theory, but instead maximized the mutual connections of his experimental actions

[105] When he introduced the idea of developing or contracting magnetic curves around a changing current (*FER* 1, series 2: #238), Faraday specified in parentheses that the magnetic curves were 'mere expressions for arranged magnetic forces.' In his experiments with the rotating cylinder magnet, he believed that the magnetic curves did not rotate with the magnet and concluded in a '*singular independence* of the magnetism and the bar in which it resides' (*FER* 1, series 2: #220). Williams (1965: 203–4) takes this to indicate that Faraday's lines of force had already become 'much more real.' However, the conception of the lines as a mere chart of magnetic forces implies their independence of the magnet's rotation. By emphasizing the 'singular' character of this independence, Faraday probably meant a contrast with the behavior of hypothetical magnetic fluids or Amperean currents, which would have had to rotate with the magnet.

[106] Faraday, sealed note of 12 March 1832, Royal Society, quoted in Williams 1965: 181.

and results. His agnosticism about the intimate nature of electricity and magnetism focused him on the *actions* of the various electrified or magnetic bodies rather than on their inner structure. His basic philosophical notion was that of 'power' (or 'force'), comprehending actual and virtual action. In his idiom, different powers induced different states of bodies (states of motion, or internal states). The most essential thing was the distribution of power. Sources (electrified body, magnet, current) were subordinated to the powers they developed.[107]

There is an essential harmony between Faraday's exploratory style of experimentation and his concept of power. A distribution of power may be seen as given by a set of virtual experiments. For instance, magnetic curves represent the motion that would be impressed on a magnetic pole at each point of space. By multiplying and varying experimental configurations, Faraday 'placed facts closely together,' which means that he connected actual effects to virtual experiments that determined the distribution of power. For example, he conceived the electromagnetic rotations by putting together on paper the virtual actions of the sides of a magnetic needle on a wire. Or he explained Ampère's attractions between two currents in terms of virtual rotations revealing the distribution of power around the currents. Hence, exploration could reveal or map powers. Or it could seek new sorts of states induced by a given power. Faraday was doing just that when he looked for electromagnetic induction. As the result did not fit the power-induces-state scheme, he imagined the electrotonic state.

Ampère and Faraday both strove for theoretical unity, but in a different way. The French philosopher imagined an internal structure of the various sources of action that would reduce them to one kind only. By reducing magnets to currents, he unified electrodynamics and magnetism; by reducing currents to flows of electricity, he hoped to unify electrostatics and electrodynamics; by identifying the ether with a neutral compound of the two electricities, he hoped to unify optics and electrodynamics. In contrast, Faraday achieved unity by identifying the powers emanating from various sources. In his view, magnetic, electromagnetic, and electrodynamic effects all derived from the interplay of magnetic powers. Strikingly, he adopted Ampère's statement that the side of a current was like the pole of a magnet. But where Ampère implied that magnets were made of currents, Faraday meant that the same power existed in both cases.

In its mature form Ampère's theory was successful, especially in France and in Germany. It was expressed in clear mathematical language, it relied on largely familiar Newtonian notions and techniques, and it seemed to rest on a sound empirical basis. In brief, anyone who understood it adopted it, save the more controversial Amperean currents. In contrast, Faraday's theory was completely ignored for many years. Not even the rule of the cut lines of force found favor with contemporary physicists. Faraday's first readers had difficulty with his unusual style, and missed

[107] For Faraday, 'force' and 'power' were roughly synonymous. In his later writings, 'force' is more common, and 'power' is used to indicate the kind of force, for example 'magnetic power' versus 'electric power.' Naturally, Faraday also used these two words in their most ordinary sense: for example, the 'force of this argument,' or the 'power of this electromagnet.'

the essential coherence of his material and conceptual practices. The first mathematical theories of electromagnetic induction, which form the subject of the next chapter, did not rest on Faraday's views. Their departure point was Ampère's mathematical theory together with Faraday's facts.

2

German precision

2.1 Introduction

Until the 1830s, most German physics consisted of an empirical, qualitative extension of major foreign discoveries such as Volta's pile or Oersted's effect. Favorite topics were the composition of the pile and the circumstances of the electromagnetic action. The most famous instrument then invented in Germany was Schweigger's and Poggendorff's 'multiplicator,' a coil that amplified the effect of the galvanic current on the magnetic needle. This device soon became an essential part of galvanometers—though not in the hands of its German inventors, who cared little about quantitative measurement.[1]

German physicists were instead attentive to interconnections between various parts of physics. Following Oersted's example, they sought direct relations between chemical reactions and magnetism, and also between heat and magnetism. The latter kind of investigation led Thomas Seebeck, a discreet *Naturphilosopher*, to the discovery of thermoelectricity in 1822. Another fruitful topic was the connection between galvanism (effects of Volta's battery) and (frictional) electricity. In 1801 the Berlin physicist Paul Erman observed electroscopic tension along a wet conductor connected to the poles of a voltaic battery. In contemporary parlance, the 'discharge of the battery' was incomplete. Erman showed that the tension between the poles was higher for poorer conductors, and that it was continuously distributed along the conductor. He believed these effects to be specific of wet conductors, and made them the basis of his personal theory of the battery.[2]

The relation between electroscopic force, current, and conducting power remained a murky subject until Georg Simon Ohm studied it in the mid-1820s with Ampère's concept of circuit and with new galvanometric techniques. Ohm used Seebeck's thermoelectric source (which has constant electromotive force), and a magnetic needle suspended to Coulomb's torsion balance to measure the current. Patiently varying the length and nature of the connecting wires, he obtained the law that bears his name. Erman's old observations now appeared to result from the competition between the internal resistance of the battery and the resistance of the external con-

[1] Schweigger 1821. Cf. Pfaff 1824; Caneva 1978; Jungnickel and McCormmach 1986, Vol. 1: 43–4.
[2] Seebeck 1822–1823; Erman 1801. Cf. Jungnickel and McCormmach 1986, Vol. 1: 43.

ductor. A few months later, Ohm published a general mathematical theory of electric conduction based on analogy with Fourier's theory of heat. Ohm postulated that the electric current density was proportional to the gradient of the electroscopic force, as the heat flow was proportional to the temperature gradient. With a wealth of Fourier series, he determined how the resistance of a finite, homogenous conductor depended on its shape.[3]

Ohm's highly mathematical, experimentally precise quantification of the galvanic circuit had the virtues of the best French physics. But it was alien to the dominant German style, and was therefore cooly received. Trust in Ohm's law only increased after Gustav Fechner's meticulous confirmation of it in 1831. Even then, Ohm and Fechner did not have the means to spread their French-inspired methods. Physics teaching in universities remained elementary, and no structure existed to train researchers in this field.[4]

More favorable institutional conditions only began to appear around 1830. Most decisive was the creation of 'physics seminars' in several German universities. The seminars familiarized students with physical apparatus and trained them to solve problems in mathematical physics. Two of the most important reformers of German physics, Franz Neumann and Wilhelm Weber, were seminar leaders at Königsberg and Göttingen respectively. With Friedrich Bessel and Carl Friedrich Gauss, they greatly improved German standards in experimental accuracy and in physico-mathematical theory. The present chapter is devoted to a comparative study of their methods and achievements in their favorite fields, electrodynamics and magnetism.[5]

2.2 Neumann's mathematical phenomenology

2.2.1 Eliminating the apparatus

Franz Neumann's first specialty was crystallography, which he learned under the Berlin mineralogist Christian Weiss. Following his mentor, Neumann used geometric methods based on crystalline homogeneity, symmetry, and goniometric measurements. According to the dictum *Hypotheses a naturae explicatione prohibendae*, he avoided the atomistic representation of crystals. His interest in their physical properties led him to read Fourier's *Théorie analytique de la chaleur*, which he found very congenial. Unlike Laplace's disciples, Fourier abstained from assumptions on the nature of heat and the structure of matter. Starting with a law for the radiant heat

[3] Ohm 1826a, 1826b (ref. to Erman), 1827 (theory). Cf. Schagrin 1963; McKnight 1967; Pourprix 1990; Jungnickel and McCormmach 1986, Vol. 1: 51–5. Presumably misled by the heat-flow analogy, Ohm identified the electroscopic force or tension with the charge density. See *infra* pp. 70–1 for Weber's and Kirchhoff's detection and correction of this mistake.

[4] Cf. Jungnickel and McCormmach 1986, Vol. 1: 55–8 (Ohm's reception), 58–62 (Fechner), Ch. 2 (physics and institutions).

[5] On the creation of physics seminars, cf. Jungnickel and McCormmach, Vol. 1: Ch. 4; Olesko 1991 (Neumann's).

exchange between neighboring elements of matter, he reduced the problem of heat propagation to the solution of a simple differential equation. The constants appearing in this equation, as well as the variable temperature, had immediate empirical meaning.[6]

In 1826, the 28-year-old Neumann was called to Königsberg, where he spent the rest of his life.[7] There he met the astronomer Friedrich Bessel, who was then busy improving the seconds pendulum for the Prussian reform of weights and measures. Bessel performed this task with the standards of rigor and accuracy he knew from astronomy. He studied the effects of the environment on the pendulum with the utmost care, and controlled the statistical errors of his measurements with Gauss's method of least squares. His declared aim was to 'eliminate the apparatus from the results,' that is, to give a complete theoretical analysis of the relation between the investigated quantity and the measured numbers. As he explained to Alexander von Humboldt: 'Results, which are based on observations, can never be found with the certainty that mathematical truth claims by right of law. Therefore, I consider it essential that results appear together with information that provides the criteria for evaluating them.'[8]

Another astronomer, Laplace, had already pleaded for an astronomically precise physics a long time before. However, French precision measurement pertained more to ideology than to practice. Error analysis was rare, and experimental protocols were often left in the dark.[9] Bessel was foremost among the Germans who turned the Laplacian ideal into a strict discipline. His young colleague Neumann followed him zealously. He made the pendulum analysis the paradigm of his physics, and applied Besselian methods to crystal physics and thermal measurements. In his renowned seminars, he trained students in the thorough analysis of the conditions of measurement and required theories. For the rest of his life he fought the neglect of experiment in mathematical physics and the neglect of mathematical theory in experimental physics.[10]

The severe, repetitive, discipline of error analysis had two important effects on Neumann's style of physics. On the experimental side, he improved the use and design of existing devices rather than inventing new kinds of apparatus.[11] On the theoretical side, he favored the type of theory that was best suited to the mathematical analysis of measurements and their perturbations. He therefore tried to eliminate the hypothetical elements of previous theories, even more than his French heroes had done. His heat theory eliminated Fourier's radiation mechanism, and his

[6] F. Neumann 1826: 324; Fourier 1822. Cf. Olesko 1991, 33–4, 62–3; Jungnickel and McCormmach 1986. Vol. 1: 84–5; Voigt 1895. On Fourier's method, cf. Friedman 1977; Wise 1981a; Grattan-Guinness 1990, Vol. 2: 583–632; Dhombres and Robert 1998: Ch. 8.

[7] Neumann's titles at Königsberg were: Privatdocent in 1826, extraordinary professor in 1828, and ordinary professor for mineralogy and physics in 1829. Cf. Jungnickel and McCormmach, Vol. 1: 85.

[8] Bessel to Gauss, 18 July 1816, in Auwers 1880: 242; Bessel to Humboldt, 24 January 1838, Humboldt *Nachlass*, quoted in Olesko 1991: 73; Bessel 1828. Cf. Olesko 1991: 66–73.

[9] Cf. Olesko 1991: 162–3; Caneva 1974: 345–6, 355–63; Buchwald 1989: 12, 18, 19.

[10] Cf. Olesko 1991: 73–80.

[11] Cf. Olesko 1991: 73, 390.

optics ignored Fresnel's ether molecules. Differential equations, observable quantities, and means to measure them were all he needed.[12]

In principle, he believed, like most of his peers, that mechanics should be the ultimate foundation of physics. In practice, he did not risk the hypotheses that such a reduction would require, and remained satisfied with a set of largely disconnected phenomenological theories. He left grand unification and experimental exploration to more adventurous physicists. This attitude was in full harmony with his psychology as sketched by Helmholtz: 'Neumann is somewhat difficult to approach, he is hypochondriac, shy, but he has a first-class brain.'[13]

2.2.2 *The elementary law*

Besides his contribution to crystal optics, Neumann's most important theoretical piece concerned the laws of electromagnetic induction. His starting point was a qualitative law established in 1834 by the Russian physicist Emil Lenz. Faraday had already given a general rule for determining the direction of induced currents in terms of cut lines of force. However, he hardly applied this rule to Volta-electric induction (induction by varying currents), and did so only in his second series. In the first series he gave the direction of Volta-induced currents directly in terms of the motion of the involved linear conductors. Consequently, Lenz thought that Faraday had no general induction rule. After examining Faraday's examples and performing a few experiments of his own, he concluded:

> The law according to which the magneto-electric phenomenon is reduced to the electromagnetic one is the following: when a metallic conductor [a wire] moves near a galvanic current or a magnet, a galvanic current is induced in a direction such that this current would have produced a motion of the wire [supposed initially] at rest in a direction opposed to that of its actual motion, provided that the wire at rest can only move in the direction of the [actual] motion or in the opposite direction.

Less awkwardly, the induced current is such that the electrodynamic force acting on the carrying wire opposes the motion given to this wire.[14]

Neumann called $E.\mathrm{D}s$ the electromotive force induced in the oriented element $\mathrm{D}s$, and $F\mathrm{D}s$ the projection along the element's motion of the electrodynamic force that would act on the element if a unit current were running in it. In these terms, Lenz's law requires that $E.\mathrm{D}s$ and $F\mathrm{D}s$ should have opposite signs. From Faraday, Neumann further knew that E was proportional to the velocity v of the element and independent of the metal. As the simplest expression that met these conditions, Neumann wrote the 'elementary law':

[12] Cf. Jungnickel and McCormmach 1986, Vol. 1: 148–9; Voigt 1895: 14. Neumann's earliest works had a more Laplacian flavor.

[13] F. Neumann 1883: 1; Helmholtz to Bois-Reymond, 15 January 1850, in Kirsten 1986: 92. Cf. Olesko 1991: 145, 156, 163, 305–06, 456.

[14] Lenz 1834: 488.

$$E.\mathrm{D}s = -\varepsilon vF.\mathrm{D}s \tag{2.1}$$

The coefficient ε, he explained, could be regarded as a constant depending only on the choice of units if the inducing action varied slowly in time. In his opinion the latter condition was only met for induction in wires. In more general cases, such as Arago's disk, he expected the induced currents to lag after the inducing action. He also excluded induction by open currents, for which he did not trust Ohm's law.[15]

From the start, Neumann carefully delimited the experimental circumstances under which his theory could be valid. His concern with measurement also led him to focus on the 'integral current,' which is the time integral of the current induced in a moving circuit:

$$J = -\frac{\varepsilon}{R}\int_{t_0}^{t_1} \mathrm{d}t \oint vF.\mathrm{D}s, \tag{2.2}$$

where R is the resistance of the circuit. This quantity, unlike the differential expression (2.1), is directly observable by measuring the action of the current on a magnetic needle during the corresponding time interval. As we will see, the central concept of Neumann's theory, the potential, comes out naturally in the expression of the integral current. This is an early example of how a consideration of observability may shape a mathematical theory.[16]

2.2.3 The potential law

In order to derive the laws of the various cases of induction, Neumann used the 'elementary law' (2.1), Ampère's expressions for electrodynamic forces, and two additional principles:[17]

1. Induction depends only on relative motion (otherwise, Neumann noted, the motion of the Earth would imply an induction in a conductor at rest near a magnet at rest).
2. The integral current (2.2) depends only on the initial and final states and configurations of the implied bodies (as results from the elementary law in the case of motion-induced currents).

Neumann first considered the case of a linear, closed conductor moving near a magnet. He proceeded gradually, from a single magnetic pole to a continuous distribution of poles. This doing, he did not decide between Amperean currents and magnetic masses: a pole could be thought either as a concentrated magnetic mass or as the end of a solenoid.[18] For a magnetic pole, the force exerted by a closed

[15] F. Neumann 1846 (read on 27 October 1845): 13–16.
[16] F. Neumann 1846: 18–19. Cf. Olesko 1991: 175–7; Jungnickel and McCormmach: 148–52. Neumann did not call the integral current an electric charge: he did not want to analyze the current as a flow of electricity.
[17] F. Neumann 1846: 22, 62. [18] F. Neumann 1846: 40.

current derives from a potential, as results from the Amperean equivalence between this current and a double magnetic sheet. In this simple case, Neumann found that the integral induced current was just the variation of the potential, calculated for a current ε in the circuit and divided by the resistance of the circuit. A similar result held for a magnet, the relevant potential being now a function of the geometrical configuration of the circuit and magnet, and of the distribution of magnetism in the magnet. According to principle (1) the same potential applied to the case when the magnet moved and the circuit was at rest.[19]

For the case of two circuits in relative motion, Neumann replaced the primary circuit with Ampère's double magnetic sheet, and defined the potential between two currents as the potential of the corresponding double sheets. The resulting sextuple integral being unpractical, he returned to the elementary law (2.1) and injected into it the expression (1.6) that Ampère had given for the force acting on a current element from a closed circuit. This yielded a nice potential formula:

$$P = -\frac{ii'}{2}\oint\oint\frac{\mathrm{d}s\mathrm{d}s'\cos\theta}{r}, \tag{2.3}$$

where i and i' are the currents in the two circuits, θ the angle between $\mathrm{d}s$ and $\mathrm{d}s'$, and r their distance (see Appendix 3).[20]

In the case of induction by a variable current, Neumann used principle (2) to replace the intensity variation with the following operations: bring the primary circuit to infinity, change the intensity to its final value, and bring back the circuit to its original position. Since the second operation, performed at infinity, cannot have any inductive effect, the integral induced current is again given by the variation of the potential.[21]

Neumann announced these results in 1845. His considerations were originally limited to rigid circuits. A few months later he could prove in full generality the 'general principle' of his 'mathematical theory of induced currents': the integral electromotive force in a circuit is given by the variation of its potential calculated as if the current in this circuit had the intensity ε. It did not matter how the change of the potential was produced, by displacement of the circuits or the magnets, by deformation of the circuits, by sliding contacts, or by intensity variation of the primary currents or magnets. The basis of the reasonings was the same as in his first memoir. What changed was the mathematical technique: Neumann now used Ampère's expression (1.4) for electrodynamic forces in terms of curvilinear abscissae, since it was well adapted to the curvilinear integrals he had to perform.[22]

2.2.4 *Mathematical phenomenology*

The emergence of Neumann's potential and the correlative simplicity of the induction laws intimately depended on his focus on the integral current in a closed circuit.

[19] F. Neumann 1846: ##5–9.
[20] F. Neumann 1846: ##10–11.
[21] F. Neumann 1846: #10.
[22] F. Neumann 1848.

For a separate circuit element the time integral of the electromotive force is not simply given by the variation of the potential. Neumann found this quantity to be equal to the potential of an imaginary circuit made of the initial and final positions of the element and the traces of its extremities.[23] Evidently, he would not have enunciated such an artificial law had he not known beforehand the simple potential law for the integral current.

In his deduction of the various forms of the potential, Neumann drew on the ample resources of Ampère's electrodynamics, including the various forms of Ampère's law, the relevant mathematical techniques, the equivalence between the end of a solenoid and a magnetic pole, and the equivalence between a current loop and a double magnetic sheet. However, this dependency on Amperean methods had a drawback: Neumann failed to create concepts more appropriate than those found in Ampère. For example, the vector potential, of which the potential (2.3) is the integral, would have greatly simplified Neumann's analytical manipulations.[24]

For one who wished to give a clear empirical meaning to his symbols, the potential integral was a daring step into abstraction. French mathematicians, however, had already introduced potentials for gravitational, electrostatic, and magnetic forces. In Ampère's case of a pole under the action of a circuit, the gradient of the potential gave the force acting on the pole. For a rigid circuit, Neumann found that the variation of the potential of the circuit with respect to a global translation yielded the resultant of electrodynamic forces.[25] The mathematical concept of electrodynamic potential thus became the central, unifying concept of electrodynamics, from which all ponderomotive and electromotive forces could be deduced by simple variations.

We may now give a more precise characterization of Neumann's 'mathematical phenomenology,' as Boltzmann later called it. Neumann constructed his theory on the basis of an empirical rule that allowed a connection with a previous theory. Simplicity, a focus on directly measurable quantitities, and the available analytical techniques guided the developments. The resulting theory expressed directly measurables quantities (the integral current and electrodynamic forces) in terms of a single, more abstract concept (the potential). Neumann thus meant to provide the missing quantitative description of a well-known phenomenon, electromagnetic induction. He did not intend to reveal new effects. On the contrary, he limited the scope of his theory to cases for which the empirical basis was unquestionable: linear conductors, closed circuits, and slowly variable currents.[26]

Neumann's rigor and soberness ensured a lasting value to his work. His potential formula (2.3) is still found in modern electrodynamic textbooks, and his style of mathematical physics initiated a powerful tradition in his country. Few contempo-

[23] F. Neumann 1846: 68. This rule is equivalent to Faraday's rule of the cut lines of force, which Neumann completely ignored.

[24] Cf. Appendix 3.

[25] F. Neumann 1846: 66–7 (Neumann's expression is in terms of current elements, but he meant it to be valid only after integration); 1848: 66–71 (with also a calculation of the torque acting on the rigid circuit).

[26] Boltzmann 1897; 1899: 217–24. Cf. Caneva 1978: 119–21.

rary German physicists, however, knew enough mathematics to appreciate the greatness of Neumann's achievement. Even to the editor of the *Annalen*, Johann Poggendorff, Neumann's memoir was 'rather like Chinese.' The editing of the manuscript was left to a competent mathematician, Carl Jacobi. Neumann had at least one appreciative reader, Wilhelm Weber, who judged that Neumann's laws 'were beyond doubt regarding their mutual connections and the intertwined empirical rules' and proved their equivalence with his own theory in the case of closed currents. In return, Neumann gracefully acknowledged that Weber's competing theory of induction 'threw a bridge over the fault in our knowledge of the electrostatic and electrodynamic actions of electricity.'[27]

2.3 The Gaussian spirit

Weber's electrodynamics owed much to his collaboration with the Göttingen astronomer and mathematician Carl Friedrich Gauss. In 1828 Gauss visited the magnetic observatory which Alexander von Humboldt had recently built in Berlin according to French methods. Gauss was unimpressed. Having collaborated with Bessel on the improvement of astronomical measurements, he was used to much higher standards of accuracy. He soon imagined ways of improving on Humboldt's project. Three years later, Wilhelm Weber obtained the physics chair at Göttingen. Gauss, who knew Weber's exceptional qualities, seized this opportunity to launch an ambitious program of geomagnetic studies.[28]

Gauss had several motivations. He was of course aware of the practical importance of magnetic measurements for navigation and geodesy, which warranted him financial support from the State of Hannover. He also emphasized the 'pure scientific interest of the subject,' which could bring new techniques of magnetometry, magnetic laws and theorems, and insights into the internal structure of the Earth. 'For the *Naturforscher*,' he declared, 'the search for the laws of natural phenomena has an end and a value in itself, and a peculiar charm accompanies the discovery of measure and harmony in the apparently ruleless.' Most importantly, Gauss and Weber wanted to provide new standards for the practice of physics. As Weber later wrote:[29]

> It happens to be my conviction that the way in which physics has been treated so far is outdated and needs to be changed, and that our treatment of the magnetic problem is a first test. It goes against many deep-rooted practices and arouses in many the wish that something like this had not been started; but if it is carried out, it will soon develop further and benefit all parts of science.

[27] Jacobi to Neumann, 5 December 1845, Neumann *Nachlass*, quoted in Olesko 1991: 176 and in Jungnickel and McCormmach 1986, Vol. 1: 150 (like Chinese); Weber 1846: 140; F. Neumann 1848: 48. See *infra* p. 63 for difficulties in the Weber–Neumann equivalence proof.

[28] Cf. Jungnickel and McCormmach 1986, Vol. 1: 65–6; Cawood 1977; Schaefer 1929.

[29] Gauss 1838: 119; 1837: 11; Weber to Karl von Richthofen, 9 April 1841, Weber Nachlass, quoted in Jungnickel and McCormmach 1986, Vol. 1: 76.

2.3.1 Potential theory

The first component of Gauss's program was mathematical. He wished to determine the best representation of terrestrial magnetism and the kinds of measurements to be performed. For this purpose, new experiments were not needed. In his 'general theory of Earth magnetism,' Gauss ingeniously combined disparate theoretical sources: Coulomb's two-fluid theory of magnets, theorems from Poisson's electrostatics and magnetism, his own studies of the distribution of gravitational forces, and his older theory of quadratic forms.[30]

Following Lagrange, Laplace, and Poisson, Gauss introduced the scalar function from which the forces derive, and named it the 'potential' in 1839.[31] By means of quadratic forms of the mass distribution, he derived a number of theorems for the potential function. According to the most important of these theorems, which he knew from a previous study of the gravitational force around a massive ellipsoid, the knowledge of the potential on a closed surface surrounding all masses suffices to determine the potential everywhere outside the surface. Consequently, the measurement of the horizontal component of the magnetic force at every point on Earth is in principle sufficient to determine the whole force, including the vertical component.[32] In practice, Gauss developed the potential into negative powers of the distance from the center of the Earth. The coefficients of this development are functions of the latitude and longitude (Laplace's spherical harmonics) that are completely determined by the harmonicity of the potential, save for a constant factor. The business of the magnetic explorer thus became the determination of the successive coefficients in a rapidly convergent series.

Gauss had most of these results in 1832, although he published them only in 1838. Here he was putting mathematics to the service of magnetic measurements. But he was also interested in the theorems for themselves, and devoted a separate memoir to their systematic exposition. As he emphasized, the same abstract theory (our potential theory) could be applied indifferently to gravitational, electrostatic, and magnetic phenomena, despite their differences of nature.[33]

In this spirit of mathematical decantation, Gauss ignored quarrels on the essence of magnetism. Amperean currents and magnetic fluids were strictly equivalent for his concern. Moreover, the mathematical properties of the potential allowed him to treat the Earth as a black box. Thanks to the above-mentioned theorem, he could ignore the distribution of magnetism within the Earth, and replace it with an equivalent surface distribution. His general theory was 'independent of any particular assumption on the repartition of magnetic fluids in the Earth.' His epistemological

[30] Gauss 1838. Gauss had the main results in 1832: Cf. Schaefer 1929: 14. Most of Gauss's theorems and the name 'potential' were already contained in a little-known essay published in 1828 by George Green: cf. Grattan-Guinness 1995.

[31] Gauss 1839: 200.

[32] The procedure is: integrate the horizontal component with respect to latitude and longitude to get the potential on the surface of the Earth, then find the corresponding harmonic function outside this surface, and finally derive this function with respect to height to get the vertical component.

[33] Gauss 1839.

creed was phenomenological: 'By explanation [*Erklären*], the *Naturforscher* means nothing but the reduction to the smallest possible number of simple fundamental laws; he knows nothing beyond these laws [. . .] but he derives the phemomena from them exhaustively and with full necessity.'[34]

Gauss was more adventurous in private. Manuscripts from the years 1835–36 attest that he tried to explain Ampère's forces in terms of velocity-dependent forces between particles of electricity. He did not publish his results, under the principle that *Nil actum reputans si quid superesset agendum.*[35] He believed that the velocity-dependent terms indicated a propagated action which remained to be found. He had 'the subjective conviction that one would first need to give a constructible representation [*construirbare Vorstellung*] of *how* the propagation happens.'[36]

2.3.2 Absolute measurement

Gauss was equally at home in theoretical and in practical matters. His and Weber's improvements of the methods of magnetic measurement were decisive. First of all, Gauss noted that previous magnetic measurements depended on a variable standard. The magnetic moment of the needle of Henri Gambey's compass diminished in time, and this variation could not be controlled by bringing back the compass to a standard location, since the Earth's magnetism varied in time. Consequently, Gauss required a measuring method that would yield the magnetic intensity in 'absolute units':

In order that we may reduce this measure to distinct notions, it is above all necessary to stabilize the units around three kinds of quantity, namely: the unity of distance, the unity of ponderable mass, and the unity of accelerating force [acceleration].

According to this fundamental principle, the absolute unit of magnetic mass is such that two poles carrying this unit and the unit of ponderable mass and separated by a unit distance repel each other with a unit acceleration. In short, the absolute unit of magnetic mass is the unit for which no numerical coefficient enters Coulomb's magnetic law: $f = mm'/d^2$ in transparent notation.[37]

For the absolute measurement of the horizontal intensity T of the magnetic force of the Earth, Gauss used two series of measurements performed with two different devices. In the first series, he determined the period τ of the small oscillations of a suspended magnetic needle (the torsion of the suspending thread being negligible) for varying computable loadings of the needle. This period is determined by the inertial moment K of the needle, the inertial moment nk_0 of the loading, and the magnetic moment M of the needle:

[34] Gauss 1838: 125; 1836b: 315–16.

[35] Nothing has been done if something remains to be done.

[36] *GW* 5: 616–620; Gauss to Weber, 19 March 1845, in *GW* 5: 629. Gauss's formula for the action between two particles of electricity moving at the velocities $\mathbf{v}$ and $\mathbf{v}'$ and separated by the distance r is $ee'\{1 + [(\mathbf{v} - \mathbf{v}')^2 - (3/2)(\mathrm{d}r/\mathrm{d}t)^2]/c^2\}$ (*GW* 5: 617). It is equivalent to Weber's law when the motion of each particle is rectilinear and uniform. It is not compatible with the energy principle. Cf. J. J. Thomson 1885: 108; Maxwell 1873a: ##846–53.

[37] Gauss 1832a: 85.

$$\tau = 2\pi\sqrt{\frac{K + nk_0}{TM}}. \tag{2.4}$$

Through this formula Gauss deduced the value of the product *TM*.[38]

Then, following a suggestion by Poisson, he determined the equilibrium position of a second suspended magnetic needle placed at some distance from the first needle. This position depends on the ratio *T/M* but also on the higher moments of the first magnetic needle, in a manner that Gauss computed. In order to eliminate these higher moments, Gauss performed several measurements for various mutual configurations of the two needles. Finally, he extracted the intensity *T* from *TM* and *T/M*.[39]

From this example we can see how the ideal of absolute measurement places strong demands on the experimenter. Devices must be conceived so that their workings may be computed on the basis of the fundamental law connecting the measured quantity to mechanical concepts. If this cannot be realistically done, the experimenter must imagine ways to eliminate the non-computable aspects of the apparatus. This may require great ingenuity and superior analytical skills. In Gauss's arrangement, the inertial moment and the higher moments of the magnetometric needle are not computable. The varying computable loading of this needle and the varying configuration of the second needle are clever tricks to eliminate these non-computable elements. Note that the latter trick requires a multipolar expansion of the action of the first needle on the second, certainly a non-trivial step in Gauss's time.

With Weber's support, Gauss was equally ingenious when he came to the precise design of apparatus (Fig. 2.1). He used a magnetic needle of one pound and one foot, suspended by a silk thread of two feet and a half. The wooden box A protected the system from air drafts. The deviations of the needle were measured by means of a mirror attached to the needle and the goniometer f placed far from the system. This apparatus had several advantages over Gambey's old compass. The needle, being much heavier, was less sensitive to perturbations. Its larger period was easier to measure. The goniometric measurement of the deviation of the needle avoided the perturbations that Gambey's microscopic observation necessarily involved. Gauss and Weber also paid much attention to the quality of the various parts of their instruments. Weber went so far as to supervise the fabrication of the steel of the magnetic needles in the royal ironworks. Lastly, Gauss conceived the optimal environment for the setups, with no perturbing iron and properly organized space. In 1833 he persuaded the curator of Göttingen University to create a magnetic observatory that met these conditions.[40]

The global understanding of geomagnetism required simultaneous measurements

[38] Gauss 1832a: 92–100.

[39] Gauss 1832a: 100–11. Increasing the distance would diminish the contribution of higher moments, but then the precision on the measurement of *T/M* would be too weak.

[40] Gauss 1832a: 95; 1832b: 298–301. Cf. Jungnickel and McCormmach 1986, Vol. 1: 71–3; Dörries 1994; Olesko 1996 (on the issue of replicability).

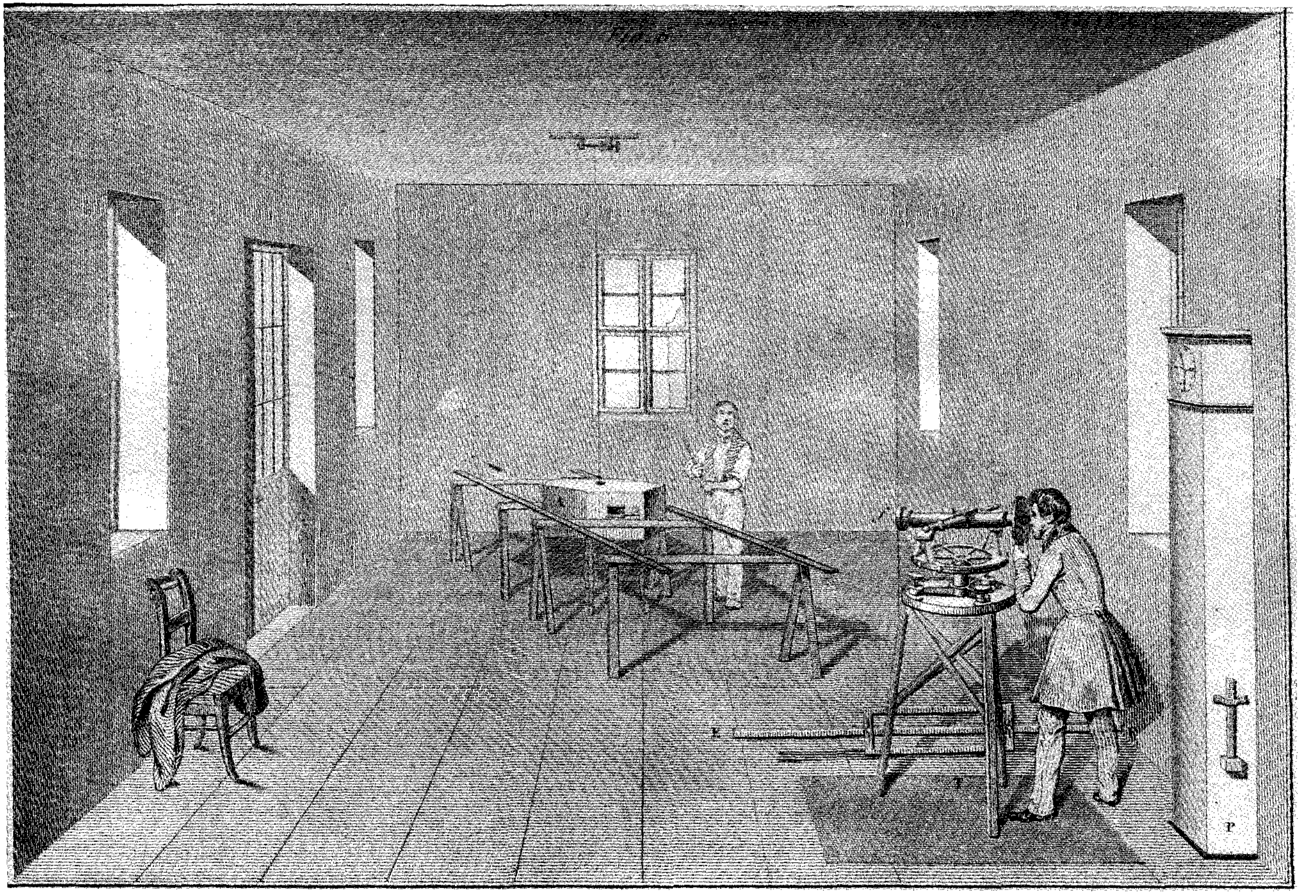

FIG. 2.1. Gaussian apparatus for magnetic intensity measurement (from A. Becquerel 1834–40, Vol. 7: plate 5).

in many parts of the world. Therefore, it implied a highly organized social activity. Gauss was fully aware of this dimension of his project, and he drew the following conclusions. The apparatus had to be described in all details so that other investigators could duplicate it without seeing the original. The measurement protocol had to be much more explicit than had usually been the case. A network of magnetic observatories had to be built according to the model set in Göttingen. Directions had to be given regarding the time and frequency of the measurements, and data had to be centralized. Such was the purpose of the informal Magnetic Union and its journal created in 1836.[41]

Gauss's project was immensely successful. In Germany and England, Gauss and Weber's methods quickly replaced the old French ones. A better knowledge of the magnetism of the Earth ensued. Absolute measurement became a leitmotif of German physics, even though it turned out to be rarely practical. According to Weber, the magnetic observatories acted as 'educational institutions for exact observers.' In sum, Gauss and Weber reduced the gap between physics and applied mathematics. As Gauss put it,[42]

> Magnetic experiments are becoming capable of a precision which far surpasses everything that went before, and its fundamental laws can have a truly mathematical precision, so that the separation between actual so-called physics and applied mathematics here too (as in the theory of motion and optics long ago) begins to disappear, and the thorougher treatment begins to fall to the mathematician.

2.4 Weber's *Maassbestimmungen*

Gauss was eager to extend his methods to galvanism. In a popular lecture of 1836, he proclaimed: 'Oersted's and Faraday's brilliant discoveries have opened up a new world of scientific research, whose enchanted gardens will fill us with admiration; these rich fields can only be conquered under the art of measurement.' A year later, he obtained a highly sensitive galvanometer by combining one of his high-class magnetometers with Schweigger's multiplicator. He also installed an electromagnetic telegraph—the first of this kind—between the observatory and Weber's institute, and determined with it that the velocity of electricity was too high to be measured. With Weber's help, he used the electromagnetic induction in a copper ring to measure the vertical component of the magnetic force of the Earth. Unfortunately, in 1838 Weber was dismissed from his Göttingen chair for political reasons. Gauss felt discouraged: 'The arrangement of the new intensity apparatus lets us look into a new world of wonders. But now that the way has been paved into this world, the gate is to be slammed shut in our faces.' Weber continued alone, and with some delay.[43]

[41] Cf. Jungnickel and McCormmach 1986, Vol. 1: 73–5; Dörries 1994.

[42] Weber to Sabine, 20 September 1845, quoted in Jungnickel and McCormmach 1986, Vol. 1: 77; Gauss to Göttingen Universität Curator, 29 January 1833, quoted *ibid.*: 70.

[43] Gauss 1836b: 336 (quote); 1837: 367 (galvanometer), 369–72 (telegraph); Gauss and Weber 1837 (ring); Gauss to Olbers, 2 September 1837, quoted in Jungnickel and McCormmach 1986: 75. For the reasons for Weber's dismissal, cf. *ibid.*: 131.

2.4.1 *Measure and picture*

Weber's interest in precision measurement antedated his collaboration with Gauss. During his student years in Halle he joined his brother Ernst Heinrich in a thorough study of mechanical waves. He read and admired the French classics on this subject, Laplace, Cauchy, and Poisson, but deplored that the field had developed in a purely mathematical manner, without proper empirical foundation. The two brothers' aim was to provide this foundation, as Fresnel had done for wave optics. They scarcely used mathematics in their authoritative *Wellenlehre auf Experimente begründet*, although they did not lack the competence. In contrast with Neumann's style of measurement, worth noting are their inventiveness for the measuring apparatus, their use of visual representations (plots and diagrams), and their interest in establishing new standards. For example, they noted that the frequency of tuning forks depended on the exciting mode and on the resonating cavity, and they proposed special reed flutes as an alternative frequency standard. Drawn by the logic of their subject and by a strong sense of unity, they easily crossed disciplinary borders, moving into musical acoustics and the physiology of hearing.[44]

During his collaboration with Gauss, Weber was especially interested in extending Gauss's methods to electromagnetism and electrodynamics. Toward the end of this period he studied 'unipolar induction,' that is, induction by the motion of a single magnetic fluid. According to unpublished considerations by Gauss, a magnetic point-mass μ moving with the velocity v produced in the element dl of a linear conductor at rest the electromotive force $v\mu dl \sin\theta / r^2$, where r is the distance between the magnetic mass and the element, and θ the angle between the velocity of the mass and the element. From this rule Weber deduced that the cyclic motion of a magnetic mass produced an electromotive force in a conducting loop if and only if the motion embraced the loop. If, Weber went on, the magnetic fluids really existed in a magnet, and if they were separated within microscopic cells *à la* Coulomb, the rotation of a magnet around its own axis had to produce an electromotive force in any stationary path between the Noth pole and the meridian of the magnet (closed by an external, fixed wire), because for any cell cutting the path, only the boreal fluid embraced the path in its motion. Weber thus explained the effect observed by Faraday on the device of Fig. 2.2, and he went on to demonstrate several quantitative properties of this kind of induction. Finally, he argued that Ampère's theory of magnets was incompatible with the phenomenon. In his opinion, Amperean loops, unlike Poisson's cells, could not induce any current when they crossed the conducting path.[45]

This episode reveals important aspects of Weber's way of picturing phenomena.

[44] Weber and Weber 1825; *WW* 1: 267 (*Zungenpfeifen*). Cf. Jungnickel and McCormmach 1986, Vol. 1: 45–50.

[45] Weber 1839. Gauss's theory was presumably based on the reciprocity between electromagnetic forces and magneto-electric induction. It was announced in Gauss 1835: 532, but never published. The given law is in Weber 1846: 136–7, with no specific attribution. Amperean loops are in fact perfectly equivalent to Poisson's cells with respect to unipolar induction. Weber's contrary belief depended on the absence of what he regarded to be the reciprocal effect: a normal force between a plane (macroscopic) current loop and a current element (belonging to the Amperean currents) crossing that plane.

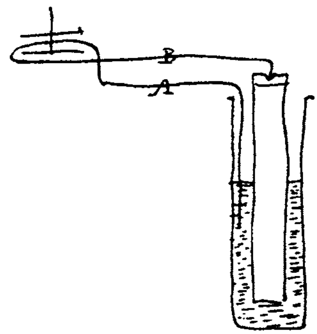

FIG. 2.2. Faraday's device for induction in a rotating cylinder magnet (*FD* 1: 403). The magnet floats in a mercury bath. Its rotation is started by a spring (not represented). The wires A and B lead to a galvanometer.

Unlike Gauss, he did not shy away from hypotheses on the nature of magnetism and he even considered a microscopic model of the magnet. Unlike Faraday, he regarded the Newtonian analogy as suggestive of such hypotheses. Like Ampère, he oriented his choice of structural hypotheses according to their unifying power. Ampère reified Amperean currents because they unified electromagnetism and electrodynamics. Weber reified the magnetic fluids because they unified electromagnetism and magneto-electric induction. The two physicists had a different view of the order in which unity should be increased, but they shared the same ideal of a global physical picture. When Weber later detected a mistake in his proof that unipolar induction excluded Amperean currents, he adopted them.[46]

2.4.2 *Criticizing Ampère*

Having lost his Göttingen institute, Weber remained without a permanent position for a few years. In 1843, however, he was offered the Leipzig chair of physics with very good material conditions. There he resumed his researches on electrodynamics. In 1846 he published the first instalment of his monumental *Elektrodynamische Maassbestimmungen* (literally: electrodynamic measure determinations). His aim was to extend Gaussian methods to the various aspects of electrodynamics. In his opinion, pure electrodynamics lacked a proper empirical foundation. No quantitative measurements had ever been made of the discouragingly small forces between two electric currents. In electromagnetism (current–magnet interactions), the forces were larger and better measured. For one who doubted the existence of Amperean currents, however, electrodynamics needed to be studied separately.[47]

The empirical foundation that Ampère himself claimed to have given to electrodynamics did not meet Weber's standards:[48]

[46] On this mistake, cf. Weber 1852: 558n. [47] Weber 1846. [48] Weber 1848a: 216.

Ampère, more a theorist than an experimenter, used the faintest indications of experiments in the sharpest manner, and he gave his system such a fine development that the rough state of the observations on which he initially relied was not in just proportion with the developed theory. Be it for firmer foundation and fructification or for refutation, electrodynamics needs a more accomplished technique of observation that enables us to enter more specific discussions of the comparison between theory and experiment and thus to equip the soul of the theory with an appropriate organ of observation, without which the soul's forces cannot unfold.

Weber found much to criticize in Ampère's experiments. He recalled that some of these existed only on paper, and gave reason to doubt their feasibility. He reproached Ampère for giving only 'negative experiments,' that is, experiments that only prove the vanishing of forces in certain circumstances. This null method could not be regarded as quantitative, because Ampère did not analyse the sensitivity of his devices. More generally, Weber criticized Ampère's silence about the experimental protocol:[49]

In such fundamental experiments, it is not enough to give their aim and to describe the instruments with which they are made and in general to simply add the assurance that they met the expected success; on the contrary, it is also necessary to enter the precise details of the experiments and to say how often each experiment has been repeated, which modifications were made, which influence the latter have had, in brief, to commmunicate in the manner of a protocol all the data that contribute to found our judgement about the degree of certainty of the results.

2.4.3 The electrodynamometer

With Gaussian standards, Weber sought a quantitative proof of Ampère's electrodynamic law. Ampère's instruments were inadequate for at least two reasons. Owing to their simple geometry, they involved exceedingly small electrodynamic forces. Their articulations based on mercury cups implied too much friction. Weber replaced Ampère's single wires with square or circular coils whose action was still computable but much more powerful, and he invented a new kind of suspension of the mobile coil.

The story of this suspension is a remarkable case of convergence between a material constraint and the demands of precision measurement. In 1834, Weber had already had the idea of suspending the galvanic coil by the two wires feeding the current. The idea was almost obvious, since the wires of a coil of many turns had to be very thin and therefore flexible. This suspension was devoid of friction, and could be used to show the electrodynamic influence of another, static coil. Yet Weber only realized the full potential of this device after he knew of another bifilar suspension invented by Gauss in 1837. Gauss's aim was to perform quasi-instantaneous measurements of the magnetic force of the Earth, which the oscillation method, of course, did not allow. The obvious solution was to turn the suspended magnet into

[49] Weber 1846: 6. Cf. Caneva 1978: 106–09.

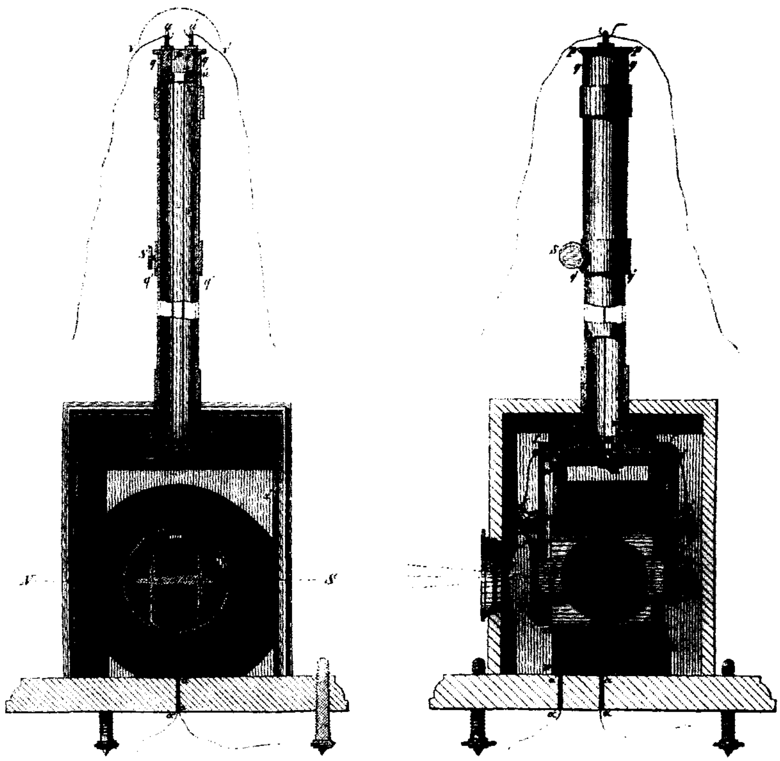

FIG. 2.3. Vertical sections of the electrodynamometer (Weber 1846: 11–12). The static coil yy is fed by the wires $\alpha\alpha'$. The bifilar coil cc is attached to the fork klk'l', which hangs through the two wires that feed it. The mirror ff' permits the optical measurement of the rotation of this coil.

a torsion balance. For the purpose of absolute measurement, however, the restoring torque of the suspension had to be computable and commensurable with the magnetic torque. Gauss hit upon the idea of suspension by two parallel silk threads, for which the restoring torque was a simple function of the weight of the magnet and which could be adjusted by playing on the distance and the length of the threads. The magnetic needle suspended in this manner, together with the wooden case, mirror, and the goniometric device, constituted Gauss's bifilar magnetometer. Weber realized that the same measuring technique could be transfered to his double-coil apparatus. Thus was born the 'electrodynamometer' (Fig. 2.3).[50]

In Leipzig, Weber verified simple consequences of Ampère's law with this highly sensitive and frictionless instrument. He first proved that the electrodynamic torque between the two coils was proportional to the square of the intensity of the current in the coils, the current being measured by the effect of the static coil on a distant

[50] Weber 1846: 10; Gauss 1837.

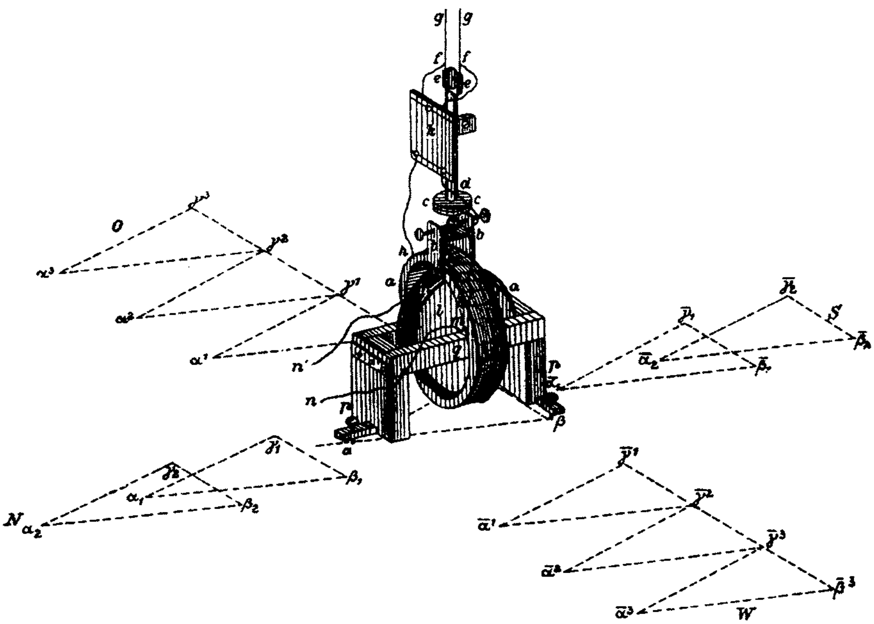

FIG. 2.4. The separable electrodynamometer (Weber 1846: 28). The bifilar coil aaa is kept at a constant position. The static coil lll can be moved to different positions $\alpha\beta\gamma$.

magnetometer.[51] In this and other experiments, Weber carefully eliminated external perturbations and described his protocol with extreme precision. However, unlike Neumann he did not make any error estimate. He satisfied himself with showing the stability of his results under repetition.[52]

Weber then used a special electrodynamometer from which the static coil could be removed and placed at various distances and angles from the suspended coil (Fig. 2.4). With this device he verified the implications of Ampère's law for the torque as a function of the geometric configuration. The calculations were eased by the analogy with Gauss's earlier device with the two magnetic needles. Weber concluded, a bit optimistically: 'This complete agreement between the values calculated by Ampère's formula and the observed ones [. . .] is, considering the diversity of the circumstances under which this agreement holds, a complete proof of Ampère's fundamental law.' Of course, he had proven nothing for open or variable currents. He could verify, however, that Ampère's law still held for the very brief currents produced by the discharge of a Leyden jar.[53]

Weber's measurements determined absolute units for the electric current. The

[51] The effect of the suspended coil on the magnetometer was negligible because it had very few turns compared with the static coil. Of course Weber took due account of the magnetic force of the Earth.

[52] Weber 1846: 15–25. [53] Weber 1846: 25–50; quotation on 50.

electrodynamometer, analyzed according to Ampère's law, gave the measure of the current in 'electrodynamic units,' while the combination of a multiplicator and Gauss's absolute magnetometric procedure gave the current in 'electromagnetic units.' Weber calculated that the former determination was $\sqrt{2}$ times the latter, and verified that his actual measurements complied with this relation (see Appendix 2). In his conclusion, he emphasized his Gaussian concern with absolute measurement, but noted difficulties:

The present investigation mainly aims at *experimentally* determining measures of electrodynamic forces and at expressing these measures in *absolute* units reduced to the units of space, time, and mass. Such was the motivation of the instruments' layout, which, like Gauss's magnetometers, requires a more rigid installation and a larger space than other physical apparatus for which the standard [*Maassstab*] is directly included in the observing instrument.

The work, Weber continued, would have been impossible if the Leipzig Physics Institute had been less spacious. Worried that duplication would be impossible for less favored investigators, he advertized simplified portable versions of his instruments made by a local mechanician.[54]

In order to appreciate the historical importance of Weber's and Gauss's innovations in instrument design, one must remember that in most of the nineteenth century instruments were built and often conceived by nearly illiterate craftsmen. Consequently, the interventions of physicists of Weber's or Thomson's caliber in this field marked essential breaks in instrumental traditions. Their instruments were widely imitated, with variations depending on the intended use, or some new aspects were transposed to other kinds of measurements, as happened with the bifilar suspension or the mirror-goniometer method.[55]

2.4.4 Organ and soul

With his electrodynamometer Weber had the proper measuring 'organ' for the 'soul' of Ampère's electrodynamics. He conceived two kinds of application:[56]

A finer technique of electrodynamic observations is not only significant and important for the proof of the fundamental principle of electrodynamics [Ampère's law], but also because it will be the source of new investigations, which otherwise could not be done at all.

Ampère had missed Faraday's discoveries, Weber went on, because he lacked the proper technique of observation. In contrast, the electrodynamometer exhibited Volta-induction and permitted a quantitative study of it. Weber only had to close the bifilar coil on itself and observe its oscillation when the static coil was fed with a constant current. A current was induced in the bifilar coil, so that an electrodynamic torque acted on it. With Gauss's method for measuring oscillation periods, Weber

[54] Weber 1846: 51–60, 96.
[55] Cf. Blondel 1997 for electric instrument making in nineteenth century France. Not every aspect of Weber's intrumental techniques convinced technicians: cf. Olesko 1996: 121.
[56] Weber 1846: 9–10.

determined the damping effect of the static coil and inferred two basic properties of the induced current: its sign, and its proportionality to the angular velocity of the circuit. He also proved that the same damping effect was obtained by substituting the static coil with a system of magnets that impressed the same torque on the bifilar coil when the latter was fed with the same constant current. In this manner, he could reduce the laws of Volta-induction to the better-known laws of magneto-electric induction.[57]

Weber also imagined applications of the electrodynamometer outside the field of electrodynamics. Since the two coils' interaction depended on the square of the intensity, this instrument could respond to rapidly alternating currents. No such current was known at that time, unless, as Weber speculated, light was itself a kind of oscillating current with extremely high frequency. Mechanical oscillations could however induce electric oscillations by means of an attached magnet and a static coil. Weber used this arrangement to study the vibrations of a sounding bar, thus reviving his old interest in the physics of waves. He also conceived a physiological application of the electrodynamometer. The intensity i and the duration τ of the electric impulses employed to excite nerves could be determined by the combined use of an electrodynamometer and a galvanometer. In the former instrument, the amplitude of the first oscillation gave the product $i^2\tau$; in the latter, it gave the product $i\tau$.[58]

In sum, the refined technique of measurement embodied in Weber's electrodynamometer brought unity within and without electrical science. The same apparatus served in many different contexts: electrodynamic forces, electromagnetic induction, electrostatic discharges, mechanical vibrations, and even physiology. Of course, unity in instrumental methods does not necessarily imply unity in the nature of phenomena. Within the narrower context of electrical science, however, Weber's *Maassbestimmungen* transcended the aim of measurement. They determined the phenomena to be measured just as much as they measured them. Therefore, instrumental unity implied phenomenal unity.

2.4.5 *Fechner's idea*

The unity was especially impressive for electrodynamic forces and Volta-induction, since both phenomena were essential to the working of the electrodynamometer as a measuring instrument. However, a comparable unity was still lacking at the theoretical level: there was yet no quantitative theory of Volta-induction (Weber knew about Neumann's theory only after completing his own), and Lenz's rule for relating Volta-induction to electrodynamic forces was purely empirical and qualitative. Weber could not accept this situation:[59]

[57] Weber 1846: 61–75.

[58] Weber 1846: 89–92, 76–81. The instruments are used ballistically: τ is very small compared to the period, and the damping is large enough so that the maximal deviation is easily observed. The name 'ballistic galvanometer' is Thomson's (*TMPP* 2: 332).

[59] Weber 1846: 99.

The measure determinations of the Volta-induction belong to the *electrodynamic measure-determinations*, which form the main subject of this memoir, and which, in order to be complete, must include the phenomena of *Volta-induction*. However, it is obvious that the establishment of such measure determinations is intimately connected with the establishment of the *laws* to which the relevant phenomena are submitted, so that the one task cannot be separated from the other.

For suggestions about how to complete theoretical electrodynamics, Weber turned to Ampère. According to the French philosopher, electrodynamic forces perhaps resulted from the electrostatic forces between the moving electric fluids. If a reduction of this kind succeeded, Weber hoped, Volta-induction might follow from it. Weber's predecessor in the Leipzig chair, Gustav Fechner, had already explored the idea.[60]

Following Ampère, Fechner imagined that an electric current consisted of a symmetrical double flow of electric fluids and that the forces between two current elements resulted from the forces acting on the fluids. Unlike Ampère, he assumed that the flow was constant and uniform. Then the forces between two fluid particles[61] had to depend on their velocity, in a manner that Fechner guessed from the actions between current elements. Next, Fechner considered the motion of a straight conducting wire toward a parallel rectilinear current, and showed that the velocity-dependent forces acting on the fluid particles of the wire tended to separate the two fluids, at a rate proportional to the velocity of the wire. In sum, he deduced a simple case of Volta-induction from the existence of Ampère's forces. For the reciprocal case, in which the inducing current moves while the conducting wire is at rest, he used the principle that induction depends only on relative motion. For induction by a variable current, he assumed that the decrease of a current was equivalent to its being taken away.[62]

Fechner deplored the artificial character of the latter assumption. He also lamented over his weakness in mathematics, which confined him to the simplest geometrical cases of induction—and made him switch to psychophysics. Weber took over, and developed Fechner's brilliant idea with superior analytical power. He adopted the uniform double flow for calculations, although he saw more truth in the composition–decomposition of the two fluids assumed by Ampère:

This simultaneous, opposing motion of positive and negative electricity, as one usually assumes in all parts of a linear conducting wire, cannot really exist, but can be seen, for our purpose, as an *ideal* motion which, as long as we only deal with action *at a distance*, *replaces* the really occurring motions with respect to all the actions under consideration and thereby has the advantage of lending itself better to calculation.

Toward the end of his memoir Weber verified that the simplification did not affect his results.[63]

[60] Weber 1846: 5; Fechner 1845.

[61] By 'particle' (*Theilchen*) Fechner and Weber only meant infinitesimal part. Weber's electricity became corpuscular only later.

[62] Fechner 1845. Fechner also deduced the magnetic action of a convection current.

[63] Weber 1846: 100, 164–6. Weber implictly excluded terms proportional to $(\mathbf{v}-\mathbf{v}')^2$. If such terms are allowed, Weber's law is not the only possibility (see note 36, p. 51 for Gauss's choice).

2.4.6 Weber's law

Weber assumed that the action between two particles of electricity only depended on their relative distance and its first two time derivatives, which he erroneously called relative velocity and acceleration. He further determined this action on the basis of two facts: the repulsion of two collinear current elements, and the attraction of two parallel current elements forming the opposite sides of a rectangle. The first action can be explained by correcting Coulomb's law with a velocity-dependent term, in Fechner's manner. In the second case, however, the relative velocity (in Weber's sense) of the particles of electricity of the two elements is always zero, since the distance between these particles reaches a minimum when they pass the elements. Weber therefore introduced another, acceleration-dependent, correction to Coulomb's law. Specifically, he showed that the formula

$$f = \frac{ee'}{r^2}\left[1 - \frac{1}{C^2}\left(\frac{\mathrm{d}r}{dt}\right)^2 + \frac{2r}{C^2}\frac{\mathrm{d}^2 r}{\mathrm{d}t^2}\right], \tag{2.5}$$

where r is the mutual distance of the particles, e and e' their charges, and C a constant, gave the proper electrodynamic forces in the two cases.[64]

Remarkably, Weber found that this simple formula implied Ampère's formula in the most general case, even for variable currents. He then used it to derive induction formulas that fitted known empirical laws (see Appendix 4). For example, the acceleration-dependent term of his fundamental law directly provided the induction by a variable current. In these calculations Weber limited himself to linear conductors, for which he benefitted from Ampère's various mathematical techniques, including spherical trigonometry and curvilinear abscissae. The latter tool was especially convenient, since for linear conductors Weber's relative velocities and accelerations directly translated into Ampère's derivatives with respect to curvilinear abscissae.[65]

When he came to know Neumann's formulas, Weber verified their agreement with his fundamental law for closed currents. However, Neumann soon pointed to a contradiction in the case of sliding contacts in the inducing circuit. He performed an experiment confirming the prediction of the potential law, and showed how to save Weber's law: the transitory acceleration of Weber's fluid particles in the sliding contacts had to be taken into account. Weber agreed and perfected Neumann's argument. In general Weber's theory required detailed microscopic considerations in order to justify the passage from molecular forces acting on the fluids to macroscopic forces acting on the total current or on the current carrier. But it was more general than Neumann, since it included electrostatics and induction by open

[64] Weber 1846: 99–108. Cf. Whittaker 1951: 201–3. Originally, Weber used the constant $a = 4/C$ instead of C. In 1850 he used the constant C, which he denoted c. I have used the notation C in order to avoid the confusion with the modern c ($= C/\sqrt{2}$), which denotes the ratio of the electromagnetic to the electrostatic charge unit (and is equal to the velocity of light according to Maxwell's theory).

[65] Weber 1846: 109–12, 113–19, 126–32, 143–64. Cf. Whittaker 1951: 204–5. For a modern elaboration, cf. Assis 1994.

currents. It was also more suggestive of the ways in which matter could interact with electricity.[66]

On the darker side of Weber's approach, a velocity dependence in a fundamental force formula was unheard of. Weber anticipated criticism of this aspect of his theory, and defended it as Ampère had defended the occurrence of angles in his force formula. To the question of whether time derivatives should be tolerated in the expression of elementary forces, he replied: '*A priori* this question cannot be decided, because there is nothing contradictory, unclear, or indefinite in the assumption of such forces.' He gave only a descriptive value to his force law:

The laws giving the dependence of forces on given physical circumstances are called *physical fundamental laws*; according to the aim of physics, these laws are not meant to give an *explanation* of the forces by their true causes but to give a clearly presented and useful general method for the *quantitative* determination of the forces by means of the fundamental measures established in physics for space and time. Therefore, from a physical point of view there is no scandal in making a force a function of time-dependent relations, no more than for dependency on distance, because a time-dependent relation is just as measurable a quantity as a distance.

Weber even suggested that other kinds of forces, for instance gravitational forces, should be corrected in the manner he had done for electrostatic forces.[67]

Again following Ampère, Weber did not exclude a more fundamental level of description. He noted that the dependence of the force between two charges on their relative acceleration implied that, in the presence of a third body, their relative acceleration depended on the action of this third body. This fact indicated that the interaction of the two charges was not direct, that it required a medium on which the third body could act. Weber then referred to Ampère's suggestion that the medium could be the neutral fluidum made of bound positive and negative electricities. Perhaps the same medium could serve to propagate light, as Faraday's recent discovery of an action of magnetism on light propagation seemed to indicate.[68]

Weber did not immediately follow up these speculations. More important to him was the anchoring of the theory on precise, well-defined measurements. As he argued in his defense of velocity-dependent forces, the aim of fundamental laws was to determine forces by the fundamental measures of space and time. This definition is highly interesting, because it indicates an intimate connection between Weber's concept of law and the Gaussian ideal of absolute measurement. This is confirmed by the reason Weber later gave for the possibility of absolute units:[69]

All other kinds of magnitudes [other than time, length, and mass] can be observed on certain geometrical or mechanical objects *at the same time* as those [length, time, and mass] for which the fundamental unit is established; and the then available relations between the different kinds of magnitudes, given as *geometrical* or *mechanical* laws, would suffice to *derive* units for all other kinds of magnitude from the three established fundamental units.

[66] Weber 1846: 138–143; Neumann 1848: 48–66; Weber 1852: 310–334.
[67] Weber 1846: 112–113. [68] Weber 1846: 167–170. [69] Weber 1861: 529.

2.4.7 *A threefold unity*

Weber's conception of mesurement determinations brought a special harmony to his work. His unification of electrical science had three intimately related aspects: the focus on absolute and precise measurement, the invention of a universal instrument, and the introduction of a fundamental law of electric interactions. The production of two different phenomena, electrodynamic forces and Volta-induction, by the same apparatus indicated an underlying theoretical unity. The reduction of the measurements to absolute units suggested seeking this unity in mechanical terms. Hence Weber conceived his theory of electrodynamics as a correction to the only electric theory that had previously been reduced to the action of mechanical forces on specified masses: Coulomb's electrostatics. This is why he reduced electrodynamic interactions to mechanical forces between moving electric fluids.

The comparison with Neumann is instructive. Both physicists promoted new standards of precision measurement. Yet they produced different kinds of theory. Neumann favored the phenomenological approach, while Weber required a basic picture of the electric currents and their interactions. This difference may in part reflect their first experiences in mathematical physics: Neumann began with the thermal properties of crystals and venerated Fourier, whereas Weber made his début with the vibrations of solids, of which Poisson had given the best theory. Neumann's theoretical style could be seen as an extension of Fourier's phenomenology, and Weber's as a modification of Poisson's neo-Newtonian physics.

This is a fragile interpretation, however, for it ignores the relations between the theoretical and experimental practices of the two German physicists. Neumann's preference for phenomenological theories should rather be seen as a correlate of his static, regional concept of experiment. For him, instruments and the theories required for their analysis were essentially given. Similarly, his new theories were based on already known experimental facts. Consequently, he did not need more theoretical unity than there was in already known facts. In contrast, under the Gaussian call for absolute measurement Weber conceived essentially new instruments and used them to relate different phenomena. This drive toward unity prompted him to open the theoretical black box of electric current and transcend Neumann's phenomenology.

2.4.8 *A new fundamental constant*

In his unification of electrodynamics with electrostatics, Weber introduced the new fundamental constant C, which occurred in the fundamental law and gave the scale of the velocity-dependent correction to Coulomb's law. In 1850 he defined C as 'the relative velocity for which two electrical masses do not at all interact' (according to formula (2.5)). He also revealed the essential role of the constant in the context of absolute measurement. On the one hand, an electric current can be measured by its electrodynamic action, and the absolute electrodynamic unit of intensity is thus defined. On the other hand, the current can be measured as the quantity of electricity it carries in a unit of time, in conformity with Weber's picture of the current and with his and Faraday's proofs that electrostatic electricity can produce the same electrodynamic effects as Voltaic electricity. The quantity of electricity can itself be counted in the absolute unit for which there is no constant factor in Coulomb's law. The resulting unit of current is what Weber called the mechanical or electrostatic unit. According to Weber's law, the electrostatic measure of the current is $C/2$ times its electrodynamic measure (see Appendix 2).[70]

The determination of the numerical value of C meant much to Weber. After five years of effort, and with the cooperation of his friend Rudolph Kohlrausch, he succeeded in this task. The principle of the determination was simple. A Leyden jar was discharged through a home-made ballistic galvanometer, which gave the electrodynamic measure of the integral current. Then this measure was compared with the electrostatic measure of the jar's loss of charge. In practice, the measurements were very difficult. The galvanometric part required all of Weber's skills, and the electrostatic part all of Kohlrausch's. To measure the charge of the jar, the two friends touched it with a conducting sphere, determined the charge of this sphere with a Coulomb balance, and found which fraction of the jar's charge this represented by measuring the tension of the jar before and after the contact. The leakage of the jar had to be taken into account, and the Coulomb balance had to be manipulated with the utmost care, at night-time and in an unheated room in order to avoid air drafts. Weber and Kohlrausch took the average of five measurements, and found, with a wealth of illusory decimals: $C = 439450 \times 10^6\,\mathrm{mm/s}$.[71]

Weber noted the proximity of this value to the velocity of light, but only to emphasize the disparity of their physical meanings. In his eyes, the essential significance of the measurement of C was that it brought his unification of electrostatics and electrodynamics to a quantitative experimental conclusion. All electrodynamic quantities could now be measured in electrostatic units. By calling the latter units 'mechanical units,' Weber advertized his reduction of all electric phenomena to the mechanical motion of the electric fluids. Conversely, he suggested that the new fundamental constant, together with the gravitational constant, could serve to reduce the three independent units of mechanics to the unit of length.[72]

2.5 Kirchhoff compared with Weber

Considerations of a unified world-view had no counterpart in Neumann's phenomenology. Further differences between Neumann's and Weber's methodologies can be traced in work done within their circles. A first example is Gustav Kirchhoff's

[70] Weber 1850: 268; ibid.: 267–70. Cf. d'Agostino 1996. C is not Weber's original notation: see note 64 above. For the ratio of electrostatic to electrodynamic measure of current, Weber had $C/4$ instead of $C/2$ because for the electrostatic measure of the current he counted only the flow of positive electricity. This is an example of how a microscopic picture may affect conventions at the macroscopic level.

[71] Weber 1855; Weber and Kohlrausch 1856, 1857. Cf. Rosenfeld 1956; Jungnickel and McCormmach 1986, Vol. 1: 144–6; d'Agostino 1996.

[72] Weber 1855: 595; Weber and Kohlrausch 1857: 667–9. Cf. Rosenfeld 1956.

solution of the prize problem proposed by Neumann in 1846: the determination of the 'deeply mysterious' constant ε of Neumann's theory of electromagnetic induction.[73]

2.5.1 *The mysterious* ε

Kirchhoff was the most oustanding of the early participants in Neumann's seminar. His first physics work, the determination of the electric current in a conducting disk fed at two points on its periphery, played themes of Neumann's physics, with a few modulations. The aim was to calculate the distribution of current according to an already known theory, Ohm's theory of electric conduction, and to verify the consequences for the tension and the resistance by means of available apparatus. This would have been a typical exercise in Neumann's seminars if Kirchhoff had not pushed the mathematical analysis far beyond the norm. He solved the relevant differential equations with great virtuosity, and used the resistance measurement as a pretext for a theory of linear conducting networks (our 'Kirchhoff's laws'), which he published in two subsequent papers.[74]

For his determination of the constant ε, Kirchhoff combined the resources of Neumann's theory of induction and of his own theory of networks. In Neumann's theory, the empirical meaning of the constant appears in the expression of the 'integral current' (time integral of the current) J induced in a circuit during a change of its potential with respect to a constant current I:

$$J = \frac{\varepsilon}{R} I \Delta P, \tag{2.6}$$

where R is the resistance of the circuit and ΔP the potential variation for $I = 1$. Hence, the value of ε can be determined by measuring I by the static deviation of a galvanometer, J by the ballistic deviation of the same galvanometer, and calculating ΔP. The result depends on the units chosen for electric resistance, for length (in the calculation of the potential P, which has the dimension of the inverse of a length), and for time (for the period of the galvanometer, which relates integral intensity measurements to intensity measurements).[75]

Instead of using separate circuits, Kirchhoff cleverly imagined the network setting of Fig. 2.5, in which R_1 and R_2 are two coils, M the galvanometer (multiplicator and suspended magnetic needle with mirror and telescope), K a battery, and 0 a copper wire. He measured the intensity I_2 in the branch 2 when the two coils were at rest, and the integral intensity J_2 during a separation of the two coils. The resistance R_0 of the branch 0 being much smaller than the resistances R_1 and R_2 of the two other branches, we have the approximate relations

[73] Neumann to Jacobi, 5 February 1846, quoted in Jungnickel and McCormmach 1986, Vol. 1: 152.

[74] Kirchhoff 1845, 1847, 1848. Cf. Olesko 1991: 179–82; Jungnickel and McCormmach 1986, Vol. 1: 153–5. For his resistance measurement Kirchhoff reinvented the Wheatstone bridge.

[75] Kirchhoff 1849a. Cf. Olesko 1991: 184–7.

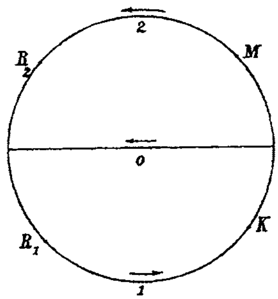

FIG. 2.5. Diagram for the measurement of the induction constant ε (Kirchhoff 1849a: 119).

$$J_2 \sim \frac{\varepsilon}{R_2} I_1 \Delta P, \quad R_2 I_2 = R_0 I_0 \sim R_0 I_1, \tag{2.7}$$

and therefore the constant ε is simply given by

$$\varepsilon = \frac{J_2 R_0}{I_2 \Delta P}. \tag{2.8}$$

After strenuous measurements and long, 'boring' calculations (for the potential), Kirchhoff won the prize competition and turned the work into his dissertation. His conclusion, published in 1849 after numerous improvements, was: 'The constant ε is equal to *one* if one takes 1000 feet per second for the unit of velocity, and, for the unit of resistance, the resistance of a copper wire with a section of one square line and a length of 0.434 Zoll.'[76]

2.5.2 Absolute resistance

During a short visit to Leipzig in October 1848, Kirchhoff found out that Weber had been working on 'the same subject.' He judged his own work 'superficial in comparison,' and almost gave up publication. Weber's results appeared in the second instalment of his *Maassbestimmungen*, published in 1850.[77] There he defined an absolute unit of resistance, as the ratio of the absolute units of electromotive force and intensity. For intensity he chose the electromagnetic unit, and for electromotive force he defined the absolute unit as that for which there is no numerical coefficient in the induction law for a moving circuit.[78] As he explained, his absolute unit of resistance was identical to what Kirchhoff called the unit for which $\varepsilon = 1$, if only

[76] Kirchhoff 1849a: 131 (boring). Kirchhoff did not take into account the self-induction of the coils. His formulas are nevertheless correct, because the time-integral of the self-induced electromotive force is zero.

[77] Kirchhoff to Neumann, 13 October 1848, quoted in Olesko 1991: 185–6; Weber 1850.

[78] More precisely: the unit of electromotive force is produced by the rotation of a conducting loop in a unit uniform magnetic force when the surface of the projection of the loop in a plane perpendicular to the magnetic force varies by one unit in a unit time.

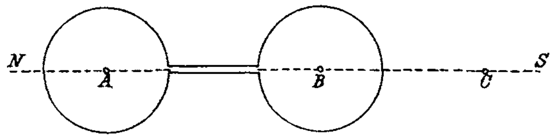

FIG. 2.6. Ideal circuit for absolute resistance measurement (Weber 1850: 220).

the unit of velocity was the same. In brief, Kirchhoff and Weber determined the same number, although their expressed aims were quite different. Kirchhoff wanted to determine an unspecified parameter of Neumann's phenomenological theory, whereas Weber sought absolute resistance measurement, a further extension of the Gaussian program.

An absolute unit of resistance was the more necessary because a unit length of copper wire with unit section did not provide a sufficiently stable standard, owing to variations in the conductivity of copper. In order to avoid this variability, Moritz von Jacobi (the mathematician's brother) had built a standard and circulated it in Germany so that local copies could be made and adjusted. Weber's aim was to immortalize Jacobi's standard by giving its measure in absolute units. He first described a simple, ideal device through which the absolute measurement of a resistance could be done. The initial configuration of the linear conductor is given on Fig. 2.6: it is made of two rigid circular loops of radius r connected by two flexible, parallel wires, the whole being included in a vertical plane. NS represents the direction of the magnetism of the Earth. A suspended magnetic needle is placed at C, at a large distance d of B. With this device two measurements are made. First, the oscillation period τ of the magnetic needle is determined. Second, the conducting circle centered in A is suddenly rotated to a position perpendicular to the direction NS. During this operation, a brief current is induced in the circuit, which acts on the magnetic needle through the loop centered on B. The maximum deviation α of the needle is measured. A simple calculation shows that the absolute resistance is given by $\pi^3 r^4/\alpha d^3 \tau$. In practice, Weber increased the induction by using coils instead of circles, and by placing the needle at the center of the second coil. In order to determine the resistance of Jacobi's standard, he inserted a copy of it in the circuit, measured the total resistance, and subtracted the resistance of the original circuit.[79]

Weber compared his result with Kirchhoff's, and found satisfactory agreement, considering the uncertainty in the conductivity of Kirchhoff's copper. He also emphasized the radical difference of their procedures. The contrast is evident in the drawings provided by the two physicists (Fig. 2.5 and Fig. 2.6). Kirchhoff's drawing is a diagram for a conducting network, and concretizes the algebraic extraction of the constant ε. Weber's drawing represents the actual geometry of the device, which is essential for the reduction to the fundamental units of space and time. Another Gaussian feature of Weber's device is the exploitation of the magnetism of the Earth, whereas Kirchhoff's device is purely electrodynamic, which implies difficult calcu-

[79] Weber 1850: 199–202; 220; 218–52.

lations for the electrodynamic potential. The chronically hypochondriac Kirchhoff believed Weber's method to be superior to his own, especially because his characterization of a resistance depended on the not-so-constant resistivity of copper. Yet both methods had sophistication and elegance. They embodied, each in its own manner, reciprocal relations between theory and precision measurement.[80]

2.5.3 Deriving Ohm's law

Kirchhoff's admiration for Gauss's and Weber's achievements had effects on his own methods. He borrowed most of his technique of galvanometric measurement from them. More important, he shared Neumann's interest in the unifying power of Weber's theory. During his visit to Leipzig, he agreed with Weber that one should try to replace Ohm's considerations 'with others that link up more closely to the rest of the theory of electricity.' A year later, in 1849, he used Weber's theory to derive and correct Ohm's laws of electric conduction. Ohm had reasoned by analogy with Fourier's theory of heat propagation and assumed that the electromotive force at a given point of a conductor was proportional to the gradient of the 'electroscopic force' or 'tension,' which he identified with the charge density. This law, Kirchhoff noted, correctly gave the distribution of currents in a conductor, for which the true nature of tension was irrelevant. It was, however, incompatible with electrostatics, for it implied that uniformly bodily charged conductors should be in equilibrium. Weber's theory, Kirchhoff went on, implied an alternative formulation of Ohm's law for conductors at rest and stationary currents. In this formulation, the current density is proportional to the gradient of the electrostatic potential.[81]

This law can be split into two partial laws. First, the current density must be proportional to the electromotive force defined as the mechanical force acting on a unit of positive electricity. In 1846 Weber assumed this proportionality without detailed mechanism. He only imagined a series of decompositions and recompositions of the neutral fluid that occurred at a rate proportional to the separating force. The picture was insufficient when free electricity was also present in the conductor. Nevertheless, Kirchhoff assumed that the current was still proportional to the electromotive force in this case.[82]

According to the second partial law, the electromotive force in a given volume element is equal to the electrostatic force impressed on a unit of positive fluid in this volume. This law is not an obvious consequence of Weber's law, since the motion of the electricity in the volume element in principle implies a contribution of the velocity-dependent part of Weber's forces. Kirchhoff argued that the conduction mechanism could be such that the electric fluids were almost always at rest, except for sudden jumps from one molecule to the next. His general strategy was plain: he

[80] Weber 1850: 252–255; Kirchhoff 1849a: 118; Kirchhoff to Neumann, 13 October 1848, quoted in Olesko 1991: 185–186.

[81] Kirchhoff to Neumann, 13 October 1848, quoted in Jungnickel and McCormmach 1986, Vol. 1: 155; Kirchhoff 1849b: 49, 52. Cf. Archibald 1988. On Ohm's theory, cf. also Jungnickel and McCormmach 1986, Vol. 1: 53–55.

[82] Kirchhoff 1849a: 52.

used Weber's hypothetical theory to guess at phenomenological relations that did not depend on the details of the conduction mechanism. More exactly, he always assumed that microscopic details were such that the phenomenological laws were the simplest possible.[83]

Not surprisingly, Weber dealt with the same problem at the same time. Like Kirchhoff, he recognized the incompatibility of Ohm's law with the laws of electrostatics, and he made electrostatic forces the cause of the current wherever the impressed electromotive force (from a battery, or by induction) vanished. However, he did not give the general law of conduction, and instead showed in particular cases that a surface distribution of free electricity could maintain the continuity of the current. In his opinion, great mathematical difficulties barred the way to such a general law. When he became aware of Kirchhoff's theory, he recognized its greater generality, but seems to have regretted the underlying simplifications.[84]

For Weber, there could be no final theory of conduction without a previous understanding of the causes of electric resistance. In his lengthy speculations on this matter, he favored the idea that the resistance originated in the interaction between opposed fluid particles according to his fundamental law. As an imperfect illustration, he considered a linear sequence of positive particles and the motion of a negative particle along this sequence. Initially, the negative particle orbits a given positive particle. In the presence of a constant electromotive force, the orbit is gradually deformed until it reaches the sphere of action of the next positive particle and starts overlapping it. Weber thus expected a regular jumping of the negative particle from one positive site to the next, in conformity with the constant (average) velocity of the electric fluids under a given electromotive force.[85]

Weber was aware of the sketchy character of such considerations. Nonetheless, he valued them highly, for they 'led into yet virgin fields of science' dealing with the 'nature of bodies.' In contrast, Kirchhoff's microphysical assumptions were nothing but a ladder leading to helpful phenomenological laws, a ladder that could be put aside after use.[86]

2.5.4 *On the motion of electricity*

Toward the end of his memoir on Ohm's law, Kirchhoff noted that in general Weber's law, not the law of electrostatics, should be taken as the basis for the motion of electricity in conductors. This generalization was indispensable in the case of moving conductors or variable currents, for which electromagnetic induction occurred. However, Kirchhoff overestimated the difficulty of this problem, and did not tackle it until 1857. In that year he first studied the ideal case of an infinitely thin conductor.[87]

[83] Kirchhoff 1849a: 55.
[84] Weber 1850: 270–93; *ibid.*: 293–5 for the comments on Kirchhoff.
[85] Weber 1850: 304–10. The modern reader may be worried that a periodic trapping of the moving charge is not compatible with the energy increase due to the electromotive force. Weber was not. And he did not consider the Joule heat.
[86] Weber 1850: 305. [87] Kirchhoff 1849b: 54–5; 1857a. Cf. Whittaker 1951: 230–2.

By integration of Weber's induction formula for variable current elements, Kirchhoff found that the electromotive force at a point of the wire included, in addition to the electrostatic contribution, a term proportional to the time derivative of the intensity at the same point. Combining the resulting equation, Ohm's law, and the conservation of electricity, he showed that the intensity obeyed a wave equation with a damping term proportional to the resistance: 'a very remarkable analogy between the propagation of electricity in a wire and the propagation of a wave in a tense string.' The propagation velocity did not depend on the nature of the wire; it was always equal to $C/\sqrt{2}$, where C is the constant in Weber's law. Using the value of C provided by Weber and Kohlrausch the preceding year, Kirchhoff found the propagation velocity to be 'very close to the velocity of light in empty space.' Yet he did not venture any opinion on the cause of this extraordinary coincidence.[88]

In a subsequent memoir, Kirchhoff treated the general case of three-dimensional conductors. On the basis of Weber's law he derived the general expression of the electromotive force, which is, in anachronistic notation (see Appendix 4):

$$\mathbf{E} = -\nabla\phi - \frac{1}{c^2}\frac{\partial \mathbf{A}}{\partial t} \tag{2.9}$$

where ϕ is the electrostatic potential, c the constant $C/\sqrt{2}$, and $\mathbf{A}$ a vector depending on the current density $\mathbf{j}$ as

$$\mathbf{A}(\mathbf{r}) = \int d\tau' \frac{\mathbf{r}-\mathbf{r}'}{|\mathbf{r}-\mathbf{r}'|^3}\, \mathbf{j}(\mathbf{r}')\cdot(\mathbf{r}-\mathbf{r}'). \tag{2.10}$$

This was the first occurrence of the vector potential in continental electrodynamics. The concept came out naturally as soon as a vectorial electromotive force was considered. Previous mathematical studies of induction could do without it because they were confined to linear conductors.[89]

Kirchhoff gave the whole set of equations for the (macroscopic) motion of electricity without explicit derivation, and with hardly any comment on the underlying assumptions. He only gave a sufficient condition for extending Ohm's law to non-stationary currents:

> This assumption will be satisfied if the forces which act on the particles of electricity and are responsible for the resistance are so intense that the time during which a particle of electricity remains in inertial motion after the interruption of accelerating forces can be regarded as infinitely small, even compared to the small characteristic times of non-stationary currents.

Weber later proved the assumption to be impossible for an infinitely thin wire. He also unearthed Kirchhoff's other implicit assumptions: that the amount of free elec-

[88] Kirchhoff 1857a: 146, 147. Cf. Jungnickel and McCormmach 1986, Vol. 1: 296–7; Rosenfeld 1956: 1635, 1640.

[89] Kirchhoff 1857b. Cf. Whittaker 1951: 232–3. Kirchhoff's potential vector differs from our vector potential (Maxwell's) by a gradient, as Helmholtz later proved: cf. Appendices 4 and 7.

tricity was negligible compared with the amount of neutral fluid, and that the current of negative electricity was everywhere equal to the current of positive electricity.[90]

Again, Weber had been working on the same topic as Kirchhoff at the same time and with different aims and methods. Whereas Kirchhoff's study was purely theoretical, Weber sought results that could be checked experimentally. He therefore derived the equations of motion of electricity in a simple particular case, that of a circular wire, but with a finite section of the wire and a finite mass of the electric fluids. He found that the effects of the finite mass could not be neglected for a vanishing section of the wire. Among these effects was a dependency of the propagation velocity on the length of the circuit. Consequently, the propagation velocity of electricity was not well defined. Weber instead talked about a modification of Ohm's law for thin wires and high frequencies.[91]

This state of affairs conditioned Weber's attitude with respect to the numerical coincidence of Kirchhoff's velocity with the velocity of light:

> If this close agreement of the propagation velocity of electric waves with the velocity of light could be regarded as an indication of a deep connection between the two sciences, then this agreement would captivate our attention, considering the high importance of the search for such a connection. However, it is obvious that the true meaning of this velocity [Kirchhoff's] with respect to electricity must be considered, and this meaning is not of a kind that would allow great expectations.

Unlike Kirchhoff, Weber was willing to risk hypotheses on the electrical nature of the optical ether. For example, late in his life he identified the ether with a lattice of imponderable positive particles. But he did not think that Kirchhoff's pseudo-propagation velocity could help in such speculations.[92]

On the experimental side, Weber judged this velocity to be beyond reach, and rejected a prior estimate by Charles Wheatstone as meaningless. Instead he wanted to verify a surprising consequence of the theory: that for a periodic excitation of a large circuit the intensity and the phase of the current were uniform, even if the source was localized around one point of the circuit. In 1857 he started measurements of this kind with Rudolph Kohlrausch. The exciting source was a small rotating magnet, and the amplitude and the phase of the current at a given point of the circuit were measured with an electrodynamometer. Unfortunately, Kohlrausch died and Weber did not obtain publishable results until 1864. Weber also hoped to determine the inertia of electricity by comparing the current amplitudes in a circuit made of two superposed rings, with parallel currents in one case, and opposing currents in the other, but he never made the necessary measurements.[93]

[90] Kirchhoff 1857b: 137. Weber 1863: 98; 1864: 114.

[91] Weber 1863: 102; 1864: 126–31.

[92] Weber 1864: 157; Weber [>1880]: 524–5.

[93] Weber 1863: 100 (on Wheatstone); 1864: 183–234 (uniformity), 226 (history), 235–41 (effect of inertia). From the modern point of view, the uniformity of the current results from the extreme smallness of retardation effects for the frequencies used by Weber and Kohlrausch. The idea underlying the two-ring experiment is that the effect of inertia on the amplitude is the same in the parallel and antiparallel configurations, whereas the effect of self-induction is different. Hertz later performed this kind of experiment: cf. Buchwald 1994: 59–74.

In sum, Weber took the shortest path between his fundamental law and specific measurements. He calculated the consequences of the law for simple but realistic setups, and performed the measurements, hoping to confirm every aspect of his law and related microphysical assumptions. In contrast, Kirchhoff constructed a phenomenological theory that mediated between Weber's microphysics and experiments. In Kirchhoff's view, Weber's microphysical theory helped construct the phenomenological theory, but the latter was not enslaved to the former. Simplified, general phenomenological relations could be truer than the exact, partial relations derived from Weber's theory.

2.6 Conclusions

Neumann and Weber widely extended the quantification of electrodynamics, and thus started two important traditions of German physics. On the experimental side, they focused on precision measurement, whereas Faraday and Ampère rarely measured quantities. On the theoretical side, they aimed at complete mathematical theories of electrodynamics. Inspired by two outstanding astronomers, Bessel and Gauss, they brought astronomical precision to the new field of electrodynamics.

Neumann's and Weber's efforts were largely independent, and their methods differed widely. Neumann's measurements were repetitive and rigid, they relied on already known devices, and they required long, exacting error analysis. Weber's measurements were constantly innovative and yielded new instruments, and Weber believed that dexterous construction and manipulation of apparatus eliminated the need for error analysis.[94] Neumann usually measured constants, whereas Weber 'defined' phenomena (produced them in quantitatively controllable circumstances) and verified their laws. The Gaussian requirement of absolute units was largely responsible for Weber's originality: it implied new kinds of instruments with simple geometry and high sensitivity, and required the verification of the laws according to which these instruments were analyzed. Although Neumann's disciples sometimes conceived new measuring setups, they did not really invent new instruments. They combined existing instruments as symbols are combined in an equation. Thus one might say that Neumannian measurement was algebraic and Weberian measurement was geometric.

In his theoretical works, Neumann focused on observable quantities, although he tolerated the more abstract concept of potential. He did not require more unity than could be expected on experimental grounds. His physics was fragmented and pessimistic. But it was very solid, yielded durable phenomenological laws, and defined mathematical terms and structures for future theories. Weber's strategy was almost

[94] Cf. Olesko 1991: 410–11.

the opposite. In harmony with the ethos of absolute measurement, he sought a global, geometrico-mechanical unity of physics, which he reached both instrumentally and theoretically. His theory was based on a mechanical force law and a hypothesis on the nature of electric currents. On the one hand, it lent itself to microphysical speculation. On the other, it suggested new phenomenological laws. Weber excelled in the first case. He was more restrained in the latter, because he believed that only the micro-world could obey simple universal laws.

Neumann's theory was incomplete, for it depended on the limitation of known empirical laws to linear, closed currents. Weber's theory was in principle complete, since its fundamental law was assumed to be valid for all kinds of electric phenomena. However, it was not so in practice. The fundamental law and the general interpretation of currents in terms of the motion of electric fluids were only sufficient to derive Ampère's electrodynamic forces and the laws of electromagnetic induction. Other phenomena, for instance electric conduction or magnetism, required additional microscopic assumptions.

Kirchhoff managed to complete both theories, Neumann's and Weber's, by combining their virtues. From Neumann (also Fourier and Gauss), he retained the trust in the mathematical simplicity of macroscopic laws and the mathematical techniques of partial differential equations and integral calculus. From Weber, he borrowed the idea that the unifying principle of electric phenomena was microphysical. His strategy was to derive macroscopic consequences of Weber's law and to adjust the picture of the currents so that these consequences were the simplest possible. He thus reached general differential equations from which the motion of electricity could be derived in any conductor and under any external circumstance, without further reference to the microphysical level. To him Weber's theory was only a springboard to the phenomenological level.

Kirchhoff's reliance on Weber's theory, and the ease with which Neumann and Weber accepted each other's theories, suggest that despite all the differences Königsberg physics and Leipzig physics shared common values. In fact they both made precision measurement the basis of physics, and they both confined experimentation to quantitative measurements with computable instruments that yielded numerical constants (for Neumann) or quantitative laws (for Weber). Consequently, their adepts reduced the exploratory value of experiment to a minimum, and they hardly discovered any new effects.[95] On the theoretical side, they both depended on neo-Newtonian and Amperean notions. This is quite obvious in Weber's case, since his fundamental law expressed a direct action between two particles. This is also true for Neumann's and Kirchhoff's theories, though in a more recondite form. Being intimately related to Ampère's law and in part deducible from Weber's theory, they involved direct action at a distance. Although they did not express this action in terms of mechanical forces, the replacing concept of electrodynamic potential was similar to the potential already introduced in gravitation theory. The French theories

[95] For an interesting exception, cf. Dörries 1991.

of electricity and magnetism had all been neo-Newtonian. Therefore subsequent mathematical theories in the same field were not likely to depart much from the Newtonian scheme of interaction. The exceptions form the subject of the next chapter.

3

British fields

3.1 Introduction

In the first two decades of the nineteenth century, opinions on the nature of electric and magnetic actions still varied, especially in England and in Germany. There even were traces of the eighteenth century 'atmospheres,' invented to avoid direct action at a ditance. After Ampère's electrodynamics and its German extensions, however, the hegemony of Newtonian fluid theories spread from France to other countries. The penetration of the methods of French mathematical physics favored the most readily quantifiable representations of phenomena, and diverted attention from more qualitative notions.[1]

A few British physicists escaped this general evolution, and proposed alternative views of electricity and magnetism. First and foremost was Faraday, who preserved his intellectual independence and his ignorance of mathematical theories. He sought a more immediate connectivity of physical objects and phenomena, and kept exploring the space intervening between electric and magnetic sources. In the 1830s and 1840s, he accumulated major discoveries including electromagnetic induction, electrochemical equivalence, inductive capacity (dielectrics), magneto-optical rotation, and diamagnetism. At the same time he developed the field conception of electric and magnetic actions.

In the 1840s William Thomson revealed analogies between the mathematical laws of electricity and magnetism and the dynamics of continuous media. He thus invented the basic concepts of field mathematics, and pointed to a surprising equivalence between Faraday's reasonings on lines of force and potential theory *à la française*. After 1850, Thomson recurrently speculated on a dynamical ether theory of electricity and magnetism. In most of his early works, however, he avoided commitment on the deeper nature of electricity and magnetism. His concepts and apparatus could be shared by any users of these sciences, be they physicists of various schools, chemists, mathematicians, or engineers. They were immensely successful in their cross-cultural purpose, and provided a lasting foundation for future research on electricity and magnetism. With his analogies,

[1] For pre-1820 field theories, cf. Heilbron 1981.

mechanical models, and energetics Thomson championed a new kind of British physics.

The present chapter is devoted to Faraday's and Thomson's field physics, to their connections and divergences. 'Field' is here used in a loose, meta-historical sense, meaning the introduction of physical or mathematical entities in the space intervening between electric and magnetic sources. With this definition, questions about the exact origin of the field concept or the relative importance of Thomson's and Faraday's contributions become largely meaningless. Field notions in this sense already existed in the eighteenth century. Faraday's lines of force, however, provided the first precise and quantitative concept of a field. Moreover, Faraday advocated a *pure* field theory, in which electric charge and current were derivative concepts. Thomson was first to introduce mathematical field concepts, and to seek their foundation on a dynamical ether theory.[2]

To a large extent, the concepts used today in common applications of electricity are Thomson's invention. This fact eases access to his writings, but it tends to obscure Thomson's merits, which are explained in the two last sections of this chapter. In contrast, Faraday's views are difficult to penetrate, because they differ from modern ones at a very basic level. Yet they provided essential components of the system cultivated by Maxwell and his followers. This is why they are given detailed consideration in the three first sections of this chapter.

3.2 Faraday's electrochemistry

3.2.1 Wet string

In his researches on electromagnetic induction, Faraday repeatedly sought electric effects of the induced currents: spark, action on the tongue, electrolytic decomposion, and heating. He also tried to induce currents by discharging a Leyden jar in the primary of a double coil. Although his attempts met little success, he did not doubt that all forms of electricity were equivalent. Most natural philosophers had shared this opinion since Volta, and Wollaston had supported it with experimental facts. Yet doubts were occasionally expressed. For example, Davy questioned the identity of animal electricity with friction or voltaic electricity. At the end of his series on electromagnetic induction, Faraday judged that a complete, systematic proof of the equivalence of all kinds of electricity was still lacking, and he set himself to fill the gaps.[3]

Trying to improve on Wollaston's proof of electrolytic effects for friction electricity, Faraday hit upon the following device (Fig. 3.1(a)). He moistened a piece of paper with a saline or acid solution and a colour indicator, laid it on a glass plate, and let the ends of two platinum wires rest on it. The first wire was connected to an electrostatic machine through a wet string and a switch made with another glass plate and a tin foil. The second wire led to 'a discharging train,' that is, grounding

[2] On the ambiguities of the field concept and its origins, cf. Nersessian 1985.
[3] *FER* 1: 6; Wollaston 1801; Davy 1829: 17. Cf. *FER* 3, series 3: 76–7; Williams 1965: 211–23.

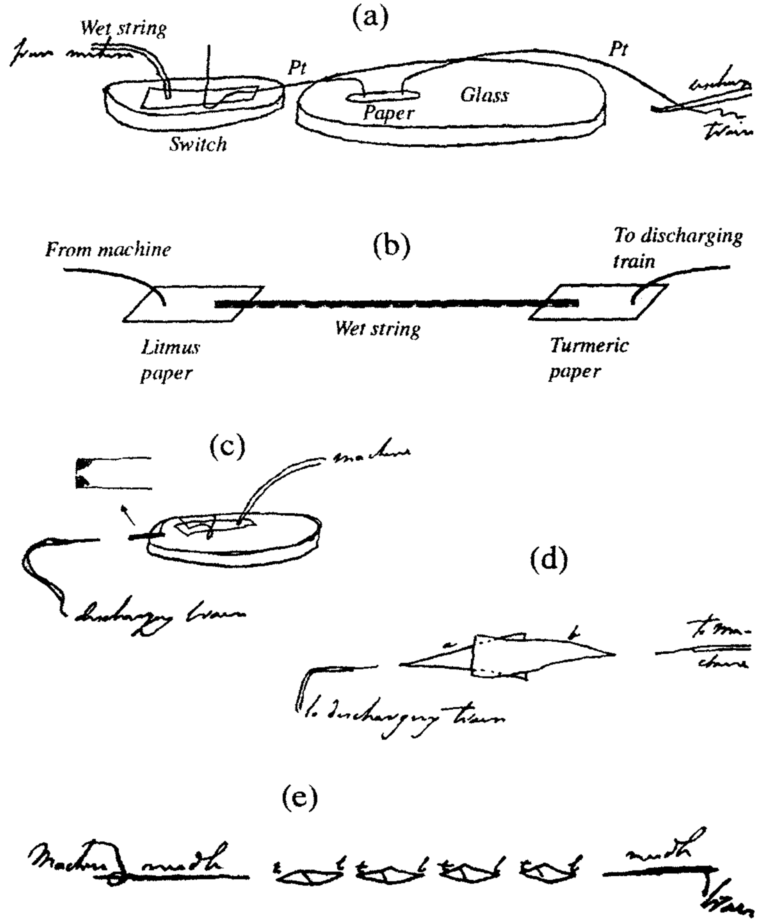

FIG. 3.1. Faraday's experiments on electrolytic decomposition: (a) with two platinum poles on paper (*FD* 2: 9), (b) with remote poles, (c) without pole at the corners of a turmeric paper (*FD* 2: 17), (d) without poles in turmeric and litmus paper, (e) without poles in a chain of turmeric–litmus papers.

by gas or water pipes. A few turns of the electrostatic machine sufficed to produce chemical decomposition at the wire ends. Faraday praised the virtues of paper: 'It makes contact by very minute surface, keeps the decomposed matter on the spot, and by its whiteness well shews the effects of change of color.' This was the starting point of a revealing series of mutations.[4]

The role of the wet string was to 'retard' the electricity by its bad conducting power and thus make it more akin to galvanic electricity. Eventually, Faraday found

[4] *FD* 2: ##46–55 (1 September 1832), quote from #51. Cf. Williams 1965: 220–3.

that the electrolytic effect was just as good without the wet string. He soon exploited another virtue of this cheap resource, that of being a non-metallic conductor. He took two pieces of paper, moistened them with a solution of soda sulphate and colored acid/alkali indicators (litmus/turmeric), and connected them with four feet of the string (Fig. 3.1(b)). The positive platinum pole, resting on the litmus paper, produced acid, and the negative one, resting on the turmeric paper, produced alkali. Faraday found that the effect was about the same as when the two papers where in direct contact. This showed that the decomposition did not depend on the distance between the two metallic poles.[5]

A moment later, Faraday placed the positive platinum pole on a piece of litmus paper, and touched the paper with a wet string connected to the discharge train. Decomposition occurred, despite the lack of a true negative pole. Two days later Faraday further modified his device so as to determine where the alkali produced in the decomposition went. He now used turmeric paper, and replaced the wet string with a metallic point about two inches from the end of the paper (Fig. 3.1(c)), exploiting the conducting power of air near charged metal points. After a few turns of the machine, the corners of the paper turned brown, thus showing the accumulation of alkali. Faraday commented:[6]

> Hence it would seem that it is not a mere repulsion of the alkali and attraction of the acid by the positive pole, etc. etc. etc., but that the current of electricity passes, whether by metallic poles or not, the elementary particles arrange themselves and that the alkali goes as far as it can with the current in one direction and the acid in the other. The metallic poles used appear to be mere terminations of the decomposable substance.

After two more days, Faraday found a better illustration of this view by combining turmeric paper, litmus paper, and two distant metal points (Fig. 3.1(d)). Finally, in April 1833 he provided a visualization of the imagined internal decomposition of electrolytes by means of an alignment of pairs of indicating papers (Fig. 3.1(e)). This case of decomposition, he commented, 'indicates at once an internal action of the parts suffering decomposition, and appears to show that the power which is effectual in separating the elements is exerted there and not at the pole.' This arrangement was similar to the iron files between the poles of magnets: in both cases Faraday took the effects observed in the intervening space to reveal the distribution of power in this space.[7]

3.2.2 *The laws of electrolysis*

In May 1833 Faraday sought independent confirmation of the distributed decomposing power by passing a beam of polarized light through a conducting solution. This failed. However, a few days earlier Faraday had serendipitously discovered

[5] *FER* 1: #295 (role of wet string); *FD* 2: #57 (1 September); #72 (3 September). Faraday also remarked that in most cases of sparking discharge, the reaction at the two poles was the same.

[6] *FD* 2: #74 (3 September), #81 (4 September); #99, #103 (quote, 6 September). Faraday later confirmed this result with another device in which water played the role of the negative pole: *FD* 2: #577 (30 May 33).

[7] *FD* 2: #108 (8 September); #469 (22 April 33); *FER* 1, series 5: #471 (quote).

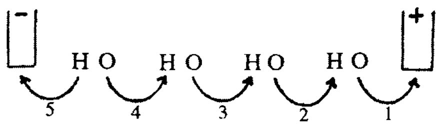

FIG. 3.2. A Grotthus chain in water (H stands for a hydrogen particle, O for an oxygen particle).

another indication that decomposition was essential to electrolytic conduction. He wanted to use ice as a non-metallic conductor in place of the wet string or the air around a metal point. But the ice would not conduct at all. In general, Faraday found that liquid electrolytes lost their conductivity when frozen. He hurried to publish this 'new law of electric conduction,' which 'afforded abundant compensation for [his] momentary disappointment.' And he queried 'whether solidification does not prevent conduction merely by chaining the particles to their places, under the influence of aggregation, and preventing their final separation in the manner necessary for decomposition?'[8]

For Faraday, electrolytic decomposition occurred within the whole substance of the electrolyte. That the products of decomposition were only seen at the ends of the substance only meant that recomposition occurred at the same rate within the substance. This view agreed with the chain process imagined by Grotthus (1806) and Davy (1807), according to which a series of decompositions and recompositions took place on lines joining the ends of the substance (Fig. 3.2). But it contradicted the more recent views of Biot (1824) and Auguste de la Rive (1825), according to which separation of the elements occurred only at the poles and was followed by migration of the charged particles of the elements. Faraday further denied that the attraction or repulsion from the poles caused the decomposition process. In his view the decomposition of a particle of the substance occurred as a consequence of the decomposition of the neighboring particle. This contradicted Grotthus's idea of a direct action of the poles on the particles of the separating elements, and also Davy's more complex idea of an action of the poles communicated by the intervening particles.[9]

From the intimate connection between electrolytic conduction and decomposition, Faraday drew two essential conclusions:[10]

1. 'The *sum of chemical decomposition is constant* for any section taken across a decomposing conductor, uniform in its nature, at whatever distance the poles may be from each other or from the section.'

[8] *FD* 2: ##482–94 (2 May 1833); ##222–49 (23–4 January 1833); *FER* 1, series 4: 110 (quote), 118 (quote).

[9] Grotthus 1806; Davy 1807; Biot 1824, Vol. 1: 636–42; A. de la Rive 1825. According to Davy's view, there should be no decomposing action in the middle of the solution. I have followed Faraday's reading of these texts: *FER* 1: 136–9. See also Ostwald 1896; Whittaker 1951: 75–7; Williams 1965: 227–1.

[10] *FER* 1, series 5 (June 1833): ##504–5. See also the weaker statement of series 7, *FER* 1: #377: '*The chemical power, like the magnetic force is in direct proportion to the quantity of electricity* which passes.' Cf. Williams 1965: 241–57.

2. *'For a constant quantity of electricity, whatever the decomposing conductor may be, whether water, saline solutions, acids, fused bodies, &c., the amount of electro-chemical action is also a constant quantity, i.e. would always be equivalent to a standard chemical effect founded upon ordinary chemical affinity.'*

The latter is what we call Faraday's law. Faraday proved it in May, August, and September 1833 by means of series of electrolytic cells of various kinds. He constructed tables of 'electro-chemical equivalents,' and described 'the only *actual measurer* of voltaic electricity,' the 'Volta-electrometer' (to become 'voltameter') based on the electrochemical decomposition of water.[11]

Faraday was especially interested in the implications for the nature of chemical forces. That the electrochemical equivalents coincided with the chemical ones seemed to confirm Davy's and Berzelius's electric conception of chemical affinity. In Faraday's words:

> I think I cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts more directly and closely than any former fact, or set of facts, have done, upon the beautiful idea, that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter.

In short: 'ELECTRICITY *determines* the equivalent number, *because* it determines the combining force.'[12]

3.2.3 *Redefining the electric current*

Why should the electric nature of chemical combinations imply that they involve a definite quantity of electricity? Faraday was aware of a possible atomistic answer: 'If we adopt the atomic theory or phraseology,' he wrote, 'then the atoms of bodies which are equivalent to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them.' He preferred, however, to avoid this speculation: 'I must confess I am jealous of the term *atom*, for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.'[13]

His own explanation of electrochemical equivalence rested on a redefinition of the electric current. As we saw in Chapter 1, he had doubted the existence of electric fluids since his earliest interest in electricity. In his work on the identity of the different forms of electricity, he took an agnostic stance: 'By current, I mean anything progressive, whether it be a fluid of electricity, or two fluids in opposite directions, or merely vibration, or speaking still more generally, progressive forces.' In his series on electrochemical decomposition, he made a more definite choice:[14]

[11] *FER* 1: #739; #1355 (voltameter).

[12] *FER* 1: #248, #256 (Faraday's emphasis). Also *FD* 2: #1917 (5 August 1834): 'The electricities appear to be the forces of attraction by which two particles combine.'

[13] *FER* 1, series 7: #869.

[14] *FER* 1, series 3: #283; series 5: #517.

Judging from the facts only, there is not as yet the slightest reasons for considering the influence which is present in what we call the electric current,—whether in metal or fused bodies or humid conductors, or even in air, flame, and rarefied elastic media,—as a compound or complicated influence. It has never been resolved into simpler or elementary influences, and may perhaps best be conceived of as *an axis of power having contrary forces, exactly equal in amount, in contrary directions*.

Faraday meant, like Oersted, that the current consisted of the propagation of a polar state in the conductor. In the case of electrolytic conduction, the polar state corresponds to the brink of decomposition; it propagates by a series of decompositions–recompositions.[15] In brief, the current *is* the series of decompositions and recompositions: 'I have no idea,' Faraday wrote to a friend, 'that in what we call the current in the decomposition of bodies anything but a resolution and recombination of forces occurs between contiguous particles.' Therefore, the strengh of the current determines the amount of decomposition, in conformity with Faraday's law. In other cases of conduction, such as metallic conduction, decomposition could not be involved. Yet Faraday suspected that a similar mechanism occurred. In his study of self-induction, published in December 1834, he proposed that any electric current involved a recurring state of temporary excitation, which could well be the electro-tonic state. Conduction occurred by 'vibrations, or by any other mode in which opposite forces are successively and rapidly excited and neutralized.'[16]

Faraday judged his concept of the electric current to be incompatible with the usual electrochemical terminology. Phrases like 'the positive pole' were impregnated with the fluid concept of electricity, and referred to electrostatic action at a distance. Faraday wanted to replace them with more neutral terms. To the inventor of the word 'physicist,' Reverent William Whewell, he wrote for consultation:[17]

'The ideas of a current especially of *one* current is a very clumsy and hypothetical view of the state of electrical forces under the circumstances. The ideas of *two* currents seems to me still more suspicious and I have little doubt that the present view of electric current and the notions by which we try to conceive of them will soon pass away and I want therefore names [. . .] without involving any theory of the nature of electricity.'

After a friendly exchange, the two men agreed on a new vocabulary for electrochemical decomposition: 'electrode,' 'anode,' and 'cathode' for the terminations of the decomposing matter, 'electrolysis' for the decomposition itself, 'electrolyte' for a substance directly decomposed by the electric current, 'ion,' 'anion,' and 'cation' for the bodies passing to the electrodes. Note that 'anode' and 'cathode' did not merely replace the positive and negative poles of the older terminology; they included cases of decomposition without metallic pole. For example, in the experiment of Fig. 3.1(c) with the turmeric paper placed at a distance of a discharging negative needle, the cathode was the end of the paper facing the needle. Faraday's

[15] *FER* 1, series 5: ##519–20.

[16] Faraday to Lemon, 25 April 1834, *CMF* 2; *FD* 2: #1167 (2 December 1833): 'Consider the transmission of electricity: that there are three modes as in a metal wire, in decomposing fluids, through air, vapour, etc. as spark or brush. Are not these all one?'; *FER* 1, series 9: #1115.

[17] Faraday to Whewell, 24 April 1834, *CMF* 2. Cf. Williams 1965: 257–67.

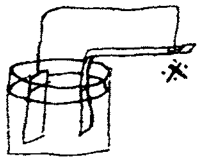

FIG. 3.3. Decomposition of 'hydriodate of potassa' (KI) at X, with no metal–metal contact (*FD* 2: 244). The straight electrode and wire are made of platinum; the bent electrode of amalgamated zinc. Both are immersed in a sulfonitric solution.

definition thus reflected the elimination of attractions and repulsions in favor of decomposition. The ethymology did the same: Whewell forged 'anode' and 'cathode' from the Greek for 'upwards way' and 'downwards way,' alluding to the fact that currents around the Earth's axis would agree with its magnetism if they followed the motion of the Sun. The polarity of the current was thus defined without reference to positive or negative electricity.[18]

3.2.4 *To the trough*

Faraday completed his study of electrolysis in late 1833. In February 1834 he decided 'to go to the trough.' In the electric conception of chemical affinity, a voltaic cell was just an electrolytic cell working backwards, the chemical decomposition now being the cause of the current. Accordingly, Faraday supported Davy's chemical theory of the pile and rejected the more popular contact theory. He showed that the electromotive force of a cell largely depended on the electrolyte, and gave numerous examples of decomposition without the contact of two metals, the first being the decomposition of potassium iodide in the circuit zinc/sulfonitric mixture/platinum/potassium iodide solution/zinc (Fig. 3.3). The argument, however, failed to convince the German and Italian supporters of the contact theory. The latter theory could be generalized to include contact tension between metals and non-metals.[19]

Five years later Faraday accepted the objections of some of his adversaries, especially Stefano Marianini's. He resumed his studies of the voltaic trough, multiplied facts in favor of the chemical theory, and denounced a fundamental defect of the contact theory: it denied 'the great principle in natural philosophy that cause and effect are equal.' If the contact of two metals was the source of the voltaic effect, then one would have a 'production of power without a corresponding exhaustion of

[18] Whewell to Faraday, 25 April, 5 May 1834, *CMF* 2; Faraday to Whewell, 3 May 1834, *ibid.*; *FER* 1, series 7: #661–2.

[19] *FD* 2: #1487 (10 February 1834); #1577 (19 February 1834); *FER* 1, series 8. On the reception, cf. *FER* 1: #1769 and Ostwald 1896, Vol. 1: 476–80 for the leading German supporter of the contact theory, C. Pfaff, and Vol. 2: 693–701 for other Germans.

something to supply it.'[20] In the 1850s, after energy considerations had become central to physics, this argument certainly weakened the positions of the contact theorists. It failed, however, to induce any spectacular conversion to Faraday's views. On the contrary, the British herald of energy physics, William Thomson, restored a form of contact theory.[21]

Most electrochemists admitted voltaic cells without metal–metal contact and electrolysis without metal electrodes. They adopted Faraday's terminology, and they applauded his demonstration of electrochemical equivalents. Yet they did not easily abandon the contact theory, and they completely ignored Faraday's concept of the electric current. Those who wished to explain the law of electrochemical equivalents did it with atoms and electric fluids. No one took seriously Faraday's claim that his views were the mere expression of facts. His exclusion of alternative theories indeed depended on personal judgments: to him electric fluids were unphilosophical, atomistic explanations were too arbitrary, and no acceptable theory could be based on principles whose consequences could only be foreseen mathematically.[22]

3.3 Dielectrics

3.3.1 Redefining electric charge

After founding a new electrochemistry and suggesting a new view of the electric current, Faraday still did not know how to define electricity. In April 1834, he confided to a friend: 'At present my view is very unsettled with regard to the nature of the electric agent. The usual notions attached to Positive and Negative and to the term current I suspect altogether wrong but I have not a *clear view* of what ought to be put in their places.' In the case of electrolysis, he could not distinctly see how the 'resolution and recombination of forces between contiguous particles' came about. A query of November 1835 indicates how he soon hoped to solve this problem:[23]

Have been thinking much lately of the relation of common and voltaic electricity: of [electrostatic] induction by the former and decomposition by the latter, and am quite convinced that there must be the closest connexion. Will be first needful to make out the true character of ordinary electrical phenomena.

[20] Marianini 1837; *FER* 2, series 17 (January 1840): #2069, #2071. Also *FD* 3: #5112 (26 August 1839): 'By the great argument that no power can be ever be evolved without the consumption of an equal amount of the same or some other power, there is *no creation of power*; but contact would be such a creation.' Cf. Williams 1965: 364–72. Unknown to Faraday, in 1829 Peter Roget had already noted that the contact theory implied the possibility of perpetual motion: cf. *FER* 2: 103n.

[21] Thomson 1862. Thomson had a number of followers, while Maxwell, Lodge, and Heaviside supported the chemical theory. For a penetrating study of the resulting controversy, cf. Hong 1994a. On the persistence of German contact theory, cf. Ostwald 1896, Vol. 2: 731–40.

[22] The modern electrolytic theory does not meet Faraday's criteria, for it requires electric atoms (the electrons) and explains the role of the electrodes by complex electro-kinetic calculations.

[23] Faraday to Lemon, 25 April 1834, *CMF* 2; *FD* 2: #2468 (3 November 1835), anticipated in *FD* 2: ##1846–7 (22 February 1834). Cf. Williams 1965: 287.

Faraday's intuition was that an insulator submitted to an electric source (a battery or an electrostatic machine) was in the same state of tension as electrolytes were before decomposition: 'This is the state of an electrolyte in the circuit before that traversing of the particles has taken place by which the electric force is transferred and the body conducts. It ought to be the state of the electrolyte when it is solid.' There was only one difference: in the insulator the state of tension could last as long as the source was in action, whereas in the electrolyte the tension was continually resolved by decomposition. This state of tension, Faraday speculated, could be the essence of electricity, as continual decomposition was the essence of the electrolytic current. Hence came the next query: 'Does common electricity reside upon the surface of a conductor or upon the surface of the electric [insulator] in contact with it? I think upon the electric, and must work out the results on that view,' with the comment: 'Would be a reason why all upon the surface of conductors.'[24]

According to Faraday's new intuition, the insulator under electrostatic induction was polarized: any part of it, separated in imagination, was positive on one side and negative on the other, just like the Northernness and the Southernness of the parts of a magnet. A surface *within* the polarized insulator had no net charge since the charges of its two sides mutually cancelled, but the surface of contact between the conductor and its insulator could be charged, since the conductor, by definition, could not sustain polarization. Then electric charge was just the termination of polarization; it belonged to the insulating medium, not to the conductor. The insulator now being the *locus* of electricity, Faraday called it 'the electric,' to become the 'dielectric' under Whewell's suggestion. As he later wrote, 'the great point of distinction and power (if it have any) in the theory is, the making the dielectric of essential and specific importance.'[25]

Faraday imagined three ways of testing his new view. First, experiments with hollow conductors would prove the impossibility of absolute charge: if charge derived from the beginning or ending of polarization, every charge was always related to an opposite charge. Second, any effect of the composition of the 'electric' would prove 'that the electricity is related to the electric, not to the conductor.' Third: 'Must try again [as for an electrolyte under decomposition] a very thin plate under induction and look for optical effects, i.e. detect its polarized state.'[26]

3.3.2 No absolute charge

Faraday never succeeded on the third point (the later 'Kerr effect'). But he did on the first two. On 26 November, he verified that an electrified quart pot contained no

[24] *FD* 2: #2511, #2469 (3 November 1835). Cf. also Faraday's historical remarks, *FER* 1: 362: 'As the whole effect in the electrolyte appeared to be an action of the particles thrown into a peculiar state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i.e. ordinary inductive action) never occurred except through the influence of the intervening matter.'

[25] *FD* 2: #2507–8 (3 November 1835); Whewell to Faraday, 29 December 1836, *CMF* 2; *FER* 1: 364: The dielectric, 'that substance through or across which the electric forces are acting'; *FER* 1, series 13: #1666.

[26] *FD* 2: ##2474–91; #2497; #2512. Cf. Gooding 1978.

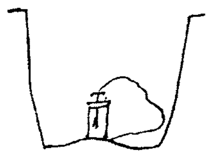

FIG. 3.4. One of Faraday's experiments with an electrified copper boiler (*FD* 2: 408). The gold-leaf electrometer at the bottom of the boiler shows no sign of electricity.

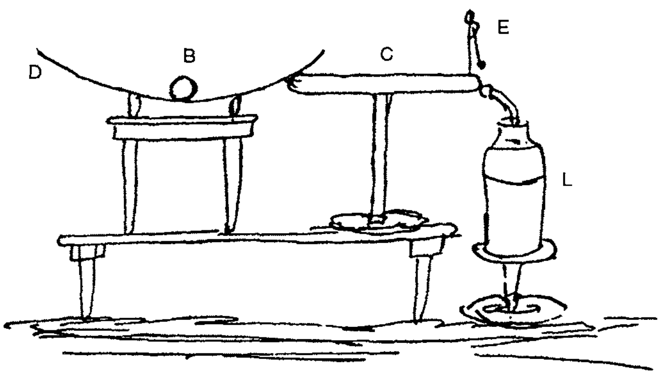

FIG. 3.5. Faraday's contrivance showing induction with no charged body in view (*FD* 2: 417). L denotes a Leyden jar, E an electrometer, C a conductor, D a metal dish, and B a carrying ball to test charge inside the dish.

electricity. In early December he performed more systematic experiments with an electrified copper boiler, electrometers, and carrying balls (Fig. 3.4). He found that the inside bottom of the boiler carried no charge, no matter how electrified the boiler globally was. In Faraday's view, this meant that a part of the surface of a conductor could be charged only if it was inductively related to another conductor. In other words, polarization starting on one conductor had to finish on another, and any charge was inductively related to an opposite charge.[27]

Yet Faraday admitted for a while that the rule could be invalidated when very large lengths of insulator were involved. On 10 December, he experimented with a spherical copper mirror (Fig. 3.5) facing the starry sky on a dry cold night and connected to a charged Leyden jar through a conductor. The bottom of the mirror had no conductor in view, and yet it carried electric charge. Faraday conjectured that the polar tension from the inner surface decayed over long distances and therefore did not need to be terminated by another charged conductor. He was then 'pretty sure' that the inside walls of a very large hollow conductor could be charged. But he soon wondered: 'Can induction through air take place in curves or round a corner? can

[27] *FD* 2: #2634 (26 November 1835); ##2664–736 (5–8 December 1835).

probably be proved experimentally. If so, is not a radiating action, and reasoning as to sky action requires modification.'[28]

In January 1836 Faraday built a twelve foot conducting cube with wood, copper wire, paper, and tin foil. He connected it to an electrostatic machine, and 'lived in it' to check the internal electric state with carrying balls and electrometers. No electricity could be found that would not be explained by the imperfect conducting power of tin foil or by induction at the entrance of the cube. Faraday concluded that induction was 'illimitable,' that there could be no global loss of power along a polarized dielectric. For example, the charge of a conductor placed in a hollow conducting sphere would always be equal to the charge of the inside walls of the sphere, no matter how large this sphere was. Consequently, absolute charge did not exist, all charge was sustained by induction and was related to another, opposite charge.[29]

3.3.3 Specific inductive capacity

Toward the end of 1836, Faraday turned to the other proof of his theory of induction: the dependency of induced charges on the nature of the dielectric. For this purpose, he built a Coulomb torsion balance according to Coulomb's own directions, with a few improvements: for instance he placed a grounded, double conducting belt on the glass cylinder surrounding the balance, 'so that the inductive action within the electrometer might be uniform in all positions.' Then he built two exemplars of a new sort of Leyden jar, in which the dielectric could be changed at pleasure. This apparatus, represented on Fig. 3.6, is made of two concentric brass spheres *aa* and *hn*. The internal sphere *hn* is connected to the conducting ball B through the brass rod *i*. The shell-lac (an excellent insulator) stem *ll* sustains the latter system and seals the collar of the external sphere. A solid or liquid dielectric can be placed in the space *oo* by opening the external sphere at the joint *b*. A gaseous one may be introduced through the stopcock *d* after proper exhaustion.[30]

The principle of Faraday's measurements was simple. He first charged the first exemplar of the apparatus with a Leyden jar and measured its 'tension' or 'degree of charge' (which I call V_0) by making a carrying ball touch the fixed ball B and measuring its resulting charge with the Coulomb electrometer. Then he brought the two exemplars into contact, and measured their resulting tensions (V_1 and V_2), which ought to be equal. Figure 3.7 gives the modern schematics of this procedure. If the two exemplars are filled with different dielectics they have different 'capacities for electric induction' C_1 and C_2. The original charge of the first apparatus, C_1V_0, is divided into C_1V_1 and C_2V_2 by contact with the second apparatus. Consequently, the

[28] *FD* 2: ##2741–6 (10 December 1835); #2766 (12 December 1835). Cf. Gooding 1978: 137.

[29] *FD* 2: ##2808–74 (15–16 January 1836); *FER* 1, series 11: #1174; *FD* 2: #2826 (15 January), 2864 (16 January); *FER* 1: #1178, #1295. Cf. Gooding 1978: 139–42.

[30] *FD* 3: ##3622–4015 (23 December 1836–7 October 1837) and ##3597–3621 (22 November to 21 December 1836) for preliminary experiments; *FER* 1, series 11: 311–409, quote from 368. Cf. Williams 1965: 291–4.

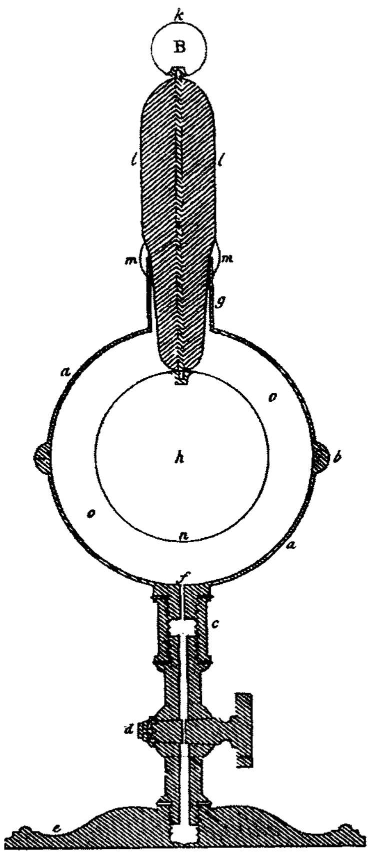

FIG. 3.6. Faraday's apparatus for studying specific inductive capacity (*FER* 1: plate 7).

ratio of the fall of tension $V_0 - V_1$ of the first apparatus to the gain of tension V_2 of the second yields the ratio C_2/C_1 of their capacities. This is how Faraday reasoned, with specific numbers instead of letters.[31]

The experiments were extremely difficult and lasted more than a year. Faraday took great precautions to avoid parasitic induction and charges. This involved long trials and ingenious tricks, for example breathing on the shell-lac and wiping it with a finger wrapped in a silk handkerchief. He had to operate very quickly in order to avoid electric leaking. Furthermore, he found that most dielectrics were not perfect insulators and that they could consequently 'absorb' electric charge. Special care had to be taken to circumvent this phenomenon. Faraday's description of his complete procedures took no less than 20 pages.[32]

[31] *FER* 1: ##1257–9. [32] *FER* 1: #1203, ##1233–1.

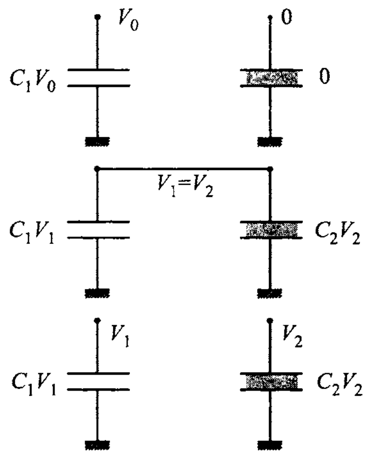

FIG. 3.7. Modern schematics of Faraday's experiments on specific inductive capacity.

For solid and liquid dielectrics, the results were beyond Faraday's expectations: the increase of inductive capacity could exceed 50 per cent. However, gases and rarefied air were a disappointment: Faraday could not find any appreciable difference in their specific inductive capacities. He nevertheless regarded induction by contiguous particles as generally proved, and took the uniform inductive capacity of gases as a further example of the simplicity of their physical properties.[33]

3.3.4 Induction in curved lines

Faraday's extreme attention to details led him to side-discoveries. Electric absorption (later called dielectric after-effect) is one of them. Another was induction in curved lines. Faraday found that parasitic charges on the shell-lac stem could charge the carrying ball when the latter was brought to touch the top of the *grounded* fixed ball B. The carrying ball being small, it was not in sight of the parasitic charges. Since B and all other nearby conductors were grounded, induction could only come from the stem. Therefore, induction had to proceed in curved lines around the ball B.[34]

In the ultimate version of this experiment, Faraday avoided direct contact between the carrying ball and the fixed ball. He grounded the latter ball, brought the carrying ball near the top, uninsulated it for a short while by means of a wire to the ground, insulated it again, and found it to be charged. This version of the experiment excluded the transfer of charge from the stem to the carrying ball through the fixed ball that the defenders of the fluid theory of electricity could have evoked. In Faraday's opinion, this experiment was the best proof that induction was an

[33] *FER* 1: #1260: 'This extraordinay difference was so unexpected in its amount . . .'; *ibid.*: #1292.
[34] *FD* 3: #4016.

action between contiguous particles, because a direct action would have been rectilinear.[35]

3.3.5 General views

For Faraday, induction in dielectrics was 'the essential principle for the development of electricity.' Electric charge was a spatial discontinuity of induction, and the electric current was a variation in time or a propagation of induction. For metallic conduction or sparking discharge, a temporary polarization of the particles of the medium occurred, followed by a mutual discharge of contiguous particles. In an electric wind (slow discharge of a metallic point in air), electrified particles traveled with their surrounding induction. In an electrolyte, Faraday imagined 'first a polarisation of the molecules of the substance, and then a lowering of the forces by the separation, advance in opposite directions, and recombination of the elements of the molecules.' In every case, an invariable amount of induction was transfered along the conducting channel. As Faraday put it, the electric current was 'constant and indivisible.'[36]

Therefore the magnetic power of a current was 'the same, whether [the current was] passing in an electrolyte, or in a conductor, or in spark, or in [electric] wind or even in inductive action.' Consequently, a convection current and even a dielectric under a varying state of induction had magnetic power, or 'such at least seem[ed] to be the case.' Conversely, Faraday expected that the crossing of magnetic curves would polarize a dielectric. More generally, he anticipated a role of the particles of matter in the communication of magnetic force. However, all the experiments he performed in 1838 on this subject failed.[37]

Induction being central to his views on electricity, Faraday sought to visualize it. From February 1836 to September 1837 he experimented on discharges in gases at various pressures and for various shapes of the conductors. He regarded the beautiful sparks, brushes, and glows as evidence of the previous state of induction of the gas. In the middle of these researches he introduced the 'lines of electric induction' or tension, a concept similar to the magnetic curves or to the lines of current:

> The description of the current as an axis of power, which I have formerly given, suggests some similar general expression for the forces of quiescent electricity. *Lines of electric tension might do*; and through I shall use the terms Pos. and Neg., by them I merely mean the *termini* of such *lines*.

[35] *FD* 3: ##4016–1 (7 October 1837), ##4092–104 (14 October 1837). *FER* 1: 380–3. The grounding of the screen, though essential, was overlooked by Riess, A. de la Rive, and Melloni: cf. Riess to Faraday, 10 December 1855, *SCMF* 2. Faraday had obtained earlier indication of induction in curved lines while experimenting with the cage: *FD* 2: ##2866–7 (16 January 1836).

[36] *FD* 3: #3425 (3 August 1836); *FER* 1, series 12: #1338 (metals), ##1405–1424 (sparks), ##1562–610 (convection); #1347 (electrolysis, also #1622–1624); *FER* 1, series 13 (February 1838): #1627 (indivisible current). Faraday thus anticipated Maxwell's doctrine that all currents are closed.

[37] *FD* 3: #3471 (3 August 1836); also *FER* 1, series 14 (June 1838): #1644, #1654; ##1709–30 (failed experiments). The contribution of the variation of dielectric polarization to the current corresponds to Maxwell's later displacement current. When seeking a role of matter in the communication of magnetic actions, Faraday also had in mind the electro-tonic state (*FER* 1: #1729).

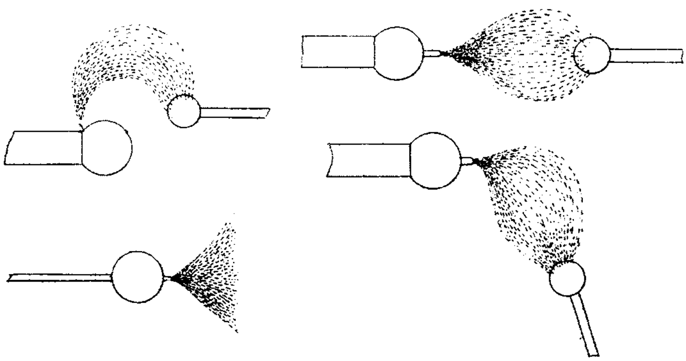

FIG. 3.8. Electric brushes (*FER* 1: plate 8).

In the case of discharge through gases, the observed luminous patterns illustrated the lines of induction (Fig. 3.8), as iron filings did for the magnetic lines of force.[38]

Faraday insisted that the lines of induction were 'imaginary,' as the lines of magnetic force already were. They were 'a temporary conventional mode of expressing the direction of the power in cases of induction.' However, Faraday imagined a mechanical tension along, and a mutual repulsion between the lines. The repulsion explained induction in curved lines and the concentrating power of conducting edges. The tension accounted for the motive forces between charged bodies. Accordingly, two charged bodies could only *attract* each other to the extent of their connection by lines of induction. Electric repulsions were only apparent, they were a consequence of the attraction by other bodies. For example, the leaves of a gold-leaf electrometer did not really repel each other: they were attracted by surrounding bodies charged by induction.[39]

As for the cause of induction, Faraday refused to take a stand. He focused on the 'manner in which the electric forces are arranged in the various phenomena generally [. . .] without committing [himself] to any opinion as to the cause of electricity.' However, he imagined a molecular process for the propagation of induction. In his view all matter was made of polarizable particles scattered in empty space. A polarized molecule polarized its nearest neighbors and only them, so that the action between distant molecules could only be indirect, through chains of intermediate polarized molecules. This transfer of polarization occurred without global loss of intensity, which explained the 'illimitable' character of induction, in conformity with the representation by lines of force. The attraction of successive polar molecules along the same line explained the tension along this line. The repulsion between molecules placed side by side explained the lateral repulsion of the lines of induction. That is to say, the polarized molecules behaved like tiny bar magnets.

[38] *FD* 2: 443–67; *FD* 3: 14–96; *FER* 1, series 12: 447–72, series 13: 473–502; *FD* 3: #3423 (quote, 3 August 1836); *FER* 1, series 11: #1224, #1231. Cf. Gooding 1978: 142–3.

[39] *FER* 1, series 11: #1304 (imaginary), #1231 (temporary), series 12: ##1371–4 (tension and repulsion); *FD* 2: #2642: 'I begin to doubt electric repulsion altogether,' also #2653 (26 November 1835).

Note that Faraday did not completely eliminate action at a distance. The action between nearest neighbors was direct action at a distance. For the time being, he only wished to eliminate the large-scale action at a distance implied in the usual fluid theories of electricity.[40]

Faraday's speculations stopped there. He did not explain the polarity of molecules, he just defined it as 'a disposition of force by which the same molecule acquires opposite powers on different parts.' Whether this disposition resulted from some kind of stress or from the displacement of a fluid, he did not want to decide. This is how one should understand his surprising statement: 'The theory of induction which I am stating does not pretend to decide whether electricity be a fluid or fluids, or a mere power or condition of recognized matter.'[41]

3.3.6 Incommunicability

Faraday's demonstration of specific inductive capacity was immediately hailed as a major electric discovery. His experiments on hollow conductors and on induction in curved lines became popular. His theoretical interpretation, however, fell in deaf ears. Well aware of the iconoclastic character of his views, Faraday concluded his eleventh series with the words:

> I beg to say that I put forth my particular view with doubt and fear, lest it should not bear the test of general examination, for unless true it will only embarrass the progress of electrical science. It has long been on my mind, but I hesitated to publish it until the increasing persuasion of its accordance with known facts, and the manner in which it linked effects apparently very different in kind, urged me to write the present paper.

Faraday's fear was justified. His theoretical statements met more misunderstanding and suspicion than ever.[42]

Experts in mathematical electrostatics had at least one good reason to doubt the soundness of Faraday's reasonings. His statement that induction in curved lines, specific inductive capacity, and impossibility of absolute charge were 'not consistent with the theory of action at a distance' could easily be refuted. A few theorems of French electrostatics covered the first and third facts. Faraday could not see this, for he judged himself 'unfit to form a judgement of [Poisson's] admirable papers' of 1811. With respect to specific inductive capacity, Faraday overlooked that the dependence of induction on the dielectric substance did not necessarily imply that the substance was *entirely* responsible for the capacity. Ottaviano Mossotti and William Thomson soon devised mathematical theories of dielectric polarization based on

[40] *FD* 3: #3423 (3 August 1836); #3512 (6 September 1836): 'Induction [. . .] an action of contiguous particles affecting each other in turn, and not action at a distance'; *FER* 1, series 11: 409–11 and 362n: 'The word *contiguous* is not the best that might have been used [. . .]; For as particles do not touch each other it is not strictly correct [. . .]. By contiguous particles I mean those which are next'; *FER* 1: #1231. Cf. Gooding 1978: 122–7.

[41] *FER* 1: #1304; *ibid.*: 409n. In the diary (*FD* 3: ##4567–70, 1 April 1838), Faraday assumed that the molecules of insulators were conductors insulated from each other (as in Poisson's theory of magnetic polarization). But he did not specify the relevant conduction mechanism.

[42] *FER* 1, series 11: #1306. Cf. Williams 1965: 372.

standard action at a distance. Anyone who could understand their calculations was likely to share the following opinion of a German expert:[43]

> Thus [by extension of Coulomb's and Poisson's electrostatics] the electrostatic problems are changed into problems of pure mechanics [. . .]. The advantage of this method is very great, it gives the result of each experiment as the sum of single actions which the mind connects without difficulty, and leaves to the mathematier [sic] the pains to sum up the single effects and to find the amount of the sum [. . .]. Therefore, I have long ago defended this theory against its—indeed not very dangerous—antagonists and I could not abstain continuating the defense, as arose an adversary in the man whom I venerate as the greatest natural philosopher of the age.

Although Faraday failed to disprove the mathematical fluid theory, he could have convinced his readers that his views were a possible alternative. Yet this almost never happened. Even favorably disposed readers found obscurity and absurdity in his statements. Faraday ascribed this communication breakdown to the ambiguities of language:

> I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

A first example of misunderstanding concerned the impossibility of absolute charge. Faraday wrote: 'It is impossible to charge a portion of matter with one electric force independently of the other.' Many of his readers understood that isolated charged particles could not be produced. In fact Faraday only meant that the charge would be the starting point of an induction that would end somewhere (possibly very far) as an opposite charge.[44]

Another difficulty had to do with induction in curved lines. Call A the originally charged body, B the influenced body, and C the screen. According to Faraday, the action of A on B was diminished by the presence of C. Reverent Whewell, Auguste de la Rive, Macedonio Melloni, and Peter Riess, among others, did not see how the action between two bodies could depend on a third body. In their interpretation (Fig. 3.9(b)), the conductor C was polarized under the effect of A, and what Faraday observed was the superposition of the actions of A and C on B. Faraday did not deny that C acted on B if the body C was insulated. But in his experiment C and B were grounded, so that no line of force could connect them. The effect of C was to deflect the lines of force from A and thus to diminish the number of lines reaching B (Fig. 3.9(a)). At that point, misunderstanding became total and reciprocal. Riess maintained that in this case too, the presence of C did not at all diminish the action of A on B. What was observed was the additional effect of the negative charge appearing in C by influence (Fig. 3.9(c)). At the root of the misunderstanding was

[43] *FER* 1: ##1166–8, #1305; Riess to Faraday, 10 December 1855, *SCMF* 2. On Thomson and Mossotti, cf. next section. On Riess, cf. Simpson 1968: 122.

[44] Faraday to Hare, 18 April 1840, *FER* 2: 262; *FER* 1: #1177; *FER* 2: 268. The misunderstanding is, e.g., in Hare to Faraday, *FER* 2: 254.

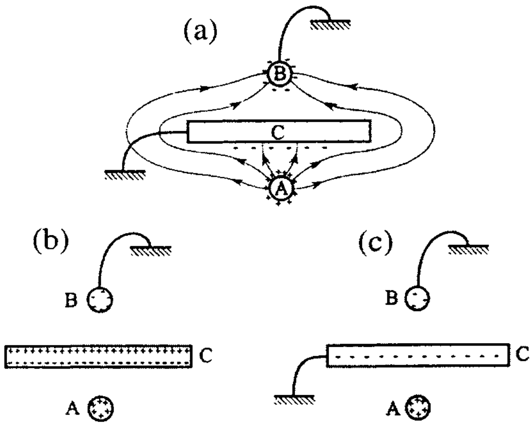

FIG. 3.9. Faraday's 'induction in curved lines': (a) authentic, (b) misinterpreted by Whewell and Riess, (c) reinterpreted by Riess.

a different concept of interaction. For Riess, the interaction of A and B was completely defined by their electric states and their relative position. For Faraday, this interaction was measured by the number of lines of force connecting the two bodies.[45]

Lastly, Faraday's molecular picture of induction arose much suspicion. Robert Hare, a chemistry professor at Penn, reproached Faraday with admitting both contiguous action and direct action at a distance. Indeed Faraday insisted that induction was an action between contiguous particles, but at the same time he admitted direct action through a vacuum, even over one inch. How, asked Hare, could the same law apply to the forces developed in such different circumstances? Should one, in the case of vacuum, admit the existence of an ether whose particles could be polarized as the particles of matter were?[46]

In his public reply Faraday first recalled that he was concerned with the arrangement of electric force, not with the deeper nature of electricity. By action between contiguous particles, he only meant action between successive particles, and not contact action as Hare assumed. A given excited particle could act directly on distant particles if no other particle existed in the intervening space. In this sense contiguous action was perfectly compatible with direct action in a vacuum. Faraday further showed that if the $1/r^2$ law applied to a vacuum it also applied to a dielectric if the latter behaved like a myriad of mutually insulated conducting molecules.[47]

[45] Whewell to Faraday, 22 November 1848, and Faraday to Whewell, 24 November 1848, *SCMF* 1; A. de la Rive 1853, Vol. 1: 143–4; Riess 1854; Faraday to Riess, 19 November 1855, in Faraday 1856 and in *SCMF* 2; Riess to Faraday, 10 December 1855, *SCMF* 2.

[46] Hare to Faraday, July 1840, in *FER* 2: 251–61, esp. 251, 252, 260; *FER* 2: #1616 for the one-inch vacuum.

[47] Faraday to Hare, 18 April 1840, *FER* 2: 262–74, esp. 262, 265–267, 264–5. Cf. Gooding 1978: 119–27.

A few years later, Faraday detected a paradox in his representation of matter: the interstitial vacuum had to be a conductor in conducting bodies and an insulator in insulating bodies. He then proposed a Boscovichian speculation: atoms could be centers of power surrounded by an atmosphere of force. Since the atmospheres never completely vanished, 'matter fill[ed] all space,' and the paradox disappeared. In previous works Faraday had replaced matter-bound imponderable fluids with powers in intervening spaces. Thanks to Boscovichian atoms, he could perhaps subsume all physics under the concept of power. If matter was condensed power, its role in the transmission of electric and other actions became self-evident.[48]

Faraday could hardly hope to disarm his colleagues' criticisms with such speculations. But he could strengthen his immunity against alien approaches. As the acute Riess noted in a letter to Faraday, this was for the better profit of science:[49]

> I have little hope to persuade you, my dear Sir, to modify your views [on electricity] and, I confess, if I could I would scarcely wish it. The great philosopher works best with his own tools, whose imperfections he avoids by dexterous application. But these tools, so efficacious in his hand, are not only useless but very dangerous in the hands of others.

3.4 The magnetic lines of force

3.4.1 Illuminating a magnetic curve

In 1839 overworking, memory loss, and perhaps feelings of intellectual isolation plunged Faraday into a long period of depression. He did not, however, forget his aim of proving the role of matter in the propagation of force. Stimulated by William Thomson, in the fall of 1845 he renewed his earlier attempt to show an effect of electrolytic currents on polarized light. He also tried the similar effect with a polarized, transparent dielectric. Both experiments failed. A week later, he 'worked with lines of magnetic force, passing them across different bodies' (Fig. 3.10). Air and flint glass did not work. However, a special kind of heavy glass that Faraday had earlier fabricated for optical use induced a faint but distinct rotation of the plane of polarization, when the light beam was parallel to the lines of force. Faraday exulted: 'This fact will most likely prove exceedingly fertile and of great value in the investigation of both conditions of natural forces [magnetism and light].'[50]

The rotation of the plane of polarization suggested some kind of rotation within the substance of the glass. 'Is it possible,' Faraday asked, 'that similar electric cur-

[48] Faraday 1844a: 293; also *FER* 3: #2225. Cf. Williams 1965: 375–80. Faraday's reference to Boscovich should be taken in the vague sense of atoms as centers of force. Faraday ignored other essential characteristics of Boscovich's atomism: cf. Spencer 1967; James 1985: 142–3.

[49] Riess 1856: 17. Cf. the defense of incommensurability in Biagioli 1990.

[50] Thomson to Faraday, 6 August 1845, *SCMF* 1; *FD* 4: ##7434–71 (30 August, 1 September 1845), earlier attempt in *FER* 1, series 8: ##951–5; *FD* 4: ##7483–97 (5 September 1845), already planned in *FD* 2: #2512 (3 November 1835); *FD* 4: #7498, #7504 (13 September 1845). Cf. Williams 1965: 384–91; Gooding 1981: 234–6.

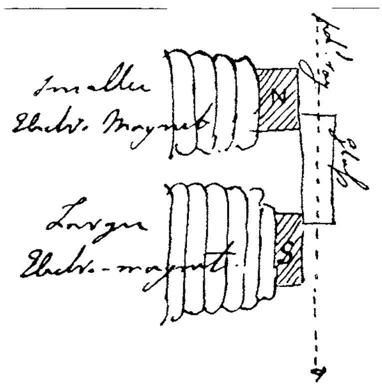

FIG. 3.10. Faraday's first device producing the magnetic rotation of a polarized ray (*FD* 4: 264).

rents are circulating both in the particles of the Iron and the particles of the glass? Or rather, perhaps, may it not be that in the iron there are circular currents, but in the glass only a tension or tendency to circular currents?' Faraday subsequently obtained the effect with other transparent bodies including oil of turpentine, flint glass, rock salt, water, and alcohol. In order to increase the length of action of the magnetic force, instead of a magnet he used a very long solenoid containing a cylinder of the transparent body. In this device, the polarization plane of the light rotated in the same direction as the helical current. The 'beautiful simplicity' of this law strengthened Faraday's idea of an internal rotation: 'Cannot but suppose some relation or similarity of constitution between bodies rotating per se and such as are under the influence of magnetic force. Such bodies are to the latter what ordinary magnets are to Magneto-helices when the current is passing through them.'[51]

If matter was modified by magnetic force, Faraday reasoned, it probably played a role in the communication of magnetic force. He promptly introduced the word 'dimagnetic' (later to become 'diamagnetic' under Whewell's advice) in analogy with 'dielectric,' and speculated that 'magnets act[ed] by intervening particles,' as electrified bodies already did. At the same time, he emphasized the progress he had made in correlating forces of a different kind. As he explained in his publication:

> I have long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are made manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their action.

[51] *FD* 4: #7569 (18 September 1845); *FER* 3, series 19: #2200; *FD* 4: #7688 (26 September 1845).

His new discovery confirmed this expectation: 'I have at last succeeded,' Faraday exclaimed, 'in *illuminating a magnetic curve* or *line of force* and in *magnetising a ray of light*.'[52]

3.4.2 Touching magnetic curves

Faraday's more immediate aim was to prove the role of matter in the communication of magnetic action. A few days after his discovery of the effect on light, he tried to show a magnetic condition of the magnetized heavy glass by means of a compass needle. This did not work. In early November 1845, he suspended a bar of heavy glass at the end of a silk thread between the jaws of a new, powerful electromagnet. Upon turning on the current, the bar immediately assumed an equatorial orientation, at a right angle to that taken by an iron bar. Faraday commented: 'Thus touching dimagnetics by magnetic curves and observing a property quite independent of light.'[53]

Faraday then tried other substances. Good conductors like copper displayed complex behavior because of the currents induced during their motion. Most non-magnetic substances, however, behaved like the heavy glass, though not with the same intensity. A poorly conducting metal, bismuth, gave the best effect. Faraday also expected the human body to be diamagnetic: 'If a man could be in the Magnetic field, like Mahomet's coffin, he would turn until across the Magnetic line, provided he was not magnetic.' This is the first known occurrence of the phrase 'magnetic field,' meaning the space between the poles. Faraday may have introduced it here because of the anthropomorphic context: men or prophets explore fields. His growing fondness for the word probably resulted from his awareness that diamagnetic actions were not directly given by the usual magnetic curves.[54]

On 10 November, Faraday 'examined the Magnetic field by the bar of bismuth.' He found that the bismuth tended to move from stronger to weaker points of magnetization. Surprisingly, the direction of the magnetic force played no role here: 'There is no apparently dual character in the force—is an unique phenomenon as to its kind.' This action indicated 'a new set of magnetic curves.' Faraday soon regarded the earlier orientation effect as deriving from the latter action: when the bismuth bar, in the middle of the field, made an angle with the magnetic axis, its ends were

[52] *FD* 4: #7576 (18 September 1845); Whewell to Faraday, 10 December 1845, *SCMF* 2; *FER* 3, series 19: #2146; *FD* 4: #7705 (26 September), #7718 (30 September). Here 'force' is to be understood in the broad, qualitative sense of the cause of physical actions; 'power' has the ordinary meaning of the intensity of the effected transformations. On the correlation of forces, cf. Gooding 1980.

[53] *FD* 4: ##7691–2 (26 September 1845), #7871 (20 October); #7902 (4 November 1845). Cf. Williams 1965: 392–4; Gooding 1981: 236–237. Effects of magnets on bismuth and other non ferromagnetic bodies had been noted long ago, but never been the object of a systematic study: cf. Williams 1965: 393.

[54] *FD* 4: ##7999–8078 (8 November 1845); #8014 (8 November), also #8085 (10 November): 'between the great poles, i.e. in the magnetic field.'

repelled by the axis on which the magnetic intensity was the highest, until the equatorial position was reached.[55]

On 12 November, Faraday imagined two possible explanations of the diamagnetic orientations and repulsions. In the first explanation, which I call Amperean, he invoked the formation of reverse Amperean currents: 'Can there be formation in Bismuth of currents in the *contrary* direction?' In the second explanation, which I call differential, the cause would be a different conductive power of the air and the diamagnetic body for magnetic action:[56]

The Bismuth goes *from strong* to *weak* points of magnetic action. This may be because it is deficient in the inductive force or action, and so is displaced by matter having stronger powers, giving way to the latter. Just as in Electrical induction the best conductors, or bodies best fitted to carry on the action, are drawn into the vicinity of the inducing bodies or into their line of action.

The latter interpretation carried on the analogy between magnetic and electric induction and the view that magnetism was an action between contiguous particles. The Amperean interpretation did not connect as well to Faraday's preconceptions, and seemed to him to have an unwanted consequence: that electromagnetically induced currents in bismuth and copper would have opposite directions. Yet, in print Faraday only mentioned the Amperean explanation:[57]

Theoretically, an explanation of the movements of the diamagnetic bodies [. . .] may be offered in the supposition that magnetic induction causes in them a contrary state to that which it produces in magnetic matter [. . .]. Upon Ampère's theory, this view would be equivalent to the supposition, that as currents are induced in iron and magnetics parallel to those existing in the inducing magnet or battery wire; so in bismuth, heavy glass and diamagnetic bodies, the currents induced are in the contrary direction.

Faraday could not endorse the differential explanation because he failed to verify a clear consequence of it: that the repulsion of a diamagnetic body by a magnet pole should depend on the pressure of the surrounding air, and turn into an attraction in the case of vacuum. One could still imagine, as Edmond Becquerel did, that a magnetic ether played the role of the air. But this option could not satisfy Faraday, who

[55] *FD* 4: #8108 (10 November 1845); #8119 (10 November): 'Its endeavour [the bismuth's] is in fact not to go along or across the curves exclusively—but to get out of the curves going from stronger to weaker points of magnetic action'; #8137 (12 November); #8121 (10 November); *FER* 3, series 20 (December 1845): #2269. Cf. Gooding 1981: 239–43.

[56] *FD* 4: #8138, ##8144–5 (12 November 1845). By 'the best conductors' of the inductive action Faraday probably meant metallic conductors (which do not sustain polarization, but transfer it most efficiently: *FER* 1: #1566, and Faraday to Riess, 19 November 1855, *SCMF* 2): as was well known, an uncharged conducting ball goes to the regions of stronger electric action. A dielectric with high specific inductive capacity would have offered a better analogy, but the effect was too small to be observed.

[57] *FD* 4: #8141 (12 November 1845): 'Would a *Bismuth wire* or rod, carried across the magnetic curves, give a current in the same direction as a wire of copper or a contrary current?' Faraday answered negatively in ##8425–6 (26 November 1845) (however, he saw that microscopic currents could perhaps behave differently from macroscopic ones: *FER* 3: #2431); *FER* 3, series 21: ##2429–30.

generally opposed imponderable fluids and ethers. Instead he reluctantly admitted two sorts of magnetic bodies, with opposite inductive properties: 'I incline, by my view of induction through particles, to think that all bodies are in *one* magnetic list—but the facts as yet rather sustain the view of *two*.'[58]

3.4.3 The 'magnecrystallic' effect

Faraday's new findings immediately attracted the attention of German investigators. In Bonn, Julius Plücker studied the magnetic behavior of birefracting crystals, and detected 'a repulsion of the optical axis' by the poles of a magnet. In August 1848 Faraday meticulously studied this effect with Plücker's collaboration. In September, he explored the magnetic field with crystalline bismuth. Besides the repulsions observed on amorphous bismuth, he found a new orientation effect depending on crystal structure. In a uniform magnetic field, for which the usual repulsions do not exist, a well-defined axis of the crystal, 'the magnecrystallic axis,' positioned itself in a direction parallel to the magnetic curves. In a heterogenous field, a cubic crystal still showed the direction of the magnetic curves, because the cubic shape prevented the equatorial orientation effect. This action, Faraday commented, was 'an important indicator of the direction of the lines of force in a magnetic field,' because the bismuth crystal, unlike a compass needle, did not perturb the lines.[59]

Regarding the nature of the magnecrystallic effect, Faraday first condidered the possibility that a crystal would be less apt for diamagnetic induction along the magnecrystallic axis than along other directions, in the spirit of the Amperean interpretation of diamagnetism. If this were true, the repulsion of the crystal by a magnetic pole would be weaker when the axis pointed toward the pole. With a carefully designed bifilar torsion balance Faraday tested this difference, but could not find it. At that point he could 'not resist throwing forth another view of these phaenomena,' in harmony with his earlier differential interpretation of diamagnetism: the lines of magnetic force could 'pass more freely' in the direction of the magnecrystallic axis, just as light rays traveled faster (or slower) along the optical axis of a crystal. Then the equilibrium position of the crystal would be the position of 'least resistance' to the passage of the lines of force.[60]

[58] *FD* 4: #8257 (15 November 1845): 'What ought a vacuum to do? This is important as regards air, gases and indeed the whole subject'; #8262 (15 November): 'If air be rarefied, ought not different bodies suspended in it to set round in succession into the axial position—Water, Heavy glass, Bismuth, etc.?' [the air becoming successively less magnetic than water, heavy glass, and bismuth]; #8362–78 (22 November): failure to detect any effect of rarefaction; E. Becquerel 1846a, 1846b, 1849, 1850; *FD* 4: #8514 (quote, 6 December 1845). Cf. Tyndall 1870: xiii; Gooding 1981: 249–51. Becquerel independently developed the idea of a differential action, by analogy with Archimedes' push. In his view an ethereal medium was responsible for the magnetic properties of vacuum. Cf. Williams 1965: 420–22.

[59] *FD* 5: ##9378–465 (16 August–1 September 1848); *FER* 3, series 12: #2592–613; *FD* 5: #9467, #9475, #9494 (2 September 1848); *FER* 3: #2479, #2546. Faraday's intense activity in this field was largely motivated by his hope to obtain informations on molecular forces. Cf. Williams 1965: 417.

[60] *FD* 5: ##9920–25 (24 October 1848), rougher try in ##9855–6 (13 October); *FER* 3, series 22: #2551, #2552, #2588, #2591 (quote).

FIG. 3.11. Faraday's replication of Weber's experiment for testing the magnetic polarization of a bismuth block (B) in the field of a horse-shoe magnet (NS), with a compass needle n and a compensatory bar-magnet *S'* (*FD* 5: 153).

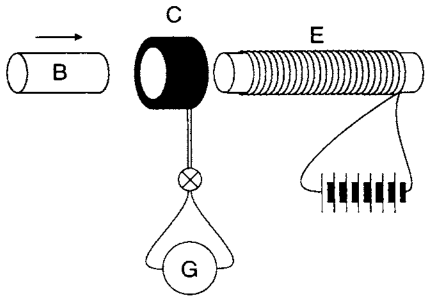

FIG. 3.12. Weber's device for showing electromagnetic induction by a variable diamagnetic polarization.

3.4.4 Weber's diamagnets

Meanwhile, Wilhelm Weber had published a double experimental proof of the interpretation of diamagnetism in terms of reversed Amperean currents. Diamagnetic actions were very small, and Weber deplored that he could not yet study them quantitatively. However, he could use Gaussian techniques to provide direct proofs of diamagnetic polarity. His first experiment aimed at showing the action of diamagnetic polarization on a magnetic needle. In the device represented in Fig. 3.11, the action of the horseshoe magnet on the needle n is exactly balanced by the action of the bar magnet until a block of bismuth B is brought between the jaws of the magnet. With the Gaussian method of the mirror and telescope, Weber detected a motion of the needle in the direction opposite to that which an iron block would give. Weber's second experiment (Fig. 3.12) concerned the electromagnetic induction produced by a variable diamagnetic polarization. The cylinder of bismuth B is periodically thrust into the hollow coil C, placed at the end of a powerful electromagnet E. The induced current is measured by means of a galvanometer connected to the coil by a commutator K compensating the sign changes of the induced currents.[61]

The positive result of these experiments was not the only source of Weber's belief in diamagnetic polarity. He also managed to integrate the phenomenon in his general theory of electricity. Faraday had already suggested that the polarity, if it existed,

[61] Weber 1848b.

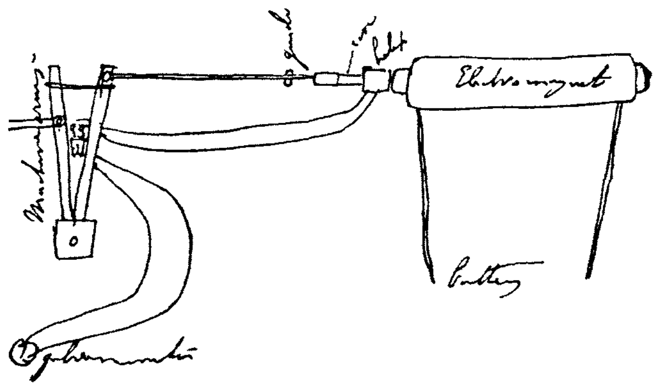

FIG. 3.13. Faraday's device for testing currents induced by the motion of various cores in the field of an electromagnet (*FD* 5: 204). Clockwise, the handwritten words read: galvanometer, machine arms, guide, core, helix, electromagnet, battery.

could be due to the temporary formation of molecular currents running opposite to ordinary Amperean currents. Weber saw that the laws of electromagnetic induction implied the formation of such currents, if only the diamagnetic body contained microscopic circular channels of zero resistance for the electric fluids. In a resistance-less circuit the electromotive force is proportional to the variation of the current (with a coefficient depending on the inertia of the electric fluids and on the self-inductance). Therefore, any variation of the magnetic force at the circuit implies a proportional variation of current. By Lenz's law, this variation must be such that it counteracts the external magnetic force, in agreement with the negative sign of diamagnetic polarization.[62]

Such microsopic reasoning on electric fluids was so alien to Faraday's views that he completely ignored Weber's theory. Faraday did, however, spend several months on Weber's experiments. In March 1849 he repeated the first experiment, but found the bismuth to be '*nil* in its action.' From September to December he worked on Weber's other experiment, with practically the same device, except that he automatized the periodic thrust of the bismuth cylinder (Fig. 3.13). He found it extremely difficult to avoid the communication of vibrations from the mechanism moving the bismuth to the coil and electromagnet. He also struggled to discriminate between currents induced in the bismuth mass and true diamagnetic polarity: 'Astonishing how great the precautions that are needed in these delicate experiments. Patience. Patience,' can be read in his diary. At the term of this painstaking excursion into the territory of Gaussian precision, Faraday concluded that the effect observed by Weber must have been parasitic electromagnetic induction.[63]

[62] Weber 1848b: 267.

[63] *FD* 5: #10050, #10691 (Faraday wrongly attributed this device to Ferdinand Reich); *ibid.*: ##10330–10690, quote from #10462 (16 November 1849). Faraday recycled the oscillating mechanism from earlier experiments on the relation between gravity and electricity.

3.4.5 Conducted lines of force

After this episode, Faraday discarded the possibility of diamagnetic polarity. He found more evidence against the German view of diamagnetism in subsequent experiments on gases. In the fall of 1847 he had learned from Francesco Zantedeschi that flames were repelled by the poles of a strong magnet. Faraday explained this effect by a temperature-dependent (dia)magnetism of gases, and proved that all current gases except oxygen were diamagnetic with respect to the air by observing their motion after being freed in the air between the poles of an electromagnet.[64]

If this motion was due to attractions or repulsions as the polarity interpretation supposed, Faraday reasoned, then a single gas should be more dense (if paramagnetic) or less dense (if diamagnetic) in the more intense parts of the magnetic field. In October 1849 with an optical method and in January 1850 with a closed vessel and a capillary manometer, he proved the absence of such compressions. To which he commented: 'Is then the effect an effect not of attraction or repulsion but a differential effect of another kind between the two bodies which are free to go to the pole?' In April he definitely adopted the idea that 'the conductor [of the magnetic action] which can conduct the most will of necessity be drawn into the place of most intense action.' A great advantage of this view was that it explained why gases were not compressed (or expanded) near magnetic poles, despite their para- or diamagnetic behavior with respect to one another.[65]

In his 26th series, of October 1850, Faraday developed the notion of conducting power 'as a general expression for the capability which bodies may possess of affecting the transmission of magnetic force, implying nothing as to how the process of conduction is carried on.' By definition, a diamagnetic body conducted less and a paramagnetic conducted more than vacuum, and some crystals had different conductivities in different directions. Then all known repulsions, attractions, and orientations resulted from the rule of least resistance to the passage of the lines of force. In sum, Faraday returned to the differential interpretation of diamagnetism of November 1845, but now avoided its main defect, its reliance on an ether, by divorcing conduction from action between contiguous particles.[66]

Faraday multiplied diagrams representing the disturbance of lines of force by dia- and paramagnetic bodies, insisting on the deformations of the lines *outside* the bodies (Fig. 3.14). These deformations, together with the law of least resistance,

[64] Zantedeschi 1847; *FD* 5: ##9066–291 (23 October–18 November 1847); Faraday, 'On the diamagnetic conditions of flame and gases,' *FER* 3: 467–93. Cf. Williams 1965: 396–9. Much later, in July 1850, Faraday was able to compare gases with vacuum and to prove that oxygen was paramagnetic: *FD* 5: ##10896–967.

[65] Plateau to Faraday, 25 March 1849, in *FD* 5: 196–8 (Plateau suggested the optical method); *FD* 5: ##10277–301 (10–5 October 1849): 'No sensible *condensation* or *expansion* of the air or gases in the *intense magnetic field*'; ##10714–43 (7–21 January 1850) and related experiments in February and March; #10744 (21 January); #10793 (4 April 1850). The argument is dubious: if a principle of least resistance to the passage of the lines of force rules the equilibrium of the gas, then a diamagnetic gas should be more expanded where the force is stronger. This effect exists (as today's physicists know) but is too small to be detectable by Faraday's devices.

[66] *FER* 3, series 16: #2797. Cf. Gooding 1981: 268–75.

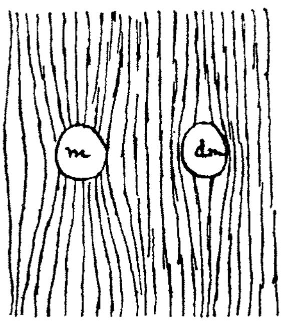

FIG. 3.14. Faraday's drawing of the lines of force around a (para)magnetic (m) body and a diamagnetic (dm) body (*FD* 5: 320).

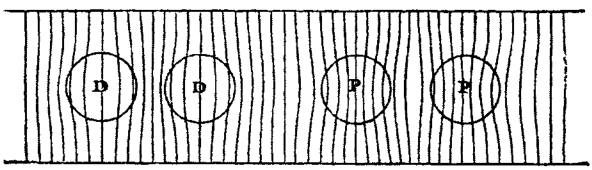

FIG. 3.15. Lines of force deformed by two neighboring diamagnetic (D) or paramagnetic (P) spheres (*FER* 3: 212).

implied new effects, for example the orientation of an oblong piece of bismuth in a homogenous magnetic field, as Thomson had earlier explained to Faraday. Such effects were too small to be observed by means of bismuth in air. Faraday obtained them with bismuth in a strong solution of iron protosulfate, thanks to the higher difference of conductive power.[67]

Effects usually attributed to magnetic polarity and previously denied by Faraday resulted from the rules of conduction. For example, on Fig. 3.15 the two diamagnetic spheres repel each other because the lines of force are compressed between the two spheres, and because diamagnetic bodies, being worse conductors than vacuum, tend to move away from fields of higher intensity. The paramagnetic spheres also repel each other, by a dual mechanism: the lines of force are further apart in the space between the spheres, and paramagnetic bodies, being better conductors than vacuum, tend to move away from places of lower intensity. In this context Faraday still used the world 'polarity,' but in a sense different from Weber's. He only meant the asymmetry of the disturbance of the lines of force when entering and leaving a body. Accordingly, he rejected Weber's idea that a diamagnetic body behaved like a paramagnetic body turned end for end without change of its magnetic state.[68]

[67] *FD* 5: #10832 (8 April 1850); #10921, #10922 (20 July 1850) *FER* 3: #2807, #2810, #2821, #2812; Thomson to Faraday, 19 June 1849, *SCMF* 2.

[68] *FER* 3, series 26: #2815, #2816, #2831, #2820. However, Faraday admitted polarization in Weber's sense for ferromagnetic bodies (#2833).

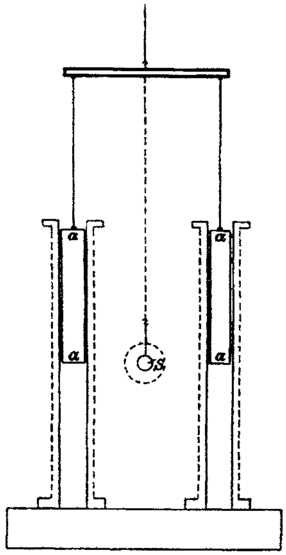

FIG. 3.16. Weber's apparatus for measuring the magnetic moment of 'diamagnets' (Weber 1852: 491).

3.4.6 Weber's revenge

In 1852 Weber published a new instalment of his *Maassbestimmungen*, the subject of which was diamagnetism. Presumably hurt by Faraday's criticism of his previous experiments, he claimed that the account he had published was only partial and provisional and that in fact he had taken into account the currents induced in the bismuth mass. He attributed Faraday's failure to duplicate his results to an inferior technique of magnetometric and galvanometric measurement. But he admitted that his previous proofs of diamagnetic polarity were insufficient. 'In order to lead to sure results,' he declared, 'the observation of so weak actions needs *quantitative control*, something that has completely lacked so far.' The main purpose of the new *Maassbestimmungen* was to provide quantitative versions of the two previous experiments on diamagnetic polarity.[69]

In order to measure the magnetic moment of a uniformly polarized bismuth cylinder, he imagined the device of Fig. 3.16. Two identical bismuth cylinders aa can slide within two long, parallel solenoids fed by the same constant current in opposite directions. The south pole S of the suspended magnetic needle of a magnetometer lies at exactly equal distances from the two solenoids. Because of this symmetry, the solenoids have no magnetic action on the needle. Any spurious dissymmetry is compensated by a distant coil (not represented on the figure). Then

[69] Weber 1852: 534–5, 488. Faraday used a ready-made galvanometer by Ruhmkorff (*FER* 3: #2651), whereas Weber used the Gaussian technique.

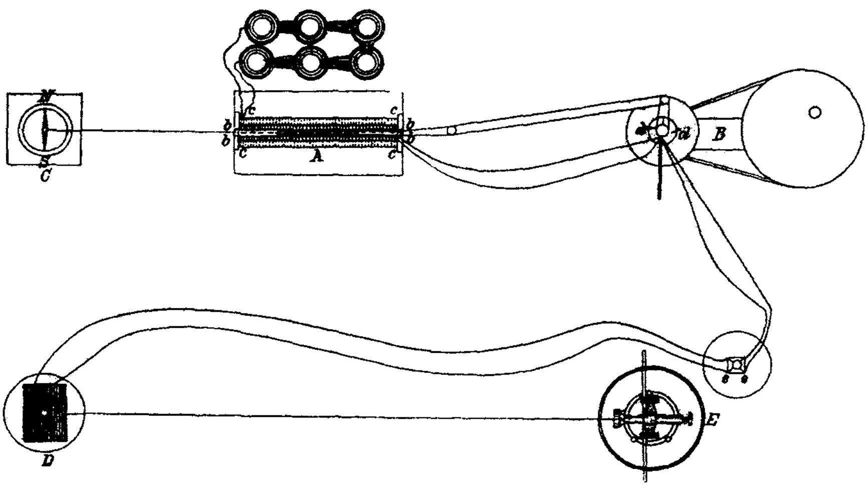

FIG. 3.17. Weber's arrangement for measuring the currents induced by a moving diamagnet (Weber 1852: 508).

only the 'diamagnets' act on the needle. Within the solenoids the magnetic force is uniform and vertical, so that vertical displacements of the bismuth bars cannot induce any currents in their mass. Such displacements occurred in Weber's measurements, because, according to an old Gaussian trick, he multiplied the action of the diamagnets on the needle by swinging the bismuth cylinders synchronically with the oscillations of the magnetometric needle. With four collaborators, including Johann Listing and Bernhard Riemann, Weber determined the magnetic moment of the bismuth bars for a given solenoid current. This moment turned out very small and negative, as was expected.[70]

In the improved version of his second experiment, Weber again exploited the uniformity of the magnetic force within a solenoid. In part A of his drawing (Fig. 3.17), one of the bismuth cylinders of the previous experiment, aa, oscillates within the solenoid cccc fed by the six-cell battery. An induction coil bbbb surrounds the moving cylinder. This coil is made of two oppositely connected halves (not shown on the figure), so that the inductive actions of the two poles of the diamagnet have the same sign. The mechanism in B, borrowed from Faraday, commands the alternating motion of the bismuth bar as well as the commutator dd. The current from the induction coil runs through this commutator and another manual commutator ee, and reaches the galvanometer D, observed with the telescope E. Lastly, the compass SN is used to measure the current in the solenoid cccc. With this sophisticated device, Weber obtained the expected induction from the moving diamagnet, in quantitative agreement with the magnetic moment measured in the first experiment.[71]

[70] Weber 1852: 489–505. [71] Weber 1852: 506–31.

Having thus proved the existence of diamagnetic polarity, Weber turned to its theory. He distinguished four kinds of explanations of magnetism: Poisson's microscopic cells for the separation of the magnetic fluids, elementary pivoting magnets, pivoting Amperean currents, and molecular channels of zero-resistance for the electric fluids. The three first hypotheses led to a magnetic polarization in the direction of the impressed magnetic force. Therefore, they could not explain diamagnetism. The last assumption was the only one left. In his theoretical world of imponderable fluids acting at a distance and organized in microscopic structures, Weber believed that he had explored all possibilities. He therefore asserted the physical existence of undamped microscopic currents. Diamagnetism corresponded to the induction of such currents during the application of a magnetic force, and (ferro)magnetism to the orientation of preexisting currents of this kind. Weber now denied the existence of the magnetic fluids, although he had originally preferred them to Amperean currents.[72]

3.4.7 Indifference

Weber's impressive memoir convinced most experts in the field, even Faraday's friend John Tyndall, who perfected Weber's experiments with Weber's own help. Yet Faraday remained undisturbed. In 1854 he included Weber's work in a long list of 'magnetic hypotheses,' with ambiguous praise:

> Weber stands eminent as a profound mathematician who has confirmed Ampère's investigations as far as they proceeded, and who has made an addition to his hypothetical views [the microscopic induced currents][. . .]It would seem that the great variety of these hypotheses and their rapid succession was rather a proof of weakness in this department of physical knowledge.

Faraday judged that Weber's idea of polar diamagnetism 'involve[d], if not magnetic impossibility, at least great contradiction and much confusion.' If a magnet induced a reverse polarization of the particles of a diamagnetic body, he reasoned, then a reverse induction should also occur from particle to particle and prevent global polarization.[73]

Faraday's antagonism depended on his unwillingness, or incapacity, to conceive secondary sources, and the superposition of their action with primary sources. For him, the disturbance of the field produced by a diamagnetic body was primitive, it was not to be deduced from the formation of a new magnetic state of the body. Faraday's polarity was about the disturbance of the lines of force. Weber's polarity was about secondary sources: 'By *magnetic or diamagnetic polarity* of a body,' Weber wrote, 'I understand a state of this body through which it exerts on other bodies actions that are so constituted that they can be completely explained by *an*

[72] Weber 1852: 538–46, 557–60.

[73] Tyndall 1856; *FER* 3, 'On magnetic hypotheses' (1854): 525–6; *FER* 3, 'On some points of magnetic philosophy' (1855): #3309 (quote), 3310–2. Cf. Tyndall 1870: xvii–xviii. Faraday had two notable supporters: Carlo Matteucci in Italy, and Fabian von Feilitzsch in Germany (cf. Tyndall 1870: 156–8).

ideal distribution of magnetic fluids.' Like Gauss's representation of the Earth's magnetism by a superficial distribution of magnetic fluids, this definition was ontologically neutral, and it gave to the question of diamagnetic polarity a clear-cut empirical meaning. Faraday, being a simple man who 'felt [his] way by facts closely placed together,' could see nothing there but a mathematician's perversion.[74]

The communication breakdown was analogous to what Faraday had already experienced in the chapter of electric induction. Ambiguous concepts were responsible: 'interaction' in one case, 'polarity' in the other. Faraday defined these concepts in terms of the distribution of power in the field, whereas other investigators thought in terms of interacting states of distant objects or sources. Consequently, utterly different interpretations could accompany the same experimental fact. After friendly chats on diamagnetism with Tyndall, Faraday observed: 'I differ from Tyndall a good deal in phrases, but when I talk with him I do not find that we differ in facts. The phrase *polarity* in its present state is a great mystifier.'[75]

3.4.8 Sharpening the lines of force

Since his discovery of 'the Faraday effect,' Faraday had made more and more frequent use of the magnetic lines of force and had become more and more convinced of their physical reality. He could 'illuminate' the lines of force, 'touch' them with diamagnetic bodies, and channel them through 'conductors.' In a study on atmospheric magnetism published in 1850, he opened himself on this speculative matter:[76]

> External to the magnet these concentrations which are named poles may be considered as connected by what are called magnetic curves, or lines of magnetic force, existing in the space around. These phrases have a high meaning, and represent the ideality of magnetism. They imply not merely the directions of force, which are made manifest when a little magnet, or a crystal or other subject of magnetic action is placed amongst them, but these lines of power which connect and sustain the polarities, and exist as much when there is no magnetic needle or crystal than as when there is; having an independent existence analogous to (though very different in nature from) a ray of light or heat, which, though it be present in a given space, and even occupies time in its transmission, is absolutely insensible to us by any means whilst it remains a ray, and is only made known through its effects where it ceases to exist.

Faraday devoted his two next series to the lines of magnetic force. There he refrained from ontological commitment: 'I desire to restrict the meaning of the term *lines of force*, so that it shall imply no more than the condition of the force in any given space, as to strength and direction: and not to include (at present) any idea of the nature or the physical cause of the phenomena.' His aim was to give a more precise definition of the lines and to offer a systematic account of their use in the representation of various phenomena. He gave three definitions of the lines: through the orientation of a compass needle, through the currents induced in a moving wire,

[74] Weber 1852: 486 (his emphasis); Faraday to Ampère, 3 September 1822, *CMF* 1.
[75] Faraday to Matteucci, 2 November 55, *SCMF* 2. [76] *FER* 3: 323.

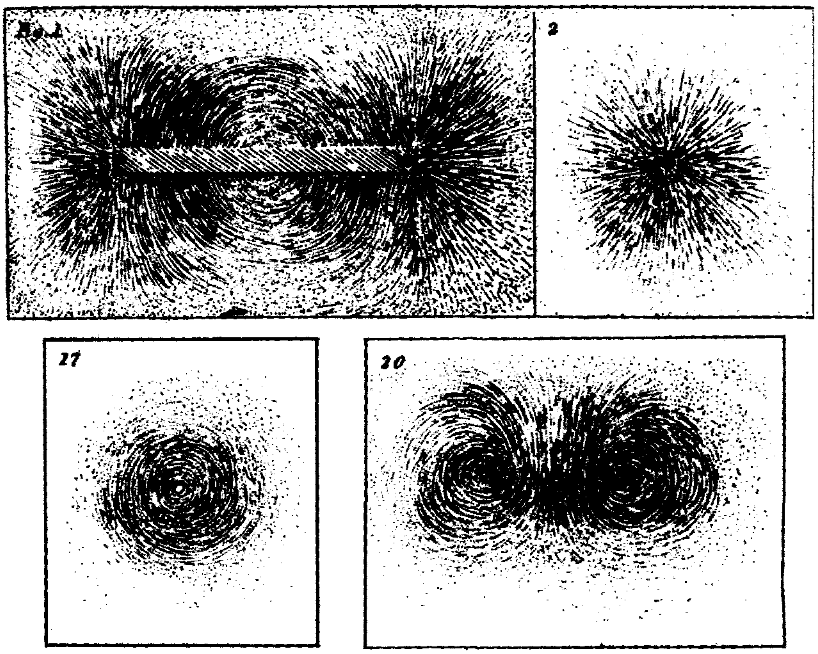

FIG. 3.18. Iron-filing patterns around magnets and conducting wires (*FER* 3: plate 3).

and through the magnecrystallic effect of bismuth. He also provided a plate of illustrations by iron filings (Fig. 3.18). The convergence of the three definitions, and the suggestiveness of the iron filing patterns, must have contributed of his conviction that the lines of force were 'true to nature.'[77]

In previous magnetic studies, Faraday had often made quantitative use of the lines of force: he had implicitly assumed that parallel equally spaced lines represented a homogenous field, and he measured the intensity of the force at a given place by the concentration of the lines. He now made the convention entirely explicit, and called 'unit lines of force' the lines that complied with it. In these terms he could give a quantitative formulation of his electromagnetic induction law: 'The quantity of electricity thrown into a current is directly as the amount of curves intersected.' The unit lines of force also reflected a basic property of the magnetic field, which Maxwell later called the conservation of the magnetic flux.[78]

Among the various ways to define the lines of force, Faraday favored the moving wire, because it could be used to test the conservation of flux inside and outside matter. He experimented with a bar magnet that could rotate around its axis, and

[77] *FER* 3, series 28: #3075 (quotes); series 29: ##3234–40 (filings).

[78] *FER* 3, series 28: #3122, #3115, #3073: 'The sum of the power contained in any one section of a given portion of the lines is exactly equal to the sum of power in any other section of the same lines,' or $\nabla \cdot \mathbf{B} = 0$ according to Maxwell.

with a wire loop connected to a galvanometer, part of which went through the mass of the magnet (with proper insulation). For various forms of the loop, he turned the magnet or the external wire. Taking for granted that the (external) lines of force did not follow the motion of the magnet, he drew the following conclusions: the lines of force traversed the substance of the magnet, they were always closed, and the magnetic flux was conserved upon entering or leaving magnetic matter.[79]

3.4.9 The physical lines of force

In the following year Faraday relaxed his empiricist reserve, and published 'On the physical character of the lines of magnetic force.' Speculations on the deeper nature of forces, he now argued, were 'wonderful aids in the hands of the experimentalist and mathematician.' In his discussion he compared the four known kinds of power: gravitation, light, electricity, and magnetism. All could be represented by lines of force, but these lines could have different meanings in each case. The lines of force for gravitation did not have to be physical, because they were straight, uninfluenced by interposed matter, and instantly acting. In contrast, those for light had a wealth of reasons to be physical: they could be emitted, curved, absorbed, and polarized, and they took time to propagate. In the case of electricity, the facts of decomposition, inductive capacity, and induction in curved lines proved the physical character of the lines: they represented an action between contiguous particles, at least when matter was present.[80]

For magnetism, the situation was unfortunately less clear. Faraday admitted that he had no strict proof of the physical existence of the lines. For sure, they were affected by the presence of matter. But the effects of intervening matter were opposite for diamagnetic and paramagnetic bodies, so that the lines of force could not possibly represent a unique kind of action between contiguous particles of matter.[81] A way out of the difficulty was to introduce, as Edmond Becquerel had already done, a polarizable ether with a polarizability intermediate between that of diamagnetic and paramagnetic bodies. Faraday himself mentioned the possibility 'that all conduction of magnetic force [was] carried on by circular electric currents round the

[79] *FER* 3: ##3090–121.

[80] *FER* 3 (June 1852): #3244; ##3245–51. By gravitational lines of force Faraday could not mean the lines tangent to the net force acting on a point mass (those would be curved in general). What he seems to have meant is the lines representing force emanating *from a given point mass* in the presence of other bodies, that is, the net force minus the forces that the other bodies would exert *if the point mass was not there*. These lines are straight, because the gravitational force exerted by the other bodies does not depend on the existence of the point mass. In the case of electrostatics, the similarly defined lines of force are generally curved, because the force exerted by other bodies (typically neutral polarizable bodies) depends on the existence of the given point charge. This interpretation fits Faraday's statement on #3245: 'One particle gravitating toward another particle has exactly the same amount of force in the same direction, whether it gravitates to that one alone or towards myriads of other like particles, exerting in the latter case upon each one of them a force equal to that it can exert upon the single one when alone: the result of course can combine, but the direction and amount of force between any two given particles remains unchanged.'

[81] Hence Faraday's difficulty to decide between one or two 'magnetic lists': *FD* 4: ##8398–9 (22 November 1845), #8514 (6 December 1845), *FD* 5: ##10806–7 (4 April 1850). Cf. Gooding 1981: 249–53.

line of magnetic force in the whole of its course, and in that case that they must exist in a vacuum itself.' However, such views were too crudely mechanistic to please him. He preferred a direct transference of the magnetic lines of force through pure space, as he already assumed for gravitation and for electric induction across the space between dielectric particles.[82]

As for the curving of magnetic lines of force, Faraday deplored the lack of strong evidence. A proof analogous to the one he had given in the electric case would have required the existence of the magnetic counterpart of electric conductors.[83] Iron filings or compass needles did not necessarily prove the curvature because their presence could alter the distribution of forces. From the beginning of his research Faraday had used entities defined by virtual experiments. Yet he did not mistake counterfactual definitions for proofs of physical existence.[84]

Having exhausted the possibilities for a clear-cut demonstration of the physical existence of the lines of magnetic force, Faraday turned to more speculative and somewhat obscure arguments. In some of them, he insisted on the dual character of magnetic power: if a pole could not be created without the simultaneous creation of an opposite pole, there had to be some kind of physical link between the two poles. Specifically, he related the existence of the lines of force of a magnet to that of inner polarization (proved, for example, by broken magnets): one could not deny an external relation between the poles of a magnet, he suggested, without denying their internal relation.[85]

In another ingenious argument he related the behavior of the magnetic lines of force to the existence of electrodynamic forces. On the one hand, all mechanical actions among magnets could be reduced to a tension along the lines of magnetic force and a mutual repulsion of these lines (suggested by the shape of the lines and by the analogy with magnetic needles placed side by side). On the other hand, electric currents tended to elongate themselves, and parallel currents placed side by side attracted each other. Using the axis–loop relation between lines of current and lines of force, Faraday showed the perfect agreement between the two kinds of forces. In Fig. 3.19(a), the repulsion between the circular lines of force C and C′ tends to elongate the current i, while on Fig. 3.19(b), the tension of the line of force L implies an attraction of the current loops i and i'. The magnetic lines of force thus seemed 'to have a physical existence correspondent to that of their analogue, the electric lines.'[86]

[82] *FD* 5: #10834 (8 April 1850); *FER* 3: #3075; *FER* 1, series 25: #2787, #2788; *FD* 5: #10374: 'Is magnetic action *across space*, through air, water, a vacuum, etc., but between contiguous particles in iron, nickel, etc.?', #10837: 'Is it not probable and most likely that lines of Magnetic force can be transferred across space in the manner of Gravitating and Static Electricity force, and without these circular currents (or their equivalents), which are assumed to exist in iron when it is in the magnetic field?'; *FER* 3: series 21, ##2445–6; *ibid.*, #3258: 'Physical lines of force.'

[83] That is, bodies unable to sustain magnetic induction: nothing to do with the conduction of the magnetic lines of force.

[84] *FER* 3: #3254. [85] *FER* 3: ##3257–64, ##3282–98.

[86] *FER* 3: ##3264–69. This explanation of the attraction of two currents is similar to the one Faraday gave in 1821 in terms of magnetic powers.

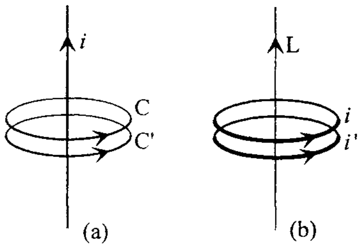

FIG. 3.19. Diagrams for the relation between electrodynamic forces and magnetic field stresses.

Faraday had no definite idea, however, of the physical condition represented by the magnetic lines of force. It could be a dynamic condition, in conformity with the dynamic nature of the electric current and with the hypothesis of Amperean currents. In this case, Faraday argued, there should be a magnetic equivalent to static electricity, and such a thing had never been observed. He therefore preferred to imagine a static condition, 'a state of tension (of the aether?)' that would provide for the long-sought electro-tonic state. As Faraday knew, in 1835 Whewell had proposed a dynamic interpretation of the tonic state as the momentum of a motion connected to the electric current. But Faraday held fast to his older intuition of a state of tension in the magnetic field.[87]

This view raised the question of the thing that was under a state of tension. It could not always be matter, since magnetism acted across a vacuum. It could perhaps be the optical ether. However, Faraday disliked the idea of assimilating empty space with a subtler kind of matter. In most of his works, he maintained a sharp distinction between vacuum and matter. Typical is his 1850 warning against a confusion between the magnetic properties of vacuum and matter: 'To confuse them together would be to confound space with matter, and to trouble all the conceptions by which we endeavour to understand and work out progressively a clearer view of the mode of action and the laws of natural forces.'[88]

Then nothing was under a state of tension. Tension, force, and power existed by themselves. So said Faraday in his occasional dynamistic speculations. In that of 1844 he reduced matter to concentrations of power. In his 'Thoughts on ray vibrations' of 1846, he proposed a natural extension of this view: the subtler kind of matter called ether did not exist; there were only gravitational, electric, and magnetic lines of force crossing empty space. Then light was a transverse vibration of the lines of force. In this view the physical character of the lines of force became a necessity,

[87] *FER* 3: #3269; Whewell to Faraday, 25 April 1834, Faraday to Whewell, 3 May 1834, *CMF* 1. On the latter exchange, cf. Anderson 1994. In the context of the magneto-optical effect, Faraday had imagined circular currents in the magnetized glass (*FD* 4: #7569, 18 September 1845); he did not publish the idea, because he was looking for a state of tension: *FER* 3: #2229.

[88] *FER* 3, series 25: #2787.

since matter and light were both derived from them. Without physical lines of force, nothing in the world would be physical.[89]

3.5 Thomson's potential

3.5.1 British reformers

In the mid-1840s, after many years of general neglect, Faraday's theoretical views attracted the attention of a young mathematical prodigy, William Thomson. This improbable encounter between two very different kinds of mind cannot be understood without first capturing some pecularities of Thomson's education.

Fourier's *Théorie analytique de la chaleur* was Thomson's first intellectual love and inspiration. During his student years at Glasgow and Cambridge, French mathematics and mathematical physics were generally considered best and most promising. Since the 1810s, progressive men like John Herschel, Charles Babbage, William Whewell, and George Airy had denounced the degeneration of the British tradition of mathematical physics. For Herschel, 'the last twenty years of the eighteenth century were not more remarkable for the triumphs of pure and applied matheamtics abroad, than for their decline, and, indeed, all but extinction at home.' The putative cause was a slavish following of Newton's methods. The proposed remedy was a thorough study of French classics.[90]

Initially, the British reformers adopted Laplace's notion of rigor. They abandoned the intuitive, geometrical conception of calculus inherited from Newton, and based mathematical analysis on algebraic definitions and manipulations. They defined derivatives in Lagrange's manner, as the successive coefficients in Taylor series. They solved differential equations by power series. For the mathematization of physics, they adopted Laplace's assumption of point-molecules of ponderable matter and imponderable fluids acting directly at a distance. This model lent itself to perfectly definite mathematical deductions: one just had to integrate the forces acting on a given molecule from all other molecules, to derive a differential equation from the resulting integral equation, and to find the solutions for given boundary solutions. Lastly, these solutions were compared with quantitative experiments.[91]

However, the British interest in Laplacian molecularism and algebraism soon declined. When they became available, Fourier's and Fresnel's memoirs on heat and light captured the attention. Being less speculative and more geometrical than Laplacian works, they pleased the pragmatic and illustrative inclinations of British natural philosophers. Fourier's theory of heat was especially attractive: it refrained from special assumptions on the nature of heat and the constitution of matter; its basic equations had a direct empirical meaning; and it made a central use of the

[89] Faraday 1844a. 1846.
[90] Herschel 1832: 9–31. Cf. [illegible] 1973; Smith and Wise 1989: 151–5; Crosland and Smith 1978; Grattan-Guinness 1985.
[91] Cf. Smith and Wise 1989: 151–5.

geometrical notion of flux across a surface. The first British works on optics and electricity that went truly beyond previous French theories, George Green's and James MacCullagh's, consciously adopted and perfected Fourier's methodology.[92]

Young William Thomson was in an excellent position to appreciate the virtues of French mathematics. His father James Thomson taught mathematics in Belfast's Academical Institution until 1830, and then at Glasgow University. In his lectures and in his numerous textbooks, he promoted French methods supplemented with geometrical illustrations and practical applications. So did his Glasgow colleagues John Pringle Nichol and William Meikleham, who instructed his son William in natural philosophy. The emphasis on geometry and sensible motions was particularly strong in Scotland. Nichol taught that 'the quality of form is the simplest of all the qualities of matter, and hence geometry, which treats of it, stands at the head of Natural Philosophy.' Like Dr Thomson, he followed another Scottish principle, the unity of art and science, and a latitudinarian value, anti-dogmatism. Both men fought metaphysics and abstraction, as William Thomson would do for the rest of his life.[93]

Among French authors, Fourier best incarnated Scottish values. Nichol's praise of the *Théorie analytique de la chaleur* was so high that William Thomson absorbed the thick volume in two weeks during May 1840, at age 16. Within a few months he was able to correct local misinterpretations of this 'great mathematical poem' and to complete some of its proofs. Most originally, he used an analogy between electrostatics and heat propagation to prove new electrostatic theorems. He sent a highly dense and concise account of this work to the *Cambridge Mathematical Journal* in September 1841, just before going up to Cambridge.[94]

3.5.2 Electrostatics and heat flow

As Thomson knew from Poisson, electrostatic forces in air derived from a function V that satisfied the same partial differential equation as that given by Fourier for the stationary temperature distribution in a homogenous solid: $\Delta V = 0$. However, Fourier treated the sources of heat as surface conditions, whereas Poisson included the sources of electricity in the differential equation. Thomson first extended the analogy by introducing point sources of heat. In this case, Fourier's equation gave a temperature proportional to the inverse distance from the source. Thomson superposed such sources on a surface S with the density σ to reach the following expression of the temperature θ:

$$\theta = \int \frac{\sigma \mathrm{d}S}{r}, \tag{3.1}$$

[92] Cf. Smith and Wise 1989: 155–68; Wise 1981a: 23–32. On Fourier's method, see also Dhombres and Robert 1998: Ch. 8.

[93] Thomson, 'Notebook of Natural Philosophy class, 1839–40,' quoted in Smith and Wise 1989: 210. Cf. *ibid.*: Chs. 1, 2; p. 40 (on Nichol).

[94] Thomson 1841a, 1841b; *TMPP* 3: 296 (poem); Thomson 1842. Cf. Smith and Wise 1989: 167, 203–4.

which is identical to the expression of V corresponding to the electric density σ.[95]

Thomson then imagined heat sources on a closed surface, their distribution being such that the surface was isothermal. He reasoned as follows. The temperature within the surface must be a constant, because if it were not, there would be a flux of heat around a small closed surface surrounding any internal temperature extremum, in contradiction with the absence of internal sources. Analogously, if a solid body has a surface charge such that the corresponding V is a constant on the surface, then V is a constant inside the body and the electric force vanishes there. Consequently, a sufficient condition of equilibrium for a conductor is that the electric force created by the surface charge should be perpendicular to the surface.[96]

Thomson now inverted the analogy. He proved, as Coulomb had done before him, that the electric force immediately outside the closed surface was normal to the surface and equal to 4π times the surface density. Consequently, the density of heat sources that sustain a constant temperature on the surface is equal to the heat flux across the surface divided by 4π. Now, the temperature outside any isothermal surface depends only on the temperature on the surface and on the heat flux at every point of the surface, as long as all sources are within or on the surface. This property is an obvious consequence of Fourier's view of the propagation of heat as an action between contiguous elements of volume. Its electrostatic counterpart is the far less obvious surface-replacement theorem: the electric force due to any distribution of electric charge is the same as the force due to a fictitious distribution of charge on a surface of constant V containing all the real charges, the surface density being equal to the electric force created on the surface by the real charges divided by 4π.[97]

Unknown to Thomson, the new theorems had already been published three times, by Green in 1828, by Gauss and by Chasles in 1839. Green and Gauss, like Thomson, ascribed a central role to the function V. However, their methods were purely analytical, based on partial integration and quadratic forms. Thomson's essential innovation was a new method for finding theorems by formal analogy between two physical theories. In his reasonings, he moved back and forth between two physical theories, transposing notions and theorems from one theory to the other. The starting point of one theory (Coulomb's law), became a new result of the other (the temperature distribution of point sources). An obvious consequence of the physical picture of one theory (Fourier's local transfer of heat) became an essential theorem of the other (the surface-replacement theorem).[98]

Thomson's analogy suggested that electrostatic action could perhaps be a contiguous action in the medium between sources, as heat propagation was. Thomson did not say so much, however, and he did not mention Faraday. For the electric

[95] Thomson 1842: #3, #4. This analogy contains what current textbooks call 'Gauss's theorem': the flux of the electric field across a closed surface must be equal to the total included charge, because the corresponding heat flux is equal to the heat provided by the included sources.

[96] Thomson 1842: #5.

[97] Thomson 1842: ##6–9. Cf. Wise 1981a: 33–9; Smith and Wise 1989: 205–12.

[98] Green 1828; Chasles 1839; Gauss 1839. Cf. Thomson 1845b: 17–18n. On Green, cf. Grattan-Guinness 1995.

density he used the name 'density of electrical matter,' which referred to the fluid conception, but also 'electric intensity' which echoed the view of one of his Glasgow professors that electricity was a *state* of bodies. This liberalness probably indicated a lack of commitment on the nature of electricity. In any case, Thomson avoided a physical exploitation of the analogy between heat and electricity. He did not even give a name to the counterpart of temperature, the function V.[99]

3.5.3 Discovering Faraday

Thomson's attitude changed somehow in 1843, as can be seen in his diary:

> I have been sitting half asleep before the fire, for a long time thinking whether gravity and electrical attraction might not be the effect of the action of contiguous particles, communicated from one surface of [equal V] to another. In Cavendish's experiment, will the attraction of the balls depend at all on the intervening medium?

'Contiguous particles' and 'intervening medium' were Faraday's expressions; surfaces of equal V referred to Thomson's thermal analogy. Most likely, Thomson realized the physical implications of the analogy by reading Faraday. Yet he had little respect for Faraday's views. He found himself 'very much disgusted with [Faraday's] way of *speaking* of the phenomena, for this theory can be called nothing else.'[100]

Thomson changed his mind in Paris, when Liouville's account of the late Poisson's worries prompted him to reexamine Faraday's challenge of Coulomb's electrostatics. While developing a mathematical theory of the effect of dielectrics on electrostatic action, he gradually understood the consistency and precision of Faraday's ideas. Most strikingly, he discovered that Faraday's reasonings in terms of electric lines of force were similar to his own reasonings in terms of heat flow:[101]

> All the views which Faraday has brought forward, and illustrated or demonstrated by experiment, lead to [my] method of establishing the mathematical theory, and, as far as the analysis is concerned, it would, in most *general* propositions, be even more simple, if possible, than that of Coulomb [. . .]. It is thus that Faraday arrives at some of the most important of the general theorems, which, from their nature, seemed destined never to be perceived except as mathematical truths.

For example, Thomson continued, Faraday knew that tubes of lines everywhere tangent to the electric force and connecting one conductor to another determined equal and opposite charges on the corresponding surface sections of the conductors (Fig. 3.20). For Faraday, the theorem was an immediate consequence of the definition of electric charge in terms of the surging or ending of lines of force. For

[99] Thomson 1842: #5, #7; Thomson, notes on Meikleham's lectures, 1839–40, quoted in Smith and Wise 1989, p. 210: 'Light heat elctricity magnetism, &c are termed imponderable. This is incorrect, as we know them, not as substances, but as states of bodies.' Cf. Smith and Wise 1989: 208–9.

[100] Thomson, Cambridge diary: 24 February, 16–17 March 1843, quoted in Smith and Wise 1989: 203, 213.

[101] Thomson 1845b: 29–30. Cf. Smith and Wise 1989: 213–8.

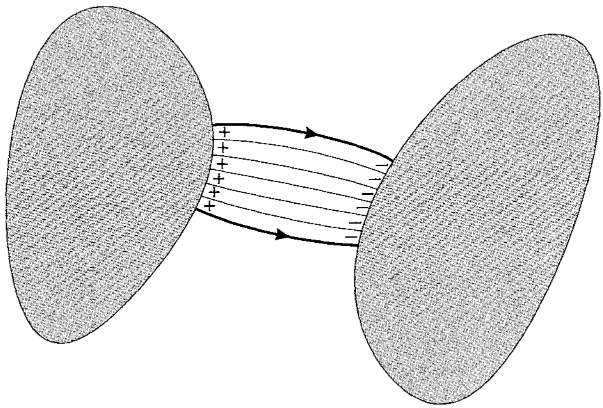

FIG. 3.20. Tube of force and corresponding surface elements of two conductors.

Thomson, it resulted from the conservation of heat flow. Faraday's lines of force had an exact counterpart in the lines of heat flow, which are everywhere perpendicular to the isothermal surfaces.[102]

At that stage, Thomson's heat-flow analogy was no longer confined to the finding of new mathematical theorems. It could be used to bridge two different physical hypotheses on the nature of electricity, Coulomb's and Faraday's. In print, Thomson refused to decide between the two hypotheses, because no known observable mechanical effect could discriminate between them.[103] The main purpose of his study was to show that one of Faraday's supposed proofs of the contiguous action, the existence of specific inductive capacity, could be interpreted in terms of direct action at a distance.

A material dielectric, Thomson showed, could be treated in a manner analogous to Poisson's theory of induced magnetism. Poisson, like Coulomb, imagined microscopic conducting cells (for the magnetic fluids) spread through the mass of the iron. The direct effect of an external magnetic force was to separate the fluids in each cell. Then the resulting dipoles generated secondary magnetic forces, to be superposed to the external one. There was a resulting discontinuity in the net magnetic force at the surface of magnetic bodies: the normal component of the force inside the body was a definite fraction of its value outside the body. The latter result, which no longer depended on microscopic assumptions, was the basis of Thomson's theory of dielectric effects. In the electric case, the surface discontinuity of the force is determined by the specific inductive capacity of the dielectric. This discontinuity can be seen to result from an 'imagined' surface charge, to be included among the sources of electric force. In the case of a Leyden phial filled with a material dielectric of inductive capacity ε, Thomson added the potential created by the imagined surface charges to that of the real charges on the conductors, and showed that for a

[102] Thomson 1845b: 30. [103] Thomson 1845b: 29.

given charge of the phial, its potential was ε times smaller than it would have been in a gaseous dielectric.[104]

This result was all that Thomson needed to explain Faraday's experiments on the 'division of charge' between identical Leyden phials filled with different dielectrics. What Faraday called 'the power or tension' of a phial corresponded to the potential, and the division of charge corresponded to the equalizing of the potentials of the external balls of the phials. Thomson concluded: 'The commonly received ideas of attraction and repulsion exercised at a distance, independently of any intervening medium, are quite consistent with all the phenomena of electrical action which have been here adduced.'[105]

At the same time, Thomson recognized that the heat-flow analogy could be extended to the case of dielectrics. The counterpart of a dielectric would be a solid with a thermal conductivity ε times larger than that of the surrounding medium. This made Poisson's force discontinuity a consequence of the continuity of the flux $-\varepsilon\nabla\theta$ across the surface of the solid. Thomson gave this precision only in a footnote. Unlike Faraday or Maxwell, he did not introduce a specific concept of flux for electrostatics. He limited the speculative use of analogies to a minimum, and tended to avoid conceptual distinctions that had no known empirical counterpart.[106]

3.5.4 The physical potential

While analyzing Faraday's experiments on inductive capacity, Thomson found that the 'power or tension' measured by Faraday by means of a carrying ball and a Coulomb balance was nothing but Green's potential. This was an essential insight, for the potential had previously been an abstract mathematical concept, with no direct operational significance.[107]

Another abstraction of potential theory was the integral $\frac{1}{2}\int\rho V d\tau$. Thomson learned from Gauss that the electricity at the surface of a conductor was in equilibrium if and only if this integral was a minimum. In August 1844 he interpreted this condition in terms of d'Alembert's principle of virtual velocities: for virtual displacements $\delta\mathbf{r}_i$ of the electric particles on the surface, the variation of the discrete version of Gauss's integral is

$$\delta\sum_{ij}\frac{1}{2}\frac{q_i q_j}{|\mathbf{r}_i-\mathbf{r}_j|}=-\frac{1}{2}\sum_{ij}q_i q_j\frac{(\mathbf{r}_i-\mathbf{r}_j)\cdot(\delta\mathbf{r}_i-\delta\mathbf{r}_j)}{|\mathbf{r}_i-\mathbf{r}_j|^3}=\sum_i \mathbf{f}_i\cdot\delta\mathbf{r}_i, \tag{3.2}$$

which must vanish according to d'Alembert.[108]

A few days earlier, Thomson had received a letter from his brother James, the engineer, concerning the efficiency of steam engines and the 'mechanical effect' they

[104] Thomson 1845b: 32–5. Cf. Wise 1981: 44–49; Smith and Wise 1989: 223–6. Ottaviano Mossotti later developed the same analogy, more slavishly and less efficiently (Mossotti 1847, 1850).

[105] *FER* 1: #1258; Thomson 1845b: 37.

[106] Thomson 1845b: 33n. Cf. Wise 1981a: 50–1; Smith and Wise 1989: 228–9.

[107] Thomson 1845b: 35.

[108] Thomson, 'Journal and research notebook': 14 August 1844, quoted in Smith and Wise 1989: 241.

could produce. The expression was synonymous with the 'travail' of French engineers, and referred to the height to which a given weight could be lifted by a machine. In the case of hydraulic engines, the mechanical effect originated from the fall of water. In the case of Carnot's ideal engine, which James and William frequently discussed together, the fall of heat from high to low 'intensity' was the source of the mechanical effect. Along the same lines, William Thomson saw that Gauss's integral was nothing but the mechanical effect needed to produce the distribution ρ.[109]

Around that time Thomson had been calculating the force between two electrified spheres, in connection with electrostatic experiments through which Snow Harris claimed to challenge Coulomb's law.[110] By analogy with an engine producing work, Thomson reasoned that the variation of the mechanical value of two insulated charged spheres during their separation measured the work spent to perform this separation. Consequently, the force between the two spheres could be calculated by taking the derivative of Gauss's integral with respect to their distance. This procedure was much simpler than the direct calculation of the force between the surface distributions of electricity.[111]

In his experiments Snow Harris grounded one of the spheres and connected the other to the ball of a charged Leyden jar. Therefore the force between the spheres depended on their distance, their radius, and the potential difference imposed by the jar, in a manner that Thomson could calculate. Thomson quickly perceived an opportunity for the absolute measurement of potentials.[112] He was familiar with Gauss's memoirs on geomagnetism, which introduced the notion of absolute measurement. Also, he spent part of 1845 working in the laboratory of the French champion of precision measurement, Victor Regnault.[113]

Immersed in a multiple context of steam engines, electrostatic experiments and calculations, geomagnetic measurements, and French steam measurements, Thomson brought physical meaning to the abstract concepts of French electrostatics. His novel insights all appear in a notebook entry of 8 April 1845:[114]

[109] James Thomson to William Thomson, 4 August 1844, discussed in Smith and Wise 1989: 242–3. Thomson, notebook remark of 8 April 1845, quoted *ibid.*: 245.

[110] Thomson 1845b; Harris 1834. Snow Harris operated with two conductors of various shapes, one being suspended on a balance and grounded, the other being fixed and connected to a battery or to a large electrified conductor. The attractive force turned out to be proportional to the square of the charge of the latter conductor. Harris doubted that received theories could explain his law (1834: 245). In response, Whewell and Thomson (1845b: 18–21) argued that his law was a consequence of Coulomb's theory, because the charge induced on the grounded body is proportional to the charge of the inducing body.

[111] Thomson, notebook entry of 8 April 1845, quoted in Smith and Wise 1989: 245; Thomson 1853c: 92–3. Thomson long delayed the publication, probably because he first wanted to develop the method of electrical images (Thomson 1845a, 1848–50): cf. Smith and Wise 1989: 246–7.

[112] For the first two-ball electrometer, cf. Thomson 1853c: 96. In an early draft of his 1845b, dated 12 April 1845, Thomson had already expressed the force of Harris's electrometer in terms of the potential difference of the two conductors: cf. Smith and Wise 1989: 246, 251–2. He noted that Harris's device was unsuited to quantitative measurements because of the lack of screening from inductive effects. Yet he could easily imagine the improvements that would tranform the device into an absolute electrometer.

[113] The head of the British 'magnetic crusade,' Colonel Sabine, had sought Thomson's expertise since the spring of 1844: cf. Smith and Wise 1989: 276–7. On Thomson and Regnault's laboratory, cf. *ibid.*: 106–8.

[114] Quoted in Smith and Wise 1989: 245.

To day, in the laboratory (of Physique at the Coll. de France, M. Regnault, prof.) I got the idea, which gives the mechanical effect necessary to produce any given amount of free electricity, on a conducting or non-conducting body [. . . .] This enables us to find the attraction or repulsion of two influencing spheres, without double integrals. Also the theorem of Gauss that [$\int \rho V d\tau$] is a minimum when V is a constant, shows how the double integral which occurs when we wish to express the action directly, may be transformed into the differential coefficient of a simple integral, taken with reference to the distance between the two spheres [. . .]. This has confirmed my resolution to commence experimental researches, if I ever make any, with an investigation of the absolute force, of statical electricity. As yet each experimenter has only compared intensities [surface charges] by the deviations of their electrometers. They must be measured by pounds on the square inch, or by 'atmospheres' [pressure units[115]].

In this incredibly dense statement, we find the germs of the energetic analysis of electrostatic systems, the energetic definition of force, and a related notion of absolute measurement.

As Gauss had remarked, absolute measurement presumed complete computability of the measuring apparatus in mechanical terms. Thomson met this requirement by means of the engineering notion of balancing mechanical values and effects. In this process, a potential difference became analogous to the difference of water height in hydraulic engines, or the difference of temperature in heat engines. In 1853 Thomson defined the potential directly in terms of a corresponding mechanical effect: 'The potential at any point in the neighbourhood of, or within an electrified body, is the quantity of work that would be required to bring a unit of positive electricity from an infinite distance to that point, if the given distribution of electricity were maintained unaltered.'[116]

3.5.5 *Absolute electrometry*

Thomson's resolution to start experimental researches concretized after he obtained, in 1846, the chair of natural philosophy at Glasgow. One of his first achievements in this area was the design of new electrometers. In his note of April 1845, Thomson meant to measure the 'electrical intensity,' which was, in contemporary terms, the surface charge of conductors. Experimenters commonly measured this quantity by means of Coulomb's '*plan d'épreuve*' or Faraday's 'carrying ball' brought in contact with the conducting surface and then with an electrometer. It seems likely, however, that Thomson also had in mind the absolute measurement of potentials. He knew that Harris's electrometers, if properly improved, would measure potential differences, and he knew that measurements of surface charge could lead to indirect potential measurements: in Faraday's experiments on specific inductive capacity, the surface charge of the connected outer ball was proportional to the potential of the internal conducting sphere. Thomson's later electrometers were all designed to measure potentials.[117]

[115] The square of a surface density has the dimension of pressure.
[116] Thomson 1853c: 87n; 1853a: 522n.
[117] On Thomson's professorship, cf. Smith and Wise 1989: Ch. 5.

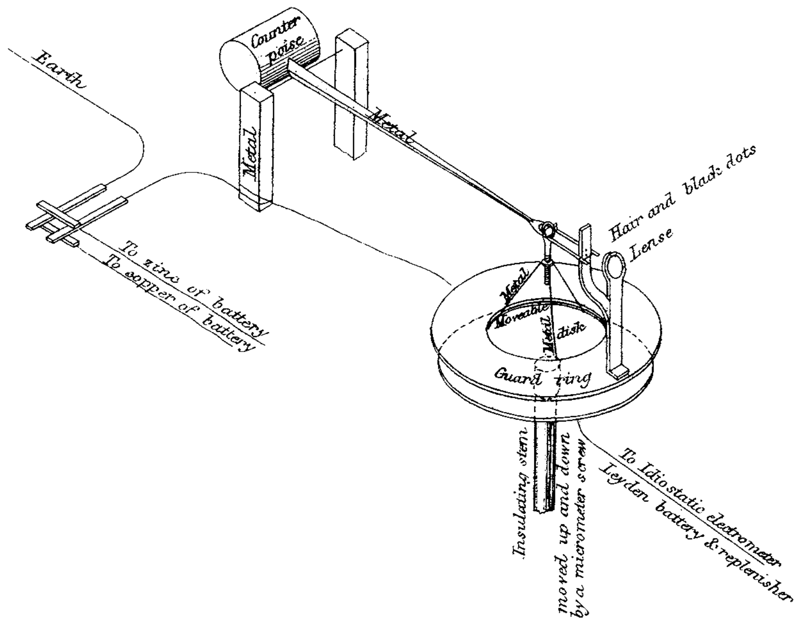

FIG. 3.21. Thomson's absolute electrometer (*TPEM*: 280–1).

Thomson's first project was an absolute electrometer based on Harris's two spheres. He soon preferred two parallel plane disks, which produced stronger forces. In this case, as Thomson had reckoned in 1845, the surface charges are mostly on the facing sides of the disks, and their density is nearly equal to $V/4\pi d$, where V is the potential difference and d the distance between the sides. The resulting attractive force between the two disks is easily found to be $V^2S/8\pi d^2$, if S is the surface of the facing sides. In deploying his device, Thomson demonstrated a Weberian patience and meticulousness. His mathematical power and his practical imagination allowed ingenious improvements. For example, he invented the 'guard-ring' surrounding the moveable disk and maintained at the same potential, so that the field below the moveable disk remained uniform for large values of the distance d (Fig. 3.21).[118]

An essential motivation of Thomson was the determination of the ratio c of the electromagnetic to the electrostatic charge unit. As he explained in 1853, the electromagnetic value of a given electromotive force could be compared with the value measured by an absolute electrometer. In 1860 he performed a first measurement of

[118] Thomson 1853c: 96 (two-ball electrometer built and analyzed); 1845b: 19–20 (force between two plates); 1853b: 553 and 1860a: 238 (description of two-plate electrometer, first exhibited at the Glasgow meeting of the British Association in 1855). For the history and the subsequent improvements, cf. Thomson 1867: 281–92. Cf. also Smith and Wise 1989: 250–2. Thomson also built the first sensitive electrometers, including the divided-ring and the quadrant electrometer. Cf. *ibid.*: 694–7, and Hong 1994a: 284–5.

this kind with a Daniell battery and the two-plate electrometer. The result agreed reasonably well with the value of c that Weber and Kohlrausch had obtained in 1856 by measuring the same electric charge with a Coulomb balance and an electrodynamometer. By that time the knowledge of c had acquired much practical importance, as we will see in a moment.[119]

The theoretical importance of this constant also increased in time. According to Maxwell's theory of 1862, c had to be equal to the velocity of light. Despite his general hostility toward this theory, Thomson found the conjecture worth verifying. In 1867 he set his Glasgow students to an improved determination of c using his absolute electrometer and a standard resistance of known absolute value. The result (2.82×10^8 m/s) agreed no better with Foucault's measurements of the velocity of light (2.98 and 3.08×10^8 m/s) than Weber and Kohlrausch's old value (3.11×10^8 m/s). However, this project nicely illustrated the increasing sophistication of Thomson's absolute electrometry.[120]

3.5.6 Electromotive force and mechanical effect

In 1848 Thomson extended his considerations of mechanical effect to electrodynamics. He first considered Neumann's 'very beautiful theorem,' according to which the electromotive force in a conductor moving with respect to a magnet is equal to the time derivative of its electromagnetic potential. 'It has appeared to me,' Thomson announced, 'that a very simple *a priori* demonstration of the theorem may be founded on the axiom that the amount of work expended in producing the relative motion on which the electro-magnetic induction depends must be equivalent to the mechanical effect lost by the currents induced in the wire.' Thomson first determined the mechanical effect lost by the current through the following reasoning. When the strength of the magnet is multiplied by n the induced current is multiplied by n, while the electromagnetic forces acting on this current and their mechanical effect thus provided are multiplied by n^2. The mechanical effect lost by the current i in the time $\mathrm{d}t$ is therefore equal to $ki^2\mathrm{d}t$, where k is a constant depending on the circuit and on the choice of units. Now the work of the electromagnetic forces during this time is $i\mathrm{d}P$, where P denotes the potential of a unit current with respect to the magnet. Balancing this work with the lost mechanical effect yields the expression $(1/k)\mathrm{d}P/\mathrm{d}t$ for the induced current, in conformity with Neumann's potential law.[121]

[119] Thomson 1853b: 553; Thomson 1860a. Maxwell and Thomson's notation for c was v. The constant C of Weber's theory (which Weber denoted c) is equal to $c\sqrt{2}$ (see Appendix 2). In 1855 Thomson had already obtained a rough estimate of c by working back from cable-retardation results: cf. Smith and Wise 1989: 456.

[120] *BAR* (1869): 434. The Glasgow students sent a constant current through a resistance of known absolute value and an electrodynamometer, and measured the potential at the terminals of the resistance with an absolute electrometer. Cf. Maxwell 1873a: #772. On the competition with Maxwell on the same problem, and on the increasing complexity of relevant resources, cf. Schaffer 1995. On later methods and the final convergence between c and the velocity of light, cf. Rosa 1889.

[121] Thomson: 1848b: 91. At that time Thomson was not yet convinced of the kinetic nature of heat, which explains why he does not identify the lost mechanical effect with the Joule heat. His reasoning,

After reading Weber's memoir on absolute resistance measurement (1850), Thomson realized that his analysis of electromagnetic induction provided a fruitful alternative to Weber's definitions. In Weber's absolute units, the electromotive force e in a rectilinear conductor of unit length cutting the lines of force of a uniform magnetic field of unit intensity at right angles is equal to the velocity v of the conductor. If this conductor is the only moving part of a closed circuit, a current is induced in proportion to this electromotive force. The electromagnetic force then acting on the moving conductor is numerically equal to the absolute electromagnetic measure i of the current. Therefore the work needed in a unit of time to move the conductor is vi, or ei. According to the 'principle of mechanical effect' this work must be equal to the mechanical effect consumed in the circuit. For other kinds of source, the mechanical effect produced by an electromotive force e acting on a current i is still equal to the product ei, because it should not depend on the nature of the source. Conversely, Thomson proposed to define electromotive forces by the mechanical effect they produced on a unit current.[122]

These considerations, and the earlier reflections on the potential, may be seen as the electric facet of Thomson's progression toward a general formulation of the energy principle. At the same time they commenced the subsumption of physics under this principle, a process intensifying in the 1850s under the lead of Thomson, Helmholtz, and Rankine. Thomson's notion of absolute measurement made mechanical effect—later to become 'energy'—the measure of all physical quantities. It bore the mark of the engineering culture of his brother James, and it prefigured the definition of forces in terms of energy functions that is found in Thomson and Tait's *Treatise on natural philosophy*.[123]

3.5.7 The transatlantic telegraph and BA units

Thomson's brand of practical physics met spectacular successes in submarine telegraphy. Around 1850 the introduction of gutta-percha, an excellent insulator, permitted the installation of the first subterranean and submarine telegraph lines. However, signalling on such lines proved much less efficient than on air lines. Consulted by the Electric Telegraph Company, Faraday experimented on the new cables, and published the following diagnosis in 1854. A single-wire cable acted as a huge Leyden jar, the gutta-percha corresponding to the glass, the surface of the copper wire to the inner coating, and the sea water or the Earth to the outer coating. Consequently, (electrostatic) induction in the gutta-percha competed with the (electrostatic) induction through the wire, and the discharge of electricity took more time. Faraday regarded this phenomenon as a sensational confirmation of his idea that induction,

similar to one Helmholtz had published a year earlier (see Chapter 6), omits self-induction and assumes that the internal energy of the magnet–circuit system does not depend on the relative position of the magnet and the circuit, which happens to be true (the field energy does not vary in this case, because the magnet's and the circuit's fields are orthogonal in Fourier space). Cf. Knudsen 1995.

122 Thomson 1851b. Thomson also proposed a thermal measurement of absolute resistance.

123 Cf. Smith and Wise 1989: 250, Ch. 11. On the growth of energy physics, cf. Smith 1998.

or dielectric polarization, was the essence of electricity and always preceded conduction.[124]

Of Faraday's considerations, Thomson retained only the idea that the electrostatic capacity of the cable had to be taken into account. With a sure feeling for legitimate approximations, he provided simple mathematical relations between directly measurable quantities, and ignored the deeper nature of the process. Calling C the capacitance per unit length of the cable, R its resistance per unit length, V the electrostatic potential, and i the current, he set the rate of change of the electric charge on an element $\mathrm{d}x$ of the wire, $C\mathrm{d}x\partial V/\partial t$, equal to the decrease of the current in this element, $-(\partial i/\partial x)\mathrm{d}x$. Using Ohm's law $i = -(1/R)\partial V/\partial x$, this leads to the diffusion equation:

$$RC\frac{\partial V}{\partial t} = \frac{\partial^2 V}{\partial x^2}. \tag{3.3}$$

According to Thomson's solution for a sudden rise of potential at the origin of the cable, 'the time required to reach a stated fraction of the maximum strength of current at the remote end [of a cable of length l] will be proportional to RCl^2.'[125]

This 'law of squares' allowed Thomson to predict the signalling performance of long cables, knowing that of smaller ones. Having computed the capacitance of a cylindrical condenser through the flow analogy, he could also indicate how to minimize the retardation. Such theoretical knowledge was essential for the projected transatlantic telegraph. Thomson did more. He designed and patented high-performance apparatus for the emission and reception of signals. He tested the various components of the cable in his laboratory. And he helped solve the many difficulties encountered in laying a 2000 mile cable at the bottom of the ocean. His methods surprised contemporary engineers, who were accustomed to rough empirical procedures. Yet he soon became a director of the Electric Telegraph Company. The success of the transatlantic cable of 1866—after a first unsuccessful trial in 1858—owed much to his advice. As a reward for this major contribution to the wealth of the British Empire, Queen Victoria knighted Thomson in November of the same year.[126]

Thomson's involvement in British telegraphy meant an important transition in the relations between fundamental science and engineering. Thanks to his efforts, the practical advantages of theoretical knowledge became evident, and precision measurement replaced the previous 'rules of the thumb' of British engineers. In the following twenty years, physics laboratories were created in several academic institutions in order to teach Thomsonian methods to future engineers and physicists. In

[124] Faraday 1854. Cf. Smith and Wise 1989: 446–7; Hunt 1991c. On early submarine telegraphy, cf. Bright 1898; Coates and Finn 1979; Smith and Wise 1989: Ch. 19.

[125] Thomson to Stokes, 28 October 1854; Thomson 1855b. Cf. Whittaker 1951: 227–30; Smith and Wise 1989: 447–53. Thomson neglected electromagnetic induction and leakage. Capacitance was indeed the dominant cause of retardation for the cables he was studying. The more complete 'equation of telegraphy' first appeared in Heaviside 1876 (without leakage), 1881 (with leakage).

[126] Cf. Smith and Wise 1989: 661–83.

1861 Thomson easily convinced the British Association for the Advancement of Science (BAAS) to create a committee on standards of electrical resistance. The precise measurement of electric quantities had great commercial importance for the telegraph industry. Thomson imposed an absolute system of electric units based on the mechanical units of work, time, and length.[127]

The BAAS committee set the electromagnetic unit of resistance, the 'ohmad,' to 10^7 m/s; measured the resistivity of pure silver in this unit by an improvement of Weber's method; built a silver-wire standard with the required resistance; and sold copies of this standard throughout the British Empire. When in 1881 the first international congress on electrical standards was held in Paris, Thomson acted as vice-president and imposed a good deal of the BAAS system. The international units were named ohm, volt, farad, coulomb, and ampere.[128]

One basic duty of the BAAS committee was to determine the ratio c of the electromagnetic to the electrostatic charge unit. As Faraday and Thomson knew, this ratio was essential to the analysis of telegraph cables, for the retardation of signals depended on the combination of an electrostatic effect (inductive capacitance) with an electrodynamic one (ohmic resistance). In Thomson's equation (3.3), the resistance R and the capacitance C must of course be given in the same system of units. However, resistances were naturally measured in electromagnetic units, and capacitances in electrostatic units. In order to compute the retardation RCl^2, Thomson needed to know the conversion factor c. In 1855 he did the reverse, that is to say, he used Faraday's retardation measurements for a rough (unpublished) estimate of c. Later, in the 1860s, with his Glasgow students he measured this important quantity for the BAAS committee, as was already mentioned.[129]

To sum up, practical concerns strongly shaped Thomson's works on electricity, as they did for the rest of his physics. This is manifest not only in his interest in instruments, but also in his theoretical approach. His concept of the electrostatic potential acted as a bridge between the different cultures to which he belonged. It provided interesting theorems for Liouville's *Journal de Mathématiques*; it justified Faraday's electrostatic manipulations; it integrated the engineering notion of mechanical effect; and it met the requirements of German absolute measurement. The notion was precise, efficient, and concrete, and yet ontologically neutral. Poisson's fluid theory of electrostatics and Faraday's reasonings on lines of electric induction could both be translated into potential language, without loss of efficiency.[130] Potentials could be measured by their mechanical effect, whereas electric fluids or the tensions of lines of force remained beyond empirical reach. In brief, the physical potential was the paragon of Thomson's most essential qualities: mathematical power, versatility,

[127] On physics teaching laboratories, cf. Gooday 1990. On the BAAS committee, cf. Smith and Wise 1989: 684–90; Schaffer 1992; Smith 1998: Ch. 13.

[128] Smith and Wise 1989: 690–5; Hunt 1994. For a closer analysis of the implied social processes, cf. Schaffer 1992. For the conflict between German and British notions of standards, cf. Olesko 1996.

[129] Thomson to Airy, 2 February 1855 (c from retardation), quoted in Smith and Wise 1989: 456; Thomson 1860a; *BAR* 39 (1869): 434–8. Cf. Smith and Wise 1989: 455–8, 694; Schaffer 1995.

[130] Cf., e.g., Thomson 1860b: 254–8.

pragmatism, and latitudinarianism. It gradually became an indispensible tool to any one interested in electricity.[131]

3.6 Thomson's magnetic field

3.6.1 *The strained solid*

Thomson excelled at developing ontologically neutral concepts that could be very practical to a variety of users. Yet he did not exclude more speculative representations of electricity and magnetism. These could offer valuable analogies and new techniques for solving problems, even if their physical meaning was uncertain. Since his work on dielectrics, Thomson had admired the consistency of Faraday's theoretical views. The discovery of magneto-optical rotations increased his sympathy for the field conception.

Faraday believed that electric and magnetic forces were propagated through stresses in the intervening medium. He did not try, however, to explain these stresses in terms of specific mechanical strains. In his view, mechanics, especially mathematical dynamics, had no precedence over the broader notions of force and power on which his physics was based. The Scottish-trained Thomson thought differently. In his mind a stress could only be understood by analogy with a strained elastic solid. By a happy coincidence, his friend George Gabriel Stokes completed an elegant study of the elasticity of solids at the time of Faraday's discovery of the magneto-optical rotation.

Stokes's study was based on a new approach to the dynamics of continuous media, with which he derived his famous equation of viscous fluids. Poisson and Navier has already obtained similar equations, starting with the Laplacian picture of molecules interacting by central forces. In conformity with the British tendency to deal directly with elements of the continuum, Stokes 'examined the nature of the most general instantaneous motion of an element of fluid.' Decomposing the velocity differential $d\mathbf{v}$ into its symmetrical and antisymmetrical parts (in the now classical manner), he found that the most general motion was obtained by superposing three dilations or contractions around three othogonal axes (the principal axes of the symmetrical part of $d\mathbf{v}$), and a rotation of angle $\frac{1}{2}\nabla \times \mathbf{v}$. Rotations do not strain the element. The dilations and contractions do, and imply three additional pressures (positive or negative) along the principal axes. From these remarks, Stokes derived the system of stresses acting on an arbitrary surface element, and the resulting equation of motion.[132]

This reasoning and its counterpart for an elastic solid involved a kinematics of continuous media that became instrumental in field mathematics. In particular, the expressions $\partial v_y/\partial z - \partial v_z/\partial y$, etc., which had previously been used only to express

[131] Thomson's potential replaced older notions of tension in three influencial texts: Jenkin 1873, Maxwell 1873a, and Wiedemann 1874. Some conservative electricians fought this evolution: cf. Hong 1994a.

[132] Stokes 1845a: 80. Of course, Stokes used Cartesian coordinates, not vectors.

the condition for the existence of a potential, now indicated a local rotation or twist in the medium if **v** meant a velocity or a displacement. Maxwell had this picture in mind when he introduced the 'curl' of a vector in 1870. So did Thomson in October 1846, when he described three simple kinds of elastic strain that were analogous to the fields of a point charge, of a magnetic dipole, and of a current element.[133]

For the displacement **u** of an incompressible elastic solid, Stokes's equilibrium equations imply that $\Delta\mathbf{u}$ should be a gradient (of pressure). The three following deformations satisfy this condition as well as the condition of incompressibility ($\nabla\cdot\mathbf{u}=0$):

$$\mathbf{u}=\frac{\mathbf{r}}{r^3},\quad \mathbf{u}=\frac{\mathbf{m}\times\mathbf{r}}{r^3},\quad \mathbf{u}=\frac{i d\mathbf{l}}{r}-\frac{1}{2}\nabla\frac{i d\mathbf{l}\cdot\mathbf{r}}{r} \tag{3.4}$$

Thomson identified the first deformation with the electric force of a unit charge, and the curls of the second and third deformations with the magnetic force produced respectively by the magnetic moment **m** and by the current element $i d\mathbf{l}$. In the magnetic case he was inspired by the Faraday effect, which suggested a rotational deformation of the medium. Thus was born the vector potential, of which the magnetic force is the curl. British mechanical analogy produced this concept ten years before German mathematical analysis did.[134]

In a letter to Faraday, Thomson set the limits of his investigation:

> I enclose the paper which I mentioned to you as giving an analogy for the electric and magnetic forces by means of the *strain*, propagated through an elastic solid. What I have written is merely a sketch of the mathematical analogy. I did not venture even to hint at the possibility of making it the foundation of a physical theory of the propagation of electric and magnetic forces, which, if established at all, would express as a necessary result the connection between electrical and magnetic forces.

Again, Thomson avoided commitment to a *physical* explanation of electric and magnetic forces. Yet the new analogy went a little further than the heat-flow analogy. The latter was meant to suggest new theorems and to retrieve some geometrical features of Faraday's view, whereas the strain analogy offered 'a mechanical representation of electric and magnetic forces' that integrated Faraday's notion of stresses in the field.[135] It could be a starting point toward truer mechanical analogies and could ultimately lead to a 'physical theory of the propagation of electric and magnetic forces.' It was a first, mild attack of what Thomson later called his 'ether dipsomania.'[136]

[133] Maxwell 1870: 265; Thomson 1847a. On Maxwell's terminology, see Crowe 1967: 117–39.

[134] Thomson 1847a; For the German vector potential see Kirchhoff 1857b, discussed *supra*, p. 72.

[135] However, the strains described by Thomson imply stresses different from Faraday's, and they do not yield the correct expressions for the mechanical forces acting on charges, magnets, or currents.

[136] Thomson to Faraday, 11 June 1847, in Thompson 1910, Vol. 1: 203–4; Thomson 1847a: title; Lord Kelvin to FitzGerald, 9 April 1896, in Thompson 1910, Vol. 2: 1065. Cf. Smith and Wise 1989: 256–60.

3.6.2 Diamagnetic forces

Thomson promptly returned to more sober physics. The object was the calculation of the mechanical force **f** acting on polarizable small spheres in a magnetic field, in relation with Faraday's recent experiments on diamagnetism. Following Poisson, Thomson replaced the polarized sphere with a magnetic dipole **M**. Then the force acting on the sphere is the sum of the forces acting on the two opposite poles. In symbols, we have

$$\mathbf{f} = (\mathbf{M}\cdot\nabla)\mathbf{H} = \frac{1}{2}\nabla(\mathbf{M}\cdot\mathbf{H}) = \frac{1}{2}k\nabla(H^2), \tag{3.5}$$

where the second and third expressions follow from the irrotational character of the external magnetic force **H** and from the assumption of a linear polarizability k of the sphere. Consequently, the sphere tends to move toward regions of higher magnetic force if k is positive, and the reverse is true if k is negative. Thomson concluded that reverse polarization completely justifed Faraday's law according to which 'a portion of [diamagnetic matter], when under magnetic action, tends to move from stronger to weaker places or points of force.'[137]

Thomson did not speculate on the deeper meaning of this result. He did not publicly espouse Faraday's view that the effect confirmed the physical character of the magnetic lines of force. In a later discussion of the same effect published in 1851, he noted that the variations of $\frac{1}{2}kH^2$ represented a mechanical effect, but did not claim that this effect (energy) was stored in the field. He adopted Faraday's field terminology, but introduced it in a purely operational manner.[138]

3.6.3 A private analogy

In 1847 Thomson tried to generalize his surface-replacement theorem to magnetic forces. One of the problems he examined was: is there a distribution of magnetic force outside a given closed surface so that the normal component of the force **H** immediately outside the surface has a given value (with zero integral, as required by the balance of Northern and Southern magnetic matter)? There is an obvious hydrodynamic counterpart to this problem, obtained by identifying **H** with the velocity of an ideal incompressible fluid (and exchanging the inside and the outside of the surface): is there an irrotational motion of a fluid mass confined within a closed surface whose motion is given? In both cases the mathematical condition determining **H** is that it should be the gradient of a harmonic function.[139]

[137] Thomson 1847b: 493, 497, and Thomson 1850a; *FER* 3: #2418. Cf. Smith and Wise 1989: 261–2.

[138] Thomson 1851a: 475: *ibid.*: 467–8: 'The *total magnetic force at any point* is the force which the north pole of a unit bar-magnet would experience from all magnets which exert any sensible action on it, if it produced no inductive action on any magnet or other body [. . .]. Any space at every point of which there is a finite magnetic force is called "a field of magnetic force" [. . .]. A "line of force" is a line drawn through a magnetic field in the direction of the force at each point through which it passes.'

[139] Thomson notebook, 29 March 1847, quoted in Smith and Wise 1989: 263–4; Thomson to Stokes, 20 October 1847, in Wilson 1990.

Thomson used this reformulation of the magnetic problem in order to bring the question to Stokes, the expert on hydrodynamics. Stokes immediately gave a positive answer. Parallely, Thomson found that Gauss's method of quadratic forms could be extended to this case: the solution exists because it is given by the function **H** for which $\int H^2 d\tau$ is a minimum with the given boundary conditions. In the hydrodynamic problem, this makes the kinetic energy of an irrotational motion a minimum. Thomson used the remark to simplify the proof of some hydrodynamic theorems.[140]

More generally, Thomson found a minimum principle that comprehended all useful existence theorems for hydrodynamics, heat theory, electrostatics, and magnetism. These problems admit a potential V, which satisfies the generic equation

$$\nabla\cdot(\alpha^2\nabla V) = -4\pi\rho. \tag{3.6}$$

The variable parameter α^2 corresponds to the conductivity in heat theory, to the dielectric constant in electrostatics, and to the permeability in the case of magnetism; in hydrodynamics, abrupt variations of this parameter can be used to simulate the boundaries of the fluid. This equation always has a solution, Thomson showed, because it corresponds to the minimum of the quadratic form

$$Q = \int\left(\alpha\nabla V - \frac{1}{\alpha}\nabla U\right)^2 \mathrm{d}\tau, \tag{3.7}$$

where U is the solution for $\alpha = 1$ (which is already known).[141]

It would be tempting to think that the hydrodynamic analogy led Thomson to regard $\frac{1}{2}H^2$ as representing the actual energy distribution in the magnetic field. Yet he did not say so. In his eyes, an essential advantage of considerations of mechanical effect was that they did not depend on the internal make up of physical systems. They made any system a black box, an engine with input and output. Moreover, Thomson could not take the fluid analogy so seriously as to make **H** a linear velocity, because this would have contradicted his intuition of the Faraday effect, according to which **H** meant a local twist of the medium. And he did not know yet how to extend the analogy to induced magnetism. For all these reasons, he did not publicize the hydrodynamic analogy. He published his results either as mathematical theorems or as hydrodynamic laws.[142]

Thomson's major memoir on magnetism of 1849 was quite positivist in tone. There he refrained from any assumption on the nature of magnetism, and avoided

[140] Stokes to Thomson, 10 April 1847, in Wilson 1990; Thomson to Stokes, 20 October 1847; Thomson 1849. Cf. Smith and Wise 1989: 263–2.

[141] Thomson 1848c. Cf. Smith and Wise 1989: 271.

[142] Thomson 1848c, 1849. However, Thomson published the hydrodynamic analogy in 1872a: 455–9, and generalized it to induced magnetism in 1872a: 578–87 (the magnetic 'permeability' being so named in analogy with the permeability of the porous medium in which the fluid circulates: cf. Thomson 1872a: 484).

both magnetic fluids and Amperean currents. He based his theory on the phenomenological notion of elementary magnetic moments, and defined the magnetic force operationally. To measure in thought the magnetic force within the substance of a magnet, he carved out a thin crevasse in the direction of the polarization, and inserted a test unit pole. He introduced magnetic charges only as mathematical aids, defined as the convergence of the polarization ($-\nabla \cdot \mathbf{M}$). In private he also used the equivalent currents given by the curl of the magnetization ($\nabla \times \mathbf{M}$), but did not publish that until the 1870s (after he had adopted Ampère's hypothesis).[143]

Thomson's main concern was a geometrical analysis of the different kinds of magnetic polarization and the resulting magnetic fields. In this context he introduced the distinction between solenoidal and lamellar distributions. For the former kind, the magnet can be decomposed into infinitesimal tubes (σωλεν in Greek) with longitudinal polarization. For the latter, the magnet can be decomposed into lamellar sheets with transverse polarization. The corresponding mathematical conditions are $\nabla \cdot \mathbf{M} = \mathbf{0}$, and $\nabla \times \mathbf{M} = \mathbf{0}$. Of course, in the first case Thomson had in mind an incompressible fluid and its tubes of flow. But he relegated the analogy to a footnote.[144]

3.6.4 Mediation

As a consequence of Faraday's developing view of magnetism, Thomson ended up releasing important aspects of the flow analogy. In a letter written in June 1849 he explained to Faraday why an elongated diamagnetic body in a uniform magnetic field should orient itself in the direction parallel to the magnetic force, using Faraday's concept of conducting power for the lines of force. At the 1852 meeting of the British Association, he showed pretty diagrams of lines of force (Fig. 3.22) which he had calculated by a method previously developed in the context of heat theory. He called attention to the remarkable resemblance of these diagrams to those Faraday had recently shown at the Royal Institution to explain his views on diamagnetic action, and claimed to have justified by rigorous mathematical analogy expressions such as 'the conducting power for the lines of force.' He could have added that his minimum principle of 1847 (Eqn. 3.7) could be interpreted as a principle of least resistance, in conformity with Faraday's intuition: the flux corresponding to the conductivity α^2 is indeed $\alpha^2\nabla V$, while the flux for a unit conductivity is ∇U.[145]

Thomson's use of analogy was here quite similar to his previous use of the heat analogy to make sense of Faraday's electrostatics. For any magnetic phenomenon, he could translate an explanation in terms of elementary polarizations acting at a distance into another explanation in terms of Faraday's conducting power for the

[143] Thomson 1849–50: 340, 361–2; Thomson 1872a: 424–5. Cf. Smith and Wise 1989: 279–81. Thomson adopted the Amperean currents after his 1856 analysis of the Fadaday effect (see below): cf. Thomson 1872a: 419n.

[144] Thomson 1849–50: 378–92.

[145] Thomson to Faraday, 19 June 1849 (or 1847: cf. Wise 1981: 59n), in Thompson 1910, Vol. 1: 214; Thomson 1848a (equilibrium of diamagnetic bodies); Thomson 1852 (BA), 1847c (magnetic curves computed), 1843 (heat flow); Thomson 1852: 515 (quote).

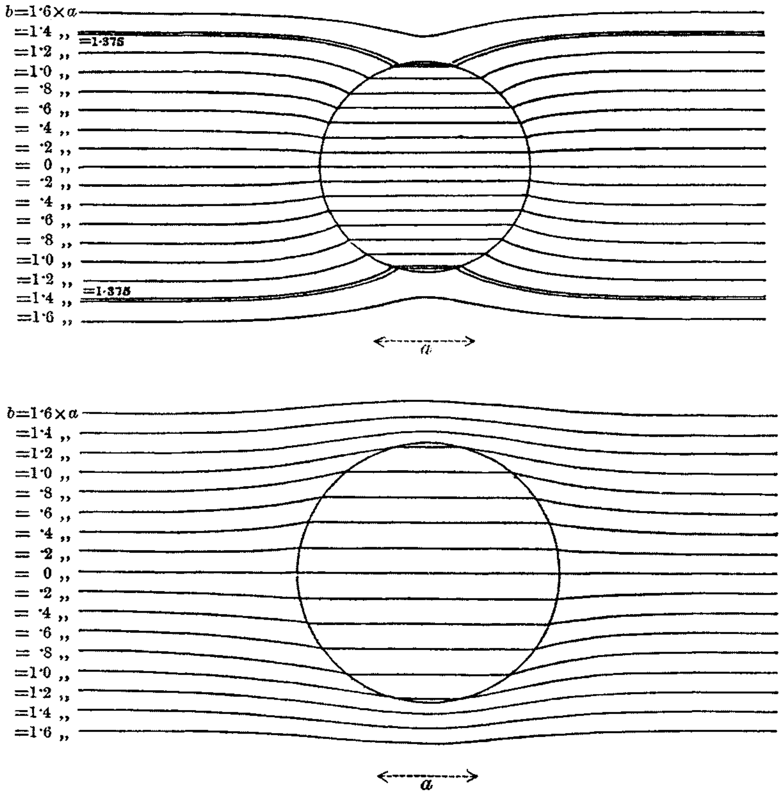

FIG. 3.22. Thomson's computed lines of force around a paramagnetic (above) and a diamagnetic (below) sphere (*TPEM*: 490–1).

lines of force. Where Faraday saw a contradiction, he perceived an exact mathematical equivalence:

> All that Tyndall has done in verifying Weber [. . .] is mere illustration or verification of a conclusion following equally from Faraday's theory, or from the arbitrary assumption [. . .] that a diamagnetic experiences a reverse effect (polarization) throughout its substance, to that experienced by a paramagnetic.

Being perfectly fluent in both languages, Thomson adressed the proponents of the opposite views in their own terms. To Faraday, he explained the orientation of a diamagnetic bar in a uniform magnetic field as an effect of least resistance to the passage of lines of force; to Tyndall he explained the same effect by the mutual interaction of elementary diamagnets.[146]

[146] Thomson, notebook, 6 January 1858, reproduced in Knudsen 1971: 50; Thomson to Faraday, 19 June 1849, in Thompson 1910, Vol. 1: 214; Thomson to Tyndall, 12 March 1855, in Thomson 1872a: 535–8.

FIG. 3.23. Rotation of the plane of polarization of light traveling through a helix for two opposite directions of propagation.

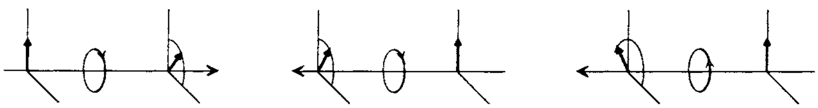

FIG. 3.24. Rotation of the plane of polarization of light traveling through a current loop. The case (b) derives from (a) by inverting the direction of propagation; (c) from (a) by time-reversal.

3.6.5 Molecular vortices

Thomson's neutrality had limits, however. Since his conversion to the kinetic theory of heat around 1850, he was more inclined to speculations on a general theory of ether and matter. In 1856 he had a serious relapse of 'ether dipsomania.' The cause was again the Faraday effect. The rotary power of optically active substances like turpentine could easily be explained by an helicoidal asymmetry of the molecules of the substance: the polarization plane of a light beam should then rotate in the direction defined by the helicity (right- or left-handed) (Fig. 3.23(a)). This explanation could not be extended to the Faraday effect, because it implied that the optical rotation, as seen from the light source, did not depend on the direction of propagation (Fig. 3.23(b)). As Faraday emphasized, the magnetically induced rotation was reversed when the direction of propagation of light was reversed (Fig. 3.24(a),(b)). Consequently, the modification of the medium that was responsible for the rotation had the asymmetry of an oriented circle. Faraday imagined some microscopic rotations in the medium. These rotations could be either static (twist) or dynamic (continuing motion).[147]

In his mechanical representation of magnetic forces of 1847, Thomson flirted with the first possibility. Yet by 1856 he was convinced that the dynamic option was the only one available. Using a general argument and a mechanical model, he explained how a microscopic rotational motion of the medium could affect the polarization of light, and he asserted that there was no other possible explanation of the symmetry properties of the Faraday effect. Unfortunately, he gave no strict proof of this crucial point. A generous reading of his obscure argument leads to the following consideration. By time reversal (Fig. 3.24(c)), the rotation of the polarization of

[147] Thomson 1856; *FER* 3, series 19: ##2231–2. Cf. Knudsen 1976: 244–7, 273–6.

the light beam (with respect to a fixed observer) is reversed. Therefore, the responsible rotation in the medium should also be reversed, which proves its dynamic nature.[148]

In Thomson's new analysis the Faraday effect demonstrated the existence of Amperean currents—which he had previously denied—and it suggested a magnetic exploitation of Rankine's kinetic theory of heat. According to the Scottish engineer, heat was nothing but the rotational motion of space-filling 'molecular vortices.' According to Thomson, magnetization could be an alignment of the vortices, and the common angular momentum would determine the magnetic moment. Strictly speaking, these pictures only applied to matter, heated and magnetized. However, Thomson had reasons to extend them to the ether.[149]

In 1854 he inferred a lower limit for the density of the ether from the mechanical effect of a cubic mile of sunlight, and suggested that the ether was only 'a continuation of our atmosphere.' In a letter to Tyndall of March 1855, he argued that since vacuum had 'perfectly decided mechanical qualities,' it probably had the magnetic property as well. In short, the ether was only a dilute form of matter. Accordingly, Thomson accompagnied his 1856 discussion of the Faraday effect with a vague but bold suggestion:[150]

> The explanation of all phenomena of electro-magnetic attraction or repulsion, and of electro-magnetic induction, is to be looked for simply in the inertia and pressure of the matter [ponderable or not] of which the motions constitute heat. Whether this matter is or is not electricity, whether it is a continuous fluid interpermeating the spaces between molecular nuclei, or is itself molecularly grouped; or whether all matter is continuous, and molecular heterogeneousness consists in finite vortical or other relative motions of contiguous parts of a body; it is impossible to decide, and perhaps in vain to speculate, in the present state of science.

Be it in vain or not, in a notebook entry of 1858 Thomson did speculate on a general picture of ether and matter. He imagined a universal fluid with myriads of rotating motes which could perhaps be further reduced to permanent eddies. The gyrostatic rigidity of the motes or eddies would permit transverse vibrations of the medium, to be identified with light. Heat would be the rotation of the eddies. Electricity would correspond to the less disturbed parts of the fluid between the motes. Then an electric current would alter the rotation of the eddies, as a string pulled between two adhering wheels. This connection would account for the Joule effect, and for magnetism, understood as an alignment of the eddies' axes. Magnetic attractions would result from the centrifugal force of the eddies combined with the pressure of the fluid. Electromagnetic induction would correspond to the storage of momentum in the oriented vortical motions. Lastly, the Faraday effect would result from the influence of these motions on transverse vibrations of the medium. Thomson ended with a prophetical remark: 'A complete dynamical

[148] For a closer analysis of Thomson's argument, cf. Knudsen 1976. [149] Thomson 1856: 571.
[150] Thomson 1854; Thomson to Tyndall, 12 March 1855, *TPEM*: 535; Thomson 1856: 571. Cf. Smith and Wise 1989: 397–402 (ether), 407–8 (Faraday effect).

illustration of magnetism and electromagnetism seems not at all difficult or far off.'[151]

This was Thomson's first attempt to understand all physics in terms of fluid vortices. Soon he found support in Helmholtz's theorems on vortical motion in an ideal incompressible fluid. For many years he tried to model ether and matter as arrays of vortices. He did not, however, pursue the idea of a vortex-based illustration of electromagnetism. This was left to his most gifted admirer, James Clerk Maxwell.[152]

3.7 Conclusions

Faraday and Thomson invented field theory: they introduced theoretical entities in the space between electric and magnetic sources, and they elaborated powerful techniques for investigating the properties of these entities. They perceived a convergence of their projects and developed a mutual admiration. However, their interests, methods, and concepts were extremely different.

In Chapter 1, we saw how Faraday's first electrodynamic studies depended on the systematic experimental exploration of the 'power' emanating from magnetic sources. He refused to speculate on the internal structure of sources, and focused instead on the intervening space, in which observed actions were regarded as a manifestation of 'magnetic power.' His explorations connected actual and virtual actions, and thus generated mappings of the power, new power-induced states of matter, and rules for the development of these states. None of this required Thomson's advanced mathematics: ordinary language and intuitive geometry sufficed. In fact, Faraday's exploring frenzy resulted in part from his distrust in established mathematical theories. The efficiency of his explorations largely depended on his qualitative concept of power.

In his later works on electrolysis, electrostatics, and diamagnetism, Faraday extended the approach of his earlier researches. In each case, he gleaned new facts and shaped original views in non-mathematical language. His exceptional attention to processes in the intervening space or matter between sources led to his discovery of dielectric and diamagnetic effects. It also instructed his redefinition of charge and current in terms of the cessation of dielectric polarization, and his notion of conducting power for the magnetic lines of force. With these field concepts he was able to predict and explain effects which, in received theories, could not be foreseen without sophisticated mathematics.

Faraday's commitment to the physical existence of field entities evolved in time. At the beginning of his experimental researches he sometimes used wordings and concepts that suggested the reality of the lines of force. The earliest example is his

[151] Thomson notebook, 6 January 1858, reproduced and commented in Knudsen 1971. Cf. also Smith and Wise 1989: 410. The interpretation of induction is not in the notebook, but may be inferred from the remark in Thomson 1856, quoted above.

[152] Cf. Smith and Wise 1989: Ch. 12.

concept of the repulsion between parallel magnetic curves, foreshadowed in the rotations paper of 1821.[153] However, he long resisted the temptation to close the issue. In public he maintained an operational definition of the lines, and did not defend their reality until he had accumulated many favorable arguments and reached the age of unconditional respectability. His arguments of the mid-1830s only concerned the effect of matter on the transmission of force: he believed he had proved that the electric force from a given particle of matter could only reach the nearest particles, more remote actions being indirect, through chains of contiguous particles. To the extent that they referred to polarization in chains, the electric lines of force were real. However, they could also travel across the intermolecular vacuum, in which case Faraday had no proof of their physical character.

The problem of transmission across a vacuum became more acute after the discovery of diamagnetism. With respect to the transfer of magnetic action, vacuum was intermediate between dia- and paramagnetic matter and thus seemed to be on the same footing as matter. Also, the proofs of action between contiguous particles did not transfer smoothly from the electrostatic to the magnetic case. Yet Faraday's confidence in the physical reality of the magnetic lines of force increased as he multiplied the varieties of their uses. He could 'touch' them and 'illuminate' them, at least metaphorically. He could modify their course by means of better or worse 'conductors.' Thus he meant them to exist independently of any ponderable or imponderable medium. In his most daring dynamicist speculations, there was nothing but force, distributed in space with variable qualities and intensities.

According to a widespread misinterpretation, from the beginning Faraday's researches were motivated by the elimination of direct action at a distance. In reality, he regarded an interaction via lines of force as direct action at a distance, whenever no matter contributed to the transmission of the force. He expected the interaction to take time in such cases, but not because a subtle medium or ether was involved. The reason of the retardation was the physical nature of the lines of force. In short, his notion of force transcended the usual dichotomy between direct action and action through a medium.

According to another misinterpretation, Faraday's researches were aimed, from the beginning, at confirming Boscovich's atomist dynamism.[154] Admittedly, Faraday's 'power' and 'force' were dynamicist notions that denied the distinction between action and agent (Newtonian force and imponderable fluid) and eventually the distinction between force and matter. But there is no evidence that Faraday supported a specific dynamicist philosophy, not even Boscovich's. As far as his experimental and conceptual practices were concerned, the focus on power and force only meant the endeavor to express phenomena in terms of virtual actions in intervening spaces. Faraday freely explored the fields of action and the correlations of different

[153] More exactly, in 1921 Faraday spoke of the repulsion between similar magnetic powers in the space between two antiparallel currents. See *supra*, p. 20.

[154] Cf. Williams 1965 for the thesis and Spencer 1967 for the refutation. Levere 1968 denies Davy's and Faraday's interest in speculative metaphysics, but documents their religious inclination toward center-of-force atoms.

powers, and gradually formed his theoretical views in this process. He suspended his opinion on the physical character of the lines of force until exploration ceased to learn him more.

Faraday had no mathematical or mechanical preconceptions, and his theory mostly reflected patient experimental explorations. In contrast, Thomson was originally a mathematician with a strong background in analytical mechanics. His practical bent did not result from familiarity with the laboratory, but from Scottish Common Sense philosophy and interactions with engineers.

Thomson's analogy between electrostatics and heat flow was originally meant to transfer theorems. Implicitly, it also provided new mathematical structures in the space between conductors. Thomson did not wish, however, to commit himself to any specific physical interpretation of these structures. He only showed how his analogy could connect two possible interpretations, Coulomb's and Faraday's. He generally avoided metaphorical concepts that had no direct empirical counterpart. To a large extent, the same remarks apply to his later analogies between magnetism and hydrodynamics.

Aware of his role as a mediator in the cultural complex of mathematics, experimental philosophy, engineering, and geophysics, Thomson forged multi-purpose concepts that transcended cultural barriers and individual theoretical preferences. The most important and successful of these concepts, the physical potential, belonged equally in mathematical theory, electrostatic experiment, engineering considerations of mechanical effect, Gaussian absolute measurability, and the later industrial design of voltmeters. Physicists conversant with French electrostatics could easily express the potential in terms of electric fluid densities. The followers of Faraday's views, if any, could draw the lines perpendicular to the equipotential surfaces and call them lines of force. Energeticists could adopt Thomson's definition of the potential in terms of mechanical effect.

After his major contributions to thermodynamics around 1850, Thomson grew more interested in speculations on the ultimate nature of heat, electricity, and magnetism. He also started to support some aspects of Faraday's new field physics. However, his approach still differed widely from Faraday's. He believed in a mechanical ether, namely a dilute form of matter to which the mechanics of continuous media applied, and of which Faraday's lines of force represented local strains or motions. Inspired by the Faraday effect, Rankine's kinetic theory of gases, and Stokes's hydrodynamics, he figured the ether as an ideal incompressible fluid in which arrays of molecular vortices would represent magnetic fields. All of this was tentative and illustrative. Yet Thomson's hope to reduce all physics to motions in an ultimate medium was well anchored. In particular, he believed that a dynamical illustration of electromagnetism was close at hand.

4

Maxwell

4.1 Introduction

War es ein Gott, der diese Zeichen schrieb?[1] So asks Boltzmann, quoting from Goethe, in an epigraph to his lectures on Maxwell's theory. In the nineteenth century section of the physicists' pantheon, Maxwell's rank remains the highest. The tribute is well deserved. Maxwell wrote the field equations which still form the basis of our understanding of electromagnetism. He subsumed optics under electromagnetism. He founded statistical physics. He created a new style of theoretical physics. As the first director of the Cavendish Laboratory, he contributed to the increasing sophistication of British experimental physics.

Glorification, however, tends to obscure the true nature of Maxwell's achievements. It was not a god who wrote these signs, but a man who had gone through two of the best British universities and had carefully studied Faraday and Thomson for himself. His electromagnetism and his style of physics, innovative though they were, owed much to Thomson, who had already transformed British physics in an even more significant manner and had defined basic concepts and new perspectives of electromagnetism. The heroic account also deforms Maxwell's results. His electrodynamics differed from today's 'Maxwell's theory' in several respects, as basic as the distinction between source and field. It was not a closed system, and it included suggestions for future electromagnetic research. In the present chapter, we will approach this more authentic Maxwell.

4.1.1 Scot and wrangler

James Clerk Maxwell was, like Thomson, a Cambridge graduate first trained in a Scottish university. Despite a seven-year difference in age, the two men's approaches to physics had deep similarities. They both lent a central role to geometry in the expression of mathematical and physical ideas. Following their Scottish professors (John Nichol for Thomson, and James Forbes for Maxwell), they held a broad view of physics, including the full range of experimental subjects and technical

[1] 'Was it a god who wrote these signs?' Boltzmann 1891–1893, Vol. 1: 96, from the introductory monologue of Goethe's *Faust*.

engineering problems. At the same time, they shared the mathematical virtuosity cultivated in the Cambridge Tripos, and had an eye for deeper theory as promoted by John Herschel and William Whewell. By drawing formal analogies between various branches of physics, they combined Baconian diversity and Newtonian unity.[2]

There were, however, perceptible nuances between Thomson's and Maxwell's research styles. Maxwell's involvement in technical, practical matters was less than Thomson's, while his interest in geometry was more sustained and diverse than Thomson's. Following the Clerk family's artistic bent, Maxwell was fascinated by the beauty of geometrical figures. After William Hamilton and Immanuel Kant, he regarded space and time as necessary forms of our intuition of phenomena. His interests and skills in philosophy and literature were exceptionally high for a British scientist. Unlike Thomson, he accompanied his use of dynamical analogies with sophisticated philosophical comment. He wrote good poetry, and brilliantly discussed moral philosophy for the Cambridge Apostles. Lastly, there was an essential psychological difference between Maxwell and Thomson. As an enthusiastic prodigy, Thomson launched essential ideas in numerous concise papers, but rarely found time for their full exploitation or for global syntheses. Maxwell was slower and more dependent on other physicists' innovations, but he could persevere several years on the same subject and erect lofty monuments.[3]

Maxwell first learned electricity and magnetism from James Forbes at Edinburgh University. Forbes adopted an empirical approach, and ignored French or German mathematical fluid theories. Maxwell was still free of theoretical prejudice when in February 1854 he asked his pen-friend William Thomson: 'Suppose a man to have a popular knowledge of electrical show experiments and a little antipathy to Murphy's Electricity [the British rendering of Poisson's electrostatics], how ought he to proceed in reading & working so as to get a little insight into the subject which may be of use in further reading?' Thomson's reply is lost. We know, however, that Maxwell read Faraday and Thomson first, then Ampère and Kirchhoff, and lastly Neumann and Weber. Thus, the young Maxwell assimilated Faraday's field conceptions and developed a distaste for continental theories. He later explained to Faraday: 'It is because I put off reading about electricity till I could do without prejudice, that I think I have been able to get hold of some of your ideas, such as the electro-tonic state, action of contiguous parts &c.'[4]

[2] On Maxwell's biography, cf. Campbell and Garnett 1882; Everitt 1975. For the relative effects of Maxwell's Scottish and Cambridge backgrounds, cf. Wilson 1985, Siegel 1991, and Harman 1998. On Cambridge's Mathematical Tripos, cf. Wilson 1982; Warwick [1999].

[3] On Maxwell and geometry, cf. Harman 1990: 2–3; Harman 1995a: 20–2, 28–9; Harman 1998: 13–15. On Maxwell, Scottish common sense, and Kant, cf. Harman 1985b, 1998: 27–36, and Hendry 1986. For the psychological comparison between Maxwell and Thomson, cf. Everitt 1975: 59–60.

[4] Maxwell to Thomson, 20 February 1854, *MSLP* 1: 237; Maxwell to Faraday, 19 October 1861, *MSLP* 1: 688. When he wrote to Thomson on 13 November 1854 (*MSLP* 1: 262), Maxwell had read Ampère and Kirchhoff, but not Neumann and Weber. He had read Thomson 1849–1850 (mathematical theory of magnetism) before his letter of February 1854 (cf. Harman 1998: 72–3). His interest in electricity was unusual for a Cambridge student, for this subject had been excluded from the Tripos curriculum some years before.

4.2 On Faraday's lines of force

4.2.1 Gridding the field

Before the end of 1854, Maxwell reported substantial progress to Thomson. Following Faraday, he defined the lines of force as the lines everywhere tangent to the force acting on a pole or point charge. Following Gauss and Thomson, he also introduced the surfaces normal to these lines, that is, the equipotentials. His first innovation was to consider simultaneously the lines and the surfaces and to regulate their spacing, in order to allow quantitative geometrical reasoning (Fig. 4.1). He had used similar space-gridding a few months earlier in a discussion of surface folding, and all his previous works involved the geometry of lines or surfaces.[5] In the electric or magnetic context, he required that the potential difference between two successive equipotentials should be a constant. On a given equipotential surface he drew two systems of curves defining cells with a size inversely proportional to the intensity of the electric or magnetic force, and then traced the tubes of force passing through these cells. The tubes played the same role as Faraday's unit lines of force.[6]

Maxwell expressed Faraday's law of electromagnetic induction in terms of the tubes. In the case of a closed circuit, the induced electromotive force depends on the decrease of the number of tubes passing through it. In mathematically precise terms, *the induced electromotive force around a circuit is equal to the decrease of the surface integral of magnetic force across any surface bounded by the circuit.* Maxwell immediately applied the law to a simple analytical case, the induction of currents in a conducting sphere rotating in the magnetic field of the Earth. Twenty years after the discovery of electromagnetic induction, he was the first theorist to take Faraday so seriously as to give a mathematical expression of his induction law.[7]

Maxwell's geometrical representation also helped him reformulate the relation between an electric current and the resulting magnetic field. In his vision, a current in a closed circuit determined a series of equipotentials bounded by the circuit (Fig. 4.2). The number of these equipotentials was a natural geometrical characteristic

[5] Maxwell to Thomson, 13 November 1854, *MSLP* 1: 258; Maxwell [1854a]: 252. Maxwell was aware of Thomson's theory of magnetization (1849–50), which introduced lamellar and tubular analysis of the distributions of magnetism in magnets. In my reconstruction, I assume that Maxwell had the line–surface gridding before he considered the relation between current and magnetic force. The essential point, however, is that he *simultaneously* considered the potential theory of magnetism and Faraday's lines of force.

[6] Originally, Maxwell spoke of lines of polarization instead of tubes of force. On the genesis and meaning of 'On Faraday's lines of force' I have found much inspiration in Norton Wise's insightful paper on 'the mutual embrace' (Wise 1979). Particularly important are his comments on Maxwell's field-geometrical method and on the role of the intensity/quantity distinction.

[7] Maxwell to Thomson, 13 November 1854, *MSLP* 1: 260; *ibid.*: 260–1, and Maxwell 1862: 226–9 for the rotating sphere. At that stage Maxwell did not have yet the distinction between force and flux (intensity and quantity). He used the term 'polarization' (which I have replaced with 'magnetic force') 'to express the fact that at a point of space the south pole of a small magnet is attracted in a certain direction with a certain force' (*MSLP* 1: 256).

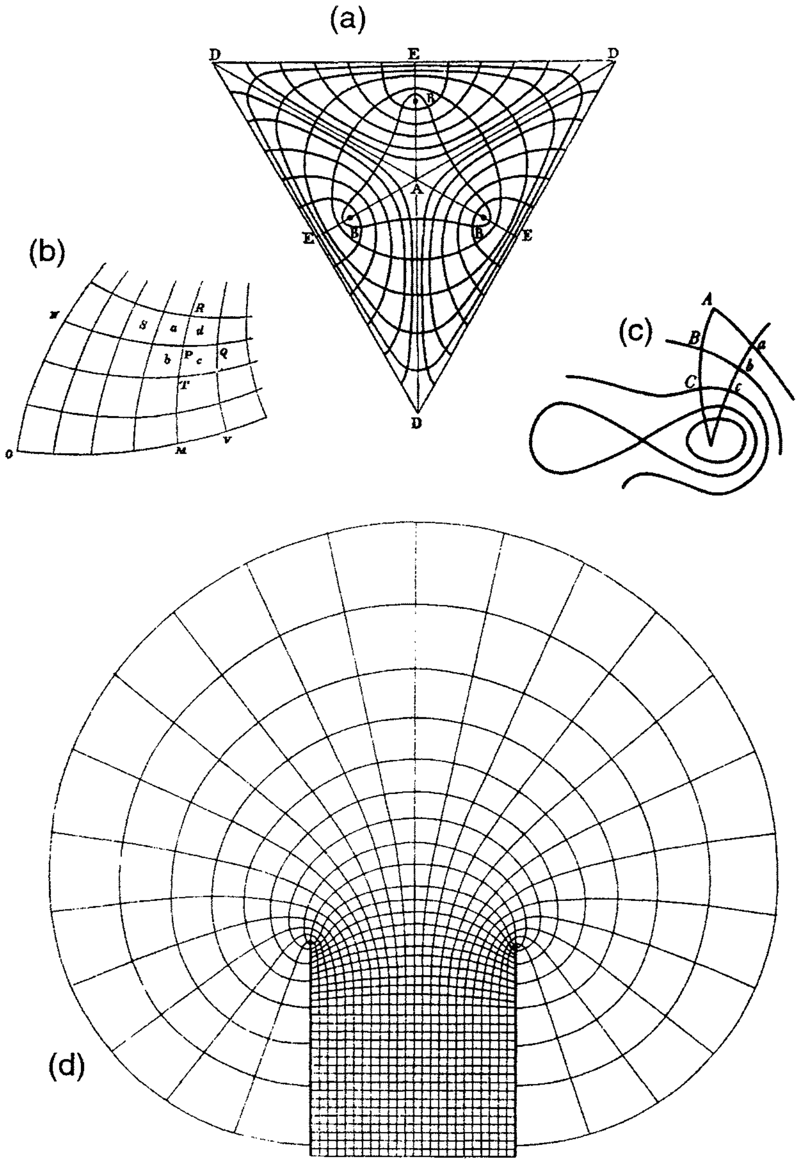

FIG. 4.1. Some of Maxwell's geometrical grids: (a) compression and dilation lines of a glass triangle (Maxwell 1850: 68), (b) lines of surface bending (Maxwell 1854b: 99), (c) electric lines of force and equipotentials (Maxwell [1854]: 252, used by permission of Cambridge University Press), (d) *idem* for a two-plate condensor (Maxwell 1873a: plate 12).

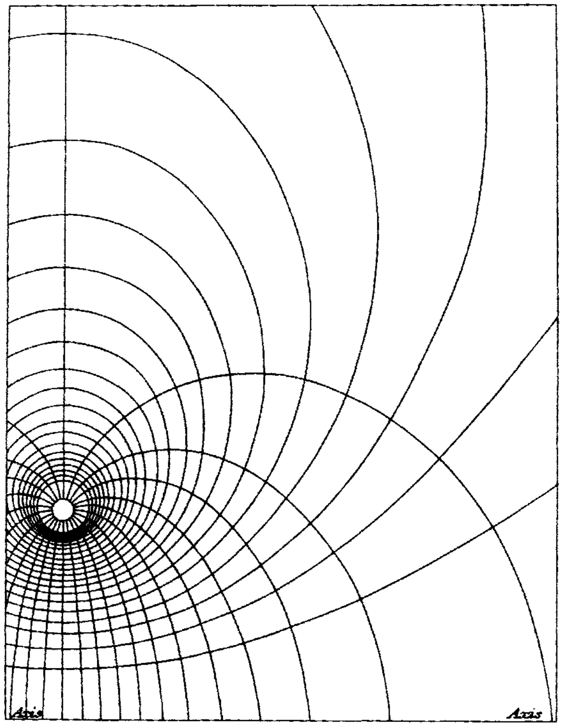

FIG. 4.2. Magnetic lines of force and equipotential surfaces of a circular current, in a half-plane delimited by the axis of the circle (Maxwell 1873a: plate 18).

that obviously depended on the intensity of the current.[8] It also had an energetic meaning, as the work performed by a unit magnetic pole on a curve γ embracing the circuit. In order to determine this number, Maxwell resorted to Ampère's equivalence between a circuit C and a net of contiguous loops (Fig. 4.3) and reasoned as follows.

If the small shaded loop embracing the curve γ were removed, the remaining loops would be equivalent to a double magnetic sheet with a hole at the place of the shaded loop. The corresponding potential would be single-valued, and its total variation on γ would be zero. Consequently, the line integral of the magnetic force, or the number of equipotentials, depends only on the current circulating in the shaded loop, which is equal to the current in C.[9] With a Gaussian eye for topological relations, Maxwell insisted that the integration curve and the current curve had to embrace each other.

[8] This number is well-defined for a proper choice of the potential unit.

[9] The numerial coefficient is determined by considering a particular case, for instance a circular current and its axis regarded as a curve closed at infinity.

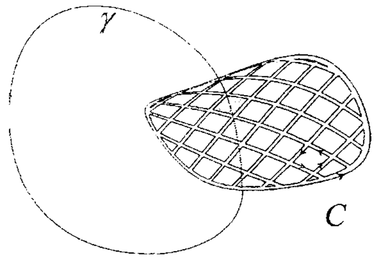

FIG. 4.3. Amperean net and mutually embracing curves for Maxwell's first proof of the magnetic circuital law.

In general, *the line integral of the magnetic force on any closed curve is measured by the sum of the intensities of the embraced currents.*[10]

Maxwell was first to enunciate this result, which is improperly called the Ampère law (or theorem).[11] Together with the induction law, it formed the basis of his own field theory of magnetism. William Thomson was no doubt aware of these two laws.[12] However, they did not appear as explicit, central statements in his papers. Having started from action-at-a-distance theory and energetic considerations, he gave central importance to the potential concept. In contrast, Maxwell started with Faraday's lines of force and expressed fundamental laws directly in terms of the field of force. He regarded the equipotentials as derivative constructs, defined as the surfaces orthogonal to the lines of force, even though they played a role in his derivation of the Ampère law and in his discussion of field energy.

4.2.2 The resisted-flow analogy

In the same letter to Thomson, Maxwell applied his line–surface geometry to conduction currents: here the lines refer to electric motion, and the surfaces to equal tension. He also suggested an analogous treatment of induced magnetism, based on Faraday's notion of conductive power for the magnetic lines of force. In the general

[10] Maxwell to Thomson, 13 November 1854, *MSLP* 1: 256–7. Maxwell also stated another theorem: the integral of the magnetic force across a surface bounded by the circuit only depends on the intensity of the current (and on the shape of the circuit, Maxwell should have added), not on the shape of the surface (*MSLP* 1: 257). This results from the equivalence of the circuit with a double magnetic sheet and from the fact that the integral of the magnetic force produced by magnetic masses is zero over any closed surface that does not contain masses, as Maxwell noted a little earlier in his letter. Cf. Wise 1979 and Hendry 1986: 126–30.

[11] I avoid the expression 'Ampère's law,' which is even more misleading.

[12] Thomson was certainly aware of the Ampère law, as appears from his discussion of the potential of a closed current (Thomson 1850b: 426n). However, he did not state it formally, presumably because a true field formulation of electromagnetism was not on his agenda. Regarding electromagnetic induction, Thomson had used Faraday's law and had given its mathematical expression in a particular case (Thomson 1851c: 484). Maxwell knew these papers very well, so he asked Thomson whether he had not 'the whole draught of the thing [Maxwell's "On Faraday's lines of force"] lying in loose papers' (Maxwell to Thomson, 13 September 1855, *SLMP* 1: 322).

case of variable conductivity, however, Maxwell did not know how to prove the existence of the potential. In 1848 Thomson had published a strict but enigmatic proof, based on minimizing a certain positive integral (see p. 129). Maxwell wondered whether his correspondent had a general theory based on the theorem. The answer is lost. In any case, by the spring of 1855 Maxwell was elaborating on the flow analogy that Thomson had so successfully applied to existence theorems. 'Have you patented that notion with all its applications?, for I intend to borrow it for a season,' he wrote Thomson.[13]

Maxwell's resulting analogy, published in the first part of 'On Faraday's line of force,' departed from Thomson's original heat analogy in several respects. Maxwell replaced heat with an 'imaginary incompressible fluid,' arguing that it would provide a more concrete analogy, since heat was no longer regarded as a substance. He treated the most general case of heterogenous and anisotropic conduction, whereas Thomson had mostly confined himself to the homogenous case. Most important, Maxwell integrated his tubes-and-cells geometry in the analogy and thus increased its intuitive appeal and demonstrative power. His aim was to produce a method that 'required attention and imagination but no calculation.'[14]

Maxwell first described the uniform motion of an incompressible and imponderable fluid through a resisting medium with sources and sinks. He parted the fluid into unit tubes, in which one unit of volume passes in a unit of time. The configuration of the tubes completely defines the flow, since their direction gives that of the fluid motion, and their inverse section determines the velocity. Maxwell further assumed the resistance of the medium (a porous body) to be proportional to the fluid velocity. Since the motion is uniform and the fluid has no mass, this implies that the velocity is proportional to the gradient of pressure, as Fourier's heat flux is proportional to the gradient of temperature.[15]

With this illustration, Maxwell proved essentially the same theorems as Thomson had done with the heat-flow analogy. He did not quite meet his aim to prove the existence of the potential—or pressure—in the case of a heterogenous medium.[16] But his reasonings, being based on the geometry of the tubes of flow, were more direct and vivid than Thomson's. For example, he obtained the surface-replacement theorem by the following simple consideration: the flow outside an imaginary closed surface is unchanged if we substitute for the fluid inside the surface a system of sources and sinks on the surface that maintain the flow in each intersecting tube.[17]

[13] Maxwell to Thomson, 13 November 1854, *MSLP* 1: 259–61; Maxwell to Thomson, 15 May 1855, *MSLP* 1: 307. Maxwell also thought of relating the work of electrodynamic forces to the number of cells in the field (*MSLP* 1: 259).

[14] Maxwell [1855]: 306; Maxwell to Stokes, 22 February 1856, *SLMP* 1: 403; Maxwell 1856b: Part I. Cf. Rosenfeld 1956: 1652–5; Heimann 1970; Everitt 1975: 87–93; Moyer 1978; Wise 1979; Hendry 1986: 133–8; Harman 1990: 12–15; Siegel 1991: 30–3.

[15] Maxwell 1856b: 160–4.

[16] He only proved that if the potential flow exists in a heterogenous medium, then it may be regarded as created by an imaginary system of sources spread in a homogenous medium (Maxwell 1856b: 168–71).

[17] Maxwell 1856b: 168 (#20).

Maxwell also introduced 'surfaces of equal pressure' such that a unit pressure difference exists between two consecutive surfaces. He used the cells determined by the intersection of these surface with the tubes of flow to express the energy spent by the fluid to overcome the resistance of the porous medium. In a given cell, a unit mass of fluid experiences a pressure decrease of one unit. Therefore, one unit of energy is spent in each cell, and the total amount of dissipated energy is equal to the total number of cells. This amount must be equal to the work produced or received by the sources and sinks, which is the sum of the products of their rate of flow times the pressure under which they are working. In this picturesque manner, Maxwell justified the interchange of a field integral with a sum over sources, which Gauss and Thomson had obtained by purely analytical means.[18]

Next, Maxwell explained the analogy of the imaginary flow with various domains of electricity and magnetism. For electrostatics, the tubes of flow correspond to Faraday's lines of electric induction, the pressure to the potential, and the resistance of the medium to the inductive capacity of the dieletric. For magnetism, the tubes of flow correspond to Faraday's magnetic lines of force,[19] the pressure gradient to 'the resultant force of magnetism,' and the resistance of the medium to the inverse of Faraday's 'conducting power' for the lines of force. For electrokinetics, the tubes of flow correspond to the lines of current, the pressure to the electrostatic potential or tension, and the resistance of the medium to the electric resistance.[20]

The total number of cells also has a counterpart in each of the three analogies. Clearly, it is equal to the electrostatic energy in the electrostatic case and to the Joule heat in the electrokinetic case. Maxwell only discussed the case of para- and dia-magnetism, for it justified Faraday's rule of least resistance to the passage of the lines of force: the total number of cells, or resistance overcome by the flow, is then equal to the total magnetic potential from which mechanical forces are derived. Note, however, that the analogy could not help Maxwell locate the magnetic energy in the field: the number of fluid cells (corresponding to the later $\int \mathbf{B}\cdot\mathbf{H}\,d\tau$) did not measure an energy stored in space, but the energy dissipated by the flow.[21]

4.2.3 *Intensity/quantity*

A more relevant aspect of the analogy was the distinction between force and flux implied in the idea of a resisted flow. Maxwell knew that for electric conduction and electrostatic induction Faraday distinguished between electric intensity and quantity. Intensity meant tension causing the current or the electroscopic effect. Quantity

[18] Maxwell 1856b: 161–2, 173–5. In electrostatic symbols, the interchange reads $\int \rho V d\tau = \int \varepsilon E^2 d\tau$.

[19] This is not quite true, because Faraday's magnetic lines of force have no source, whereas Maxwell's tubes of flow have sources corresponding to the magnetic masses.

[20] Maxwell 1856b: 175–83. In the draft of December 1855 (*MSLP* 1: 364), Maxwell did not introduce the electrostatic potential as a counterpart of the pressure. He did in the final version, as a consequence of his reading Kirchhoff 1849b.

[21] Maxwell 1856b: 178–80. Maxwell also combined this analogy with the equivalence between closed currents and double magnetic sheets, to derive the rule that circuits tend to move in such a way as to maximize the magnetic quantity (flux) across them (*ibid.*: 185).

referred to the strength of the electric current, or to the integral current that a charged condenser could produce. Faraday forcefully defended this usage, even though it departed from Ampère's and Thomson's. Shortly before Maxwell's elaboration of his lines of force, he wrote: 'The idea of intensity or the power of overcoming resistance [to induction or to conduction], is as necessary to that of electricity, either static or current, as the idea of pressure is to steam in a boiler, or to air passing through apertures or tubes; and we must have language competent to express these conditions and these ideas.' Maxwell used his flow analogy to systematize the distinction.[22]

In Maxwell's frame of tubes and surfaces, quantity referred to the number of tubes crossing a surface and intensity to the number of surfaces crossed by a given tube. In terms reminiscent of Faraday's, Maxwell wrote: 'The amount of fluid passing through any area in a unit of time measures the *quantity* of action over this area; and the moving force which acts on any element in order to overcome the resistance, represents the total *intensity* of action within the element.' This distinction immediately became central to Maxwell's field theory. An essential virtue of formal analogies accorging to Maxwell was to provide a classification of physico-mathematical quantities that guided theory construction.[23]

Maxwell's first use of the quantity/intensity distinction was unfortunate. To a given intensity, he surmised, there should correspond one and only one quantity. Therefore, the quantity corresponding to the electric potential differences should be the same in electrostatics and in electrokinetics, and a dielectric should be nothing but a very bad conductor in which the electric quantity or current were too small to be detected. Maxwell believed that he could find support for this idea in Faraday's assertion that 'insulation and ordinary conduction cannot be properly separated when we are examining into their nature,' whereas Faraday only meant that conduction always involved the build up and breakdown of electrostatic induction.[24]

Maxwell made a more felicitous use of quantities and intensities in further reflections on electromagnetic induction. He had already been able to express Faraday's law in mathematical terms, and he had learned from Helmholtz how to derive it by an energetic argument. Yet he was no more satisfied with the form of Faraday's law than Faraday himself was:

> This law, though it is sufficiently simple and general to render intelligible all the phaenomena of induction in closed circuits, contains the somewhat artificial conception of the number of lines *passing through* the circuit, exerting a physical influence on it. It would be better if we could avoid, in the enunciation of the law, making the electromotive force in a conductor depend upon lines of force external to the conductor.

Maxwell wanted to express the electromotive force as the variation of some 'intensity' representing the electrotonic state of the conductor. The quantity/intensity

[22] Faraday 1854: 519

[23] Maxwell 1856a: 371; Maxwell 1856b: 182, 189–92. *Ibid.* on 182 Maxwell referred to the distinction of Faraday 1854: 519. Cf. Wise 1979; Everitt 1975: 89–90; Moyer 1978; Hendry 1986: 136–42.

[24] Maxwell 1856b: 181, including a reference to Faraday 1854: 513n.

distinction, a derived symbolism, and some theorems by Thomson and Stokes provided the answer.[25]

In symbols, the fluid quantity across a surface element $\mathbf{dS}$ is $\mathbf{a}\cdot\mathbf{dS}$, where $\mathbf{a}$ denotes the fluid current. The intensity (pressure difference) along the length element $\mathbf{dl}$ is $\boldsymbol{\alpha}\cdot\mathbf{dl}$, where $\boldsymbol{\alpha}$ denotes the moving force. The incompressibility of the fluid gives $\nabla\cdot\mathbf{a} = 0$ (in the absence of sources). The resistance k of the medium implies $\boldsymbol{\alpha} = k\mathbf{a}$. To specify the magnetic and electric cases, Maxwell inserted the suffixes 1 and 2. Then the Ampère law applied to an infinitesimal closed curve yields

$$\nabla\times\boldsymbol{\alpha}_1 = \mathbf{a}_2 \quad (\nabla\times\mathbf{H} = \mathbf{j}). \tag{4.1}$$

Conversely, this relation implies the Ampère law, because, as Maxwell had learned from Stokes,

$$\int\boldsymbol{\alpha}\cdot\mathbf{dl} = \int\int(\nabla\times\boldsymbol{\alpha})\cdot\mathbf{dS}, \tag{4.2}$$

if the first integration is performed over a curve bounding the surface of the second.[26]

In the same notation, Faraday's law reads:

$$\int\boldsymbol{\alpha}_2\cdot\mathbf{dl} = -\frac{\mathrm{d}}{\mathrm{d}t}\int\int\mathbf{a}_1\cdot\mathbf{dS} \quad \left(\int\mathbf{E}\cdot\mathbf{dl} = -\frac{\mathrm{d}}{\mathrm{d}t}\int\int\mathbf{B}\cdot\mathbf{dS}\right). \tag{4.3}$$

Maxwell wanted to reformulate this law in terms of a state of the circuit itself. From Thomson he knew that any divergenceless vector could be regarded as the curl of another vector. He therefore introduced the intensity $\boldsymbol{\alpha}_0$ such that[27]

$$\mathbf{a}_1 = \nabla\times\boldsymbol{\alpha}_0 \quad (\mathbf{B} = \nabla\times\mathbf{A}). \tag{4.4}$$

According to theorem (4.2), the line integral of this intensity is equal to the magnetic quantity passing through the curve. Consequently, the induced electromotive force is simply given by

$$\boldsymbol{\alpha}_2 = -\frac{\partial\boldsymbol{\alpha}_0}{\partial t} \quad \left(\mathbf{E} = -\frac{\partial\mathbf{A}}{\partial t}\right). \tag{4.5}$$

[25] Maxwell 1856a: 373. On Maxwell's reference to Faraday's electrotonic state, cf. Doncel and Lorenzo 1996.

[26] Maxwell 1856b: 203–5; *ibid.*: 206, with proof of Stokes' theorem based on the equivalence between the curves and a net of infinitesimal loops. This theorem was first stated by Thomson in a letter to Stokes of 2 July 1850 (Wilson 1990: 96–7; Stokes 1880–1905, Vol. 5: 320–1), and published by Stokes in the Smith prize examination for 1854, which Maxwell took.

[27] When there are magnetic masses (magnets), $\mathbf{a}_1$ is not divergenceless; Maxwell extracted the divergence-free part in a manner found in Stokes memoir on diffraction (Stokes 1849: 254–7): Maxwell 1856b: 200–1, 203–4.

Maxwell called α_0 the 'electro-tonic intensity,' for he believed that he had found the mathematical expression of Faraday's long-sought electro-tonic state.[28]

More generally, Maxwell professed to have reached the 'mathematical foundation of the modes of thought indicated in the *Experimental Researches*.' His success depended on a geometric deployment of the resisted-flow analogy, followed by a more symbolic approach in which the quantity/intensity distinction played a crucial guiding role. Unlike Thomson, Maxwell accompanied his use of analogy with philosophical comments. He explained that 'physical analogies' offered 'a method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is never drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favorite hypothesis.' Weber's theory, elegant though it was, depended on a questionable physical hypothesis. In contrast, Maxwell's own theory did not contain 'even the shadow of a true physical theory; in fact,' Maxwell went on, 'its chief merit as a temporary instrument of research is that it does not, even in appearance, *account for* anything.' The fluid analogy applied indifferently to separate compartments of electric science; it did not account for mechanical forces among charged bodies, currents, or magnets; and it ignored the relation between electricity and magnetism. The incompressible fluid was purely imaginary, the electro-tonic intensity purely symbolic. Nevertheless, 'by a careful study of the laws of elastic solids and of the motions of viscous fluids' Maxwell hoped 'to discover a method of forming a mechanical conception of the electro-tonic state adapted to general reasoning.'[29]

4.3 On physical lines of force

4.3.1 Molecular vortices

In May 1857, after reading Thomson's 'new lights' on the Faraday effect and molecular vortices, Maxwell wrote to his friend Cecil Monro: 'This was a wet day & I have been grinding at many things and lately during this letter at a Vortical theory of magnetism and electricity which is very crude but has some merits, so I spin & spin.' In a letter to Thomson written a few months later he described a gyromagnetic device that would confirm the existence of vortices in magnetized iron, if only the rotating fluid had enough inertia. Three years later, in the first part of 'On

[28] Maxwell 1856a: 374. In the final paper (1856b), instead of assuming Faraday's law, Maxwell used a flawed energetic reasoning inspired by Helmholtz's 'derivation' of electromagnetic induction: cf. Knudsen 1995.

[29] Maxwell 1856b: 207, 156, 207, 188. For Maxwell's reaction to Weber's theory, see also Maxwell to Thomson, 15 May 1855, *MSLP* 1: 305–6. On the analogies of 'On Faraday's lines of force,' cf. Moyer 1978; Wise 1979, 1981a; Hendry 1986: 143–55; Siegel 1991: 30–3, 38–9. On Maxwell's use of analogy in general, cf. Turner 1955; Hesse 1961, 1966, 1973; Kargon 1969; Chalmers 1973a; Hendry 1986; Siegel 1991; Cat 1995.

physical lines of force' he proposed a theory of magnetism based on molecular vortices.[30]

In his 1856 paper on the Faraday effect, Thomson had written: 'The explanation of all phenomena of electro-magnetic attraction or repulsion, and of electro-magnetic induction, is to be looked simply in the inertia and pressure of the matter of which the motions constitute heat.' He then assumed heat to consist of Rankine's molecular vortices and magnetism in the alignment of these vortices. In 1860 Maxwell supported Clausius's kinetic theory, and therefore could not follow the whole of Thomson's suggestion. He did not doubt, however, that magnetism involved vortical motion, as a consequence of Thomson's analysis of the Faraday effect. And he could precisely see why Thomson believed that the pressure and inertia of the revolving matter determined magnetic forces and electromagnetic induction.[31]

If there exist fluid vortices along the lines of force, he reasoned, then the centrifugal force of the vortices implies a larger pressure in the directions perpendicular to the lines of force than along the lines of force. This is equivalent to an isotropic pressure combined with a tension along the lines of force. Maxwell thus retrieved Faraday's intuition of a mutual repulsion of the lines of force and a tension along them. He only had to verify that this stress system implied the known magnetic attractions and repulsions.[32]

Calling p the isotropic pressure, μ the density of the medium, and $\mathbf{H}$ a vector giving the direction of the vortices and the average linear velocity of the fluid, the stress system is

$$\sigma_{ij} = -p\delta_{ij} + \mu H_i H_j, \tag{4.6}$$

in anachronistic tensor notation. From the net effect of these stresses on the sides of an infinitesimal cube, Maxwell derived the force

$$f_i = \partial_j \sigma_{ij} = -\partial_i p + H_i \partial_j (\mu H_j) + \mu H_j (\partial_j H_i - \partial_i H_j) + \mu \partial_i \left(\frac{1}{2} H^2\right), \tag{4.7}$$

or

$$\mathbf{f} = (\nabla \cdot \mu \mathbf{H})\mathbf{H} + (\nabla \times \mathbf{H}) \times \mu \mathbf{H} + \mu \nabla \left(\frac{1}{2} H^2\right) - \nabla p. \tag{4.8}$$

[30] Maxwell to Faraday, 9 November 1857, *MSLP* 1: 552: 'But there are questions relating to the connexion between magneto-electricity and a possible confirmation of the physical nature of magnetic lines of force. Professor W. Thomson seems to have some new lights on this subject'; Maxwell to Monro, 20 May 1857, *MSLP* 1: 507; Maxwell to Thomson, 30 January 1858, *MSLP* 1: 579–80. Cf. Siegel 1991: 33–7; Harman 1990: 30–1; Everitt 1975: 93–5; Everitt 1983: 132–4.

[31] Thomson 1856: 571. Cf. Chapter 3, pp. 133.

[32] Maxwell 1861: 452–5. Cf. Siegel 1991: 56–65.

Maxwell was now on the grounds of his 'On Faraday's lines of force.' Identifying **H** and $\mu\mathbf{H}$ with the magnetic intensity and quantity defined there, in the successive terms of eqn. (4.8) he recognized the force acting on the imaginary magnetic masses $\nabla \cdot \mu\mathbf{H}$, the force acting on the current $\nabla \times \mathbf{H}$, and the force responsible for the tendency of paramagnetic (diamagnetic) bodies to move toward places of stronger (weaker) magnetic intensity. Hence Thomson's molecular vortices and the resulting stresses accounted for all known magnetic and electromagnetic forces, with striking mathematical exactitude.[33]

4.3.2 *The idle wheels*

Maxwell next wondered why a distribution of vortices for which $\nabla \times \mathbf{H}$ did not vanish indicated an electric current. His answer came with the resolution of the following puzzle:

> I have found great difficulty in conceiving of the existence of vortices in a medium, side by side, revolving in the same direction about parallel axes. The contiguous portions of consecutive vortices must be moving in opposite directions; and it is difficult to understand how the motion of one part of the medium can coexist with, and even produce, an opposite motion of a part in contact with it.

Being aware of electromagnetic induction, Maxwell expected the system of vortices to act as a connected mechanism, able to transfer electric motion from one conductor to another. Like his father and his Scottish professors, he was highly interested in practical mechanics. He had read several treatises on this subject, and taught his students the rudiments of kinematics with toothed wheels and cranks. He was surely familiar with the use of 'idle wheels' for transmitting rotation between two toothed wheels without change in the sense of rotation. Accordingly, he somewhat rigidified his fluid vortices and introduced between them a layer of small, round particles that rolled without sliding (Fig. 4.4(a)).[34]

Whenever two contiguous vortices do not rotate at the same speed, the particles between them must shift laterally (Fig. 4.4(b)). For example, if the vortices are parallel to the axis Oz, and if the rotation velocity H_z grows in the direction Ox, the shift occurs in the direction Oy at the rate $-\partial_x H_z$. In general, the shift is given by $\nabla \times \mathbf{H}$, which is equal to the electric current. Maxwell therefore identified the stream of particles with the electric current.[35]

After this purely kinematical analysis, Maxwell examined the dynamics of the new model. As a result of the tangential action **T** of the particles on the cells, there

[33] Maxwell 1861: 456–64. Note that the 'quantity' $\mu\mathbf{H}$ differs from the **B** of Maxwell's *Treatise* when there are magnets.

[34] Maxwell 1861: 468. Maxwell attended Robert Willis's lectures on mechanism: cf. Maxwell to John Clerk Maxwell, 12 November 1855, *MSLP* 1: 333; and he read a few books on this topic, including Goodeve's *Elements of mechanism* and Rankine's *Applied mechanics* to which he referred in Maxwell 1861: 469n, 458n. On Maxwell's teaching of kinematics, cf. Maxwell to William Thomson, 30 January 1858, *MSLP* 1: 580. On the kinematics of the vortex model, cf. Siegel 1991: 65–9.

[35] Maxwell 1861: 469–71.

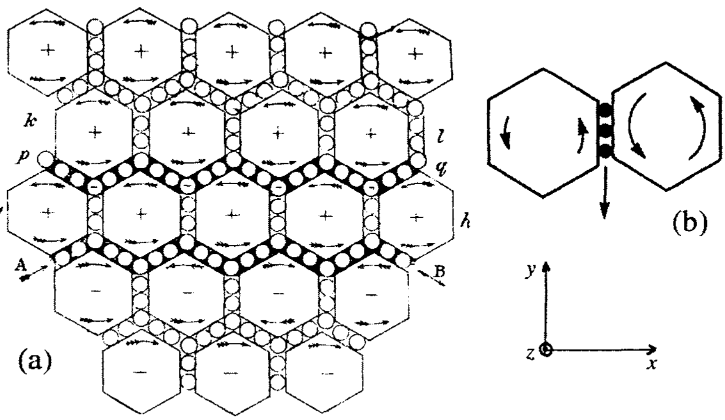

FIG. 4.4. Maxwell's cells and idle wheels (Maxwell 1861: 488 for (a) with mistakes in the arrows from the *MCP* reprint; Siegel 1991: 69 for (b), used by permission of Cambridge University Press).

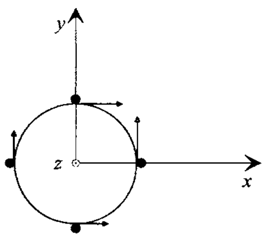

FIG. 4.5. Tangential actions of four idle wheels on a cell.

is a torque acting on each cell. For example, the torque around Oz is proportional to $\partial_x T_y - \partial_y T_x$ (see Fig. 4.5). According to a well-known theorem of dynamics, this torque must be equal to the time derivative of the angular momentum of the cell, which is proportional to $\mu\mathbf{H}$. According to the equality of action and reaction, the force $\mathbf{T}$ must be equal and opposite to the tangential action of the cell on the particles. Maxwell interpreted the latter action as the electromotive force $\mathbf{E}$ of magnetic origin acting on the current. In sum, the curl of $\mathbf{E}$ is found to be proportional to the time derivative of $\mu\mathbf{H}$. The condition that the work of the force $\mathbf{E}$ on the particles should be globally equal to the decrease of the kinetic energy of the cells determines the coefficient. The final equation of motion is

$$\nabla\times\mathbf{E} = -\frac{\partial\mu\mathbf{H}}{\partial t}, \qquad (4.9)$$

in conformity with Maxwell's earlier expression of Faraday's induction law.[36]

Maxwell accompanied his derivation of the fundamental field equations with an intuitive explanation of electromagnetic induction. Consider two conducting circuits separated by an insulator, and let a current be started in one of the circuits. The corresponding flow of particles induces a rotation of the cells immediately outside the conductor. Since in the insulator the particles cannot circulate, they transmit the rotation to the next layer of cells, and so forth until the surface of the second conducting circuit is reached. At this surface the particles are again able to circulate. If there were no electric resistance, they would circulate for ever, and the cells within the conductor would remain at rest. In actual conductors, a frictional force gradually checks the circulation of the particles, and the cells of the conductor are set into rotation. Hence the induced current is only temporary, and the magnetic field in the second conductor is soon the same as it would be in an insulator.[37]

With his wonderful model Maxwell demonstrated the possibility of reducing electromagnetic actions to contiguous mechanical actions. He published his reasoning in the spring of 1861, with a few comments on the awkwardness of the model and on his ignorance of the true nature of electricity. At that time he did not seem to forecast any extension of the model. The obvious limitation to closed currents could not worry him much, since the electrodynamic properties of open currents were experimentally inaccessible.[38]

4.3.3 Electrostatics and light!

A few months elapsed before Maxwell realized that the elasticity of the vortices, which was necessary to their mechanical linking, offered an opportunity to connect electrodynamics with optics and electrostatics. Perhaps a transverse vibration of the substance of the cells could represent light. Perhaps an elastic yielding of the cells under the pressure of the particles could represent dielectric polarization. Specifically, Maxwell imagined that the tangential action of the particles on the cells, which is opposed to the electromotive force by Newton's third law, induced an elastic deformation of the kind represented in Fig. 4.6. Owing to this deformation, the particles in contact with the cells are displaced in a direction opposite to the electromotive force. Calling δ the average displacement, we have

$$\boldsymbol{\delta} = -\varepsilon \mathbf{E}, \tag{4.10}$$

where ε is a constant depending on the elastic constants and on the shape of the cells. The kinematic relation between the flux of particles and the rotation of the cells becomes:

[36] Maxwell 1861: 472–6. Instead of using the theorem of angular momentum, Maxwell used an imperfect energetic reasoning (cf. Darrigol 1993b: note 47). On pp. 479–82 he treated the case of a moving conductor (cf. Darrigol 1993b: 277–9).

[37] Maxwell 1861: 477–8. Cf. Everitt 1975: 96–7.

[38] Cf. Siegel 1991: 75–7; Bromberg 1967: 227; Harman 1970: 191; Everitt 1975: 98–9.

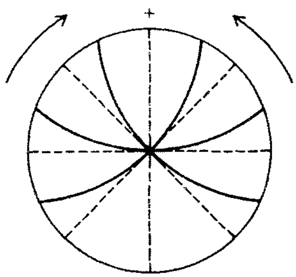

FIG. 4.6. Tangential distortion of a spherical cell (Maxwell to Faraday, 19 October 1861, *MSLP* 1: 684).

$$\mathbf{j} = \nabla \times \mathbf{H} + \frac{\partial \boldsymbol{\delta}}{\partial t}. \tag{4.11}$$

Consequently, the divergence of the current is

$$\nabla \cdot \mathbf{j} = \frac{\partial}{\partial t} \nabla \cdot \boldsymbol{\delta}. \tag{4.12}$$

This equation agrees with the conservation of electricity if the charge density is given by

$$\rho = -\nabla \cdot \boldsymbol{\delta}. \tag{4.13}$$

Although Maxwell does not explictly say so, we may note that ρ also represents an excess of particles, typically occurring at the limit between a conductor and a non-conductor.[39]

Next, Maxwell proceeded to derive the usual electrostatic forces. To this end he considered the elastic energy of the medium,

$$U = \frac{1}{2} \int (-\mathbf{E}) \cdot \boldsymbol{\delta} \mathrm{d}\tau = \frac{1}{2} \int \varepsilon E^2 \mathrm{d}\tau, \tag{4.14}$$

computed it for two point charges q and q', and derived this quantity with respect to the distance d between the charges. The result, $qq'/4\pi\varepsilon d^2$, agreed with Coulomb's

[39] Maxwell 1862: 489–96. For a detailed analysis of the workings of the model, cf. Boltzmann 1898, and Siegel 1986, 1991: 77–119. Most other commentators have misunderstood the mechanics of the model and treated Maxwell's negative sign in the relation between displacement and electromotive force as a mistake. Siegel clarifies this point, and shows how the model accounts for basic electrostatic effects. Some of Maxwell's phrases suggest that he wanted to interpret $\boldsymbol{\delta}$ as a polarization in the Poisson–Mossotti sense. However, in an insulator the displacement of the particles due to the distorsion of the cells must be exactly compensated by a differential rotation of these cells so that the net current $\mathbf{j}$ is zero. As Boltzmann and Siegel argue, the fixity of the particles is essential to the transmission of strain from cell to cell.

law and gave the value of the absolute electrostatic unit of electric charge as $(4\pi\varepsilon_0)^{1/2}$ (the index 0 referring to a vacuum). Consequently, the ratio c of the electromagnetic to the electrostatic charge unit had to be $(\varepsilon_0\mu_0)^{-1/2}$.[40]

At that stage Maxwell had a consistent mechanical model that unified electrostatics and electrodynamics, and he could write the corresponding system of field equations, now called 'the Maxwell equations.' This is not all. He considered transverse waves in the elastic medium. Their velocity is $(k/m)^{1/2}$ if k denotes the transverse elasticity and m the density of the medium. The constant k is inversely proportional to ε, and m is proportional to μ. In order to determine the proportionality coefficients, Maxwell assumed that the cells were spherical and that their elasticity was due to forces between pairs of molecules. He found $k = 1/4\pi^2\varepsilon$, and $m = \mu/4\pi^2$. Then the velocity of transverse waves in a vacuum had to be identical to the ratio c.[41]

Comparing Fizeau's value for the velocity of light and the value of c from Weber and Kohlrausch, Maxwell found agreement within 1% and concluded: 'We can scarcely avoid the inference that *light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.*' In the same stroke, Maxwell explained the strange proximity of the electromagnetic constant c with the velocity of light and realized Faraday's dream of unifying optics and electromagnetism. Yet the quality of the numerical agreement was accidental. As Duhem pointed out many years later, Maxwell had overlooked a factor of 2 in the transverse elasticity of the cells' substance. In any case, the cells could not be spherical. Moreover, Weber and Kohlrausch's and Fizeau's measurements later proved to be both wrong by 3%. What Maxwell truly had was a rough magneto-mechanical theory of light, based on the elasticity of the substance whose rotation represented the magnetic field.[42]

In the last part of his memoir Maxwell returned to the very phenomenon that had inspired his vortex model, the Faraday effect. The rotation of the cells implied a rotation of the polarization of light in the same direction, by an amount proportional to the radius of the cells. Faraday's observations could be explained if the cells were much smaller in a vacuum than in transparent matter, and if their size depended on the kind of matter. However, Maxwell's model implied that the optical rotation should always be in the direction defined by the magnetic field, whereas Emile Verdet had recently observed an opposite rotation for solutions of iron salts. Maxwell briefly suggested that a proper combination of his cellular model with Weber's molecular currents would explain the anomaly.[43]

[40] Maxwell 1862: 497–9. The stress of the cells, which is linear in **E**, cannot be directly responsible for the electrostatic forces, which require a quadratic stress (see Appendix 6). Maxwell never found a mechanical representation of Faraday's electric stresses (cf. Siegel 1991: 83). Maxwell's notation for c was v. This constant is related to that of Weber's theory by $c = C/\sqrt{2}$.

[41] Maxwell 1862: 499.

[42] Maxwell 1862: 500 (Maxwell's emphasis); Duhem 1902: 208–9, 211–12. Cf. Siegel 1991: 136–41. Bromberg 1967 called Maxwell's theory of light of 1862 'electro-mechanical.' I prefer 'magneto-mechanical' because magnetic vortices were the starting point.

[43] Maxwell 1862: 502–13; Verdet 1854–1863. Cf. Knudsen 1976: 255–8.

4.3.4 An orrery

Maxwell had more to say on the status of his mechanical assumptions. His previous analogies with resisted flow, he recalled, were intended to provide a clear geometrical conception of the lines of force. They did not involve any hypothesis on the deeper nature of electric and magnetic actions. In contrast, his new approach assumed the existence of stresses from which observed mechanical actions derived. The lines of force now referred to these stresses and were therefore as physical as Faraday wanted them to be. Maxwell further adopted Thomson's assumption that the stresses in the magnetic field were due to molecular vortices. These physical hypotheses permitted a unified, dynamical understanding of magnetism and electromagnetism; and they were anchored on the rock of Thomson's argument on the Faraday effect. They remained in the core of Maxwell's theory until his death.[44]

However, Maxwell did not believe in the literal truth of his more specific assumptions regarding the constitution and interconnection of the molecular vortices:

> The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connexion existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is however, a mode of connexion wich is mechanically conceivable, and easily investigated, and it serves to bring out the actual mechanical connexions between the known electro-magnetic phenomena; so that I venture to say that any one who understands the provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena.

Maxwell did not doubt the truth of the relations he had obtained between the electric and magnetic fields, and he believed that these relations derived from the laws of mechanics. But a peculiar combination of vortices and idle wheels could not meet his idea of the simplicity of nature. As he explained to Tait: 'The nature of this mechanism is to the true mechanism what an orrery is to the solar system.'[45]

4.4 The dynamical field

After the publication of 'On physical lines of force,' Maxwell's agenda included the experimental verification of three predictions of his theory. He planned to renew his attempts at detecting gyromagnetic effects. He envisioned precise measurements of the inductive capacity ε of various transparent substances in order to verify the theoretical relation with the optical index ($\varepsilon = n^2$). Most importantly, he intended to verify the identity of the velocity of light with the ratio of absolute electromagnetic and electrostatic charge units by improving on Weber and Kohlrausch's measurement. His enrollment in the British project for electric standards eased this task. In 1864 he imagined an arrangement based on the direct comparison between an elec-

[44] Maxwell 1862: 451–3. Cf. Knudsen 1976: 248–55; Siegel 1991: 39–55.
[45] Maxwell 1861: 486; Maxwell to Tait, 23 December 1867, *MSLP* 2: 337.

trodynamic and an electrostatic force. Four year laters he published the results of a more sophisticated experiment based on the same principle.[46]

Considering that the electromagnetic derivation of the velocity of light was his most important result, Maxwell tried to 'clear the electromagnetic theory of light of all unwarranted assumptions.' The velocity of light could not possibly depend on the shape of vortices or on their kind of elasticity. In 1864 Maxwell managed to reformulate his theory without any specific mechanism and to describe wave propagation in purely electromagnetic terms. In order to understand how he accomplished this, we must return to the electrotonic state.[47]

4.4.1 The reduced momentum

When Maxwell designed the vortex model, he was still looking for a mechanical interpretation of the electrotonic state. He found one of an unexpected sort. Having rewritten the induction law (4.9) as

$$\mathbf{E} = -\frac{\partial \mathbf{A}}{\partial t} \quad \text{with} \quad \mu\mathbf{H} = \nabla\times\mathbf{A}, \tag{4.15}$$

he noted that **A** played the role of a 'reduced momentum' for the mechanism driven by the flow of particles. He thereby meant a generalization of Newton's second law, in which the force −**E** served to increase the reduced momentum. More concretely, he compared **A** with 'the *impulse* which would act on the axle of a wheel in a machine if the actual velocity were suddenly given to the driving wheel, the machine being previously at rest.' Impulse and momentum were prominent notions in the treatises on mechanism he had been reading, especially Rankine's. Also, impulsive forces played a central role in Stokes's and Thomson's considerations of irrotational flow.[48]

Two simple examples will illustrate what Maxwell had in mind. In the case of a single linear circuit, the current i sets the surrounding cells into rotary motion, as a rack pulled between toothed wheels (Fig. 4.7). If the mass of the axle is negligible, a finite force is still necessary to set it into motion because of the inertia of the connected wheels. By Maxwell's definition, the reduced momentum is the impulse

[46] Maxwell to Thomson, 10 December 1861, *MSLP* 1: 694–8; Maxwell to Thomson, 15 October 1864, *MSLP* 2: 176; Maxwell 1868a. On the gyromagnetic experiments, cf. Maxwell to Faraday, 19 October 1861, *MSLP* 1: 688–9; Maxwell 1861: 485n–6n; Maxwell 1873a: ##574–5; and Galison 1982 for the later history of such effects. For a classification of the devices to measure the units ratio, cf. Jenkin and Maxwell 1863. In Maxwell's 1864 device, the repulsion of two current-fed coils is balanced by the attraction between two electrified disks; the current feeding the coils passes through a resistance of known absolute value; and the potential difference at the ends of this resistance is applied to the disks. On the ensuing project, cf. Schaffer 1995; d'Agostino 1996: 31–6; Simpson 1997: 347–63; Harman 1998: 65–8.

[47] Maxwell to Hockin, 7 September 1864, *MSLP* 2: 164; Maxwell 1865.

[48] Maxwell 1861: 478. I have changed the sign of **A** for consistency with Maxwell's later papers. Reference to Rankine's *Applied mechanics* is found *ibid.*: 458n (for the definition of stresses). On impulsive forces, cf. Moyer 1977: 257–8.

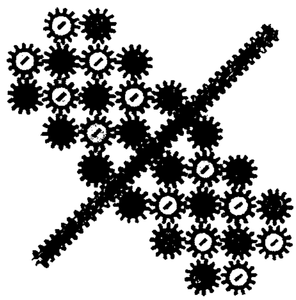

FIG. 4.7. Illustration of self-induction in a linear circuit (from Lodge 1889: 186). The + and – signs indicate the sense of the rotation.

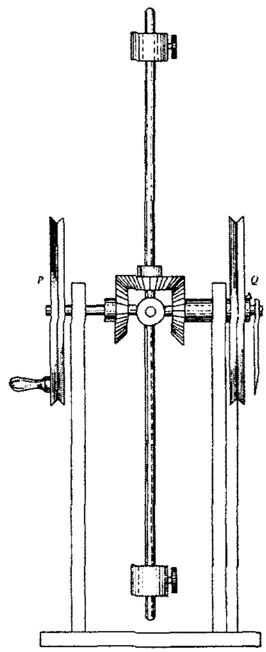

FIG. 4.8. Maxwell's model for mutual induction (Maxwell 1891, Vol. 2: 228).

necessary to obtain a given velocity i. This impulse is proportional to the velocity and to the inertia of the wheels. In electric language, it is equal to Li, where L is the self-inductance of the circuit, and it measures the electro-tonic state.

In the case of two linear cicuits, the rotation of the cells is in a one-to-one correspondence with the two currents i_1 and i_2. This situation is analogous to the mechanism of Fig. 4.8, in which the rotations of the wheels P and Q play the role of the two currents and the rotation of the fly-weights plays the role of the vortex rotation in the magnetic field. The wheels P and Q have a negligible inertia. However, a finite force is in general necessary to set them into motion, because of the inertia of the fly-weights. The reduced momenta at P and Q are the impulses necessary to impart on them the velocities i_1 and i_2. These impulses have the linear forms

$$p_1 = L_1 i_1 + M i_2, \quad p_2 = L_2 i_2 + M i_1. \tag{4.16}$$

They measure the electro-tonic states of the two circuits. Generalizing to a three-dimensional current distribution **J**, the electromotive force necessary to start this current impulsively must be a certain linear function of **J**, to be identified with the electrotonic state **A**.[49]

4.4.2 *Hidden mechanism*

Maxwell reached this mechanical interpretation of the electrotonic state in 1861, on the basis of the vortex model. Three years later, he realized that the interpretation was essentially independent of any specific mechanism and could serve as a more abstract foundation for the dynamics of the magnetic field. He simply admitted that through an unspecified connected mechanism the existence of an electric current implied a motion in the surrounding field. Then, the force necessary to communicate this motion had to be the time derivative of a generalized momentum **A**, which he now called the 'electromagnetic momentum.' In the case of two circuits, this yields the usual equations for inductive coupling (Neumann's)

$$\begin{aligned} e_1' - R_1 i_1 &= \frac{\mathrm{d}}{\mathrm{d}t}(L_1 i_1 + M i_2) \\ e_2' - R_2 i_2 &= \frac{\mathrm{d}}{\mathrm{d}t}(L_2 i_2 + M i_1) \end{aligned} \tag{4.17}$$

where e_1' and e_2' are the impressed electromotive forces and R_1 and R_2 the resistances.[50]

Maxwell next considered the energy brought by the electromotive sources according to Thomson:

$$\begin{aligned} e_1' i_1 + e_1' i_2 = R_1 i_1^2 + R_2 i_2^2 &+ \frac{\mathrm{d}}{\mathrm{d}t}\left(\frac{1}{2}L_1 i_1^2 + \frac{1}{2}L_2 i_2^2 + M i_1 i_2\right) \\ &+ \frac{1}{2}\frac{\mathrm{d}L_1}{\mathrm{d}t} i_1^2 + \frac{1}{2}\frac{\mathrm{d}L_2}{\mathrm{d}t} i_2^2 + \frac{\mathrm{d}M}{\mathrm{d}t} i_1 i_2. \end{aligned} \tag{4.18}$$

[49] Maxwell constructed this model in 1874. Cf. Maxwell 1891: 228, and Everitt 1975: 103–4.
[50] Maxwell 1865: 536–40. Cf. Simpson 1970; Topper 1971; Chalmers 1973; Moyer 1977; Siegel 1981; Buchwald 1985a: 20–3; Hendry 1986: 191–206; Siegel 1991.

The two first terms represent the Joule heat. The third represents the variation of the energy

$$T = \frac{1}{2}(p_1 i_1 + p_2 i_2) \tag{4.19}$$

stored in the hidden mechanism. The three last terms exist only if the geometrical configuration of the circuits varies: they represent the work of electrodynamic forces during this motion. Maxwell thus inverted the prodedure followed by Helmholtz and Thomson; that is, he derived the expression of electrodynamic forces from the laws of induction.[51]

4.4.3 *Lagrangian dynamics*

Maxwell's reasoning appeared in his 'dynamical theory of the electromagnetic field,' published in 1865. With this title he meant to announce a reduction of electrodynamics to hidden motion in the field. In the *Treatise*, published in 1873, he improved his presentation by a recourse to the Lagrange equations. A system of two currents according to Maxwell is a connected system the motion of which is completely defined by two generalized velocities i_1 and i_2. Following Lagrange, the motion of this system is completely determined by the form of its kinetic energy, which is

$$T = \frac{1}{2}L_1 i_1^2 + \frac{1}{2}L_2 i_2^2 + M i_1 i_2, \tag{4.20}$$

for the electrodynamic part. The Lagrange equations with respect to the generalized velocities i_1 and i_2 give

$$e_1' - R_1 i_1 = \frac{\mathrm{d}}{\mathrm{d}t}\frac{\partial T}{\partial i_1}, \quad e_2' - R_2 i_2 = \frac{\mathrm{d}}{\mathrm{d}t}\frac{\partial T}{\partial i_2}, \tag{4.21}$$

in conformity with eqns (4.17). If the geometric configuration of the circuits depends on the coordinate ξ, the corresponding Lagrange equation yields the electrodynamic force $\partial T/\partial \xi$ (see Appendix 9 for later three-dimensional generalizations).[52]

Originally, Lagrange designed his analytical method as a way of eliminating the quantities that pertain to the internal connections of a connected mechanical system. Laplacian physicists had little use of the method, since they always started with molecular forces. British physicists were first to appreciate the great power of the method: it gave the equations of motion of a mechanical system by an automatic

[51] Maxwell 1865: 541–2.

[52] Maxwell 1873a: ##578–83. Save for their Lagrangian justification, Maxwell's circuit equations are exactly identical to those of Neumann's theory of induction.

prescription, directly in terms of the controllable elements. For example, in 1837 George Green derived the equations of motion of an elastic solid by expressing its kinetic and potential energy in terms of the local displacements and writing the corresponding Lagrange equations. William Thomson adopted the method, for it shared the virtue of the energy principle of dealing with controllable inputs and outputs. He tried to make it less abstract by combining it with the more physical notions of work and impulse. In his and Tait's *Treatise of Natural Philosophy* (known as TT′), first published in 1867 and proof-read by Maxwell, he defined generalized forces through the work they brought to the system ($\Sigma f_i \mathrm{d}q_i$, for a variation $\mathrm{d}q_i$ of the generalized coordinates), and the generalized 'momenta' p_i as the impulses necessary to suddenly start the motion of the system from rest. The Lagrange equations, $f_i = \mathrm{d}p_i/\mathrm{d}t - \partial T/\partial q_i$, thus took a physically transparent form.[53]

Maxwell was very sympathetic to Thomson and Tait's presentation. He developed it in a chapter of his *Treatise*, with the comment: 'We avail ourselves of the labours of the mathematicians [Lagrange and Hamilton], and retranslate their results from the language of the calculus into the language of dynamics, so that our words may call up the mental image, not of some algebraical process, but of some property of moving bodies.' In this process Maxwell was less careful than Thomson and Tait, and erred in a pseudo-derivation of the Lagrange equations based on energy conservation. However, thanks to the new dynamical language he perceived an essential advantage of Lagrange's method: that the motion of the driving points of a connected mechanism could be studied without any knowledge of the internal connections, as some kind of black box. Maxwell used the metaphor of a belfry, the machinery of which is controlled by a number of ropes. The machinery being originally at rest, finite velocities are impressed impulsively on the ropes. If the necessary impulses are measured for every possible value of the positions and final velocities of the ropes, the kinetic energy of the system can be computed as a function of generalized coordinates and velocities (the homogeneity of T implies that $2T = \Sigma\, p_i \mathrm{d}q_i/\mathrm{d}t$). Then the motion of the ropes for any applied force is given by the corresponding Lagrange equations.[54]

4.4.4 The electromagnetic momentum

With the momentum interpretation, the vector potential became the central dynamical concept of Maxwell's theory. The induced electromotive force in a circuit was

[53] Green 1838: 246: 'One of the great advantages of this method [of the *Mécanique analytique*], of great importance, is, that we are necessarily led by the mere process of the calculation, and with little care on our part, to all the equations and conditions which are *requisite* and *sufficient* for the complete solution of any problem to which it may be applied'; Thomson to Stokes, 20 October 1847, in Wilson 1990: 32 (for least action applied to impulsively started fluid motion); Thomson and Tait 1867: 217–35. Cf. Siegel 1981: 259–63; Everitt 1975: 105–6; Everitt 1983: 128–9; Buchwald 1985: 60–1; Harman 1987: 287–88; Smith and Wise 1989: 270–3, 390–5 (on TT′).

[54] Maxwell 1873a: #554; Maxwell 1879: 783–84 for the belfry metaphor (for simplicity, I have excluded potential energy). Cf. Moyer 1977; Siegel 1981; Simpson 1970; Topper 1971; Buchwald 1985: 20–3.

just the time derivative of its reduced momentum. Maxwell further assumed that the circuit momentum was the line integral of the 'electromagnetic momentum' **A**. This gives

$$\int \mathbf{E}\cdot d\mathbf{l} = -\frac{d}{dt}\int \mathbf{A}\cdot d\mathbf{l}, \tag{4.22}$$

or

$$\mathbf{E} = -\frac{\partial \mathbf{A}}{\partial t} + \mathbf{v}\times(\nabla\times\mathbf{A}) - \nabla\phi \tag{4.23}$$

for the electromotive force **E** at a point of the conductor moving with the velocity **v**. Maxwell called ϕ the 'electric potential' and mentioned that it was determined by other conditions of the problem.[55]

In order to relate **A** to the magnetic field, Maxwell followed Faraday's suggestion of defining the magnetic lines of force by the electromotive force induced during their cutting by a linear conductor. Hence the magnetic quantity **B** must be identified with the curl of the electromagnetic momentum **A**. For the determination of **B** in terms of the current **J**, Maxwell used the reasoning of his 'On Faraday lines of force,' which leads to

$$\nabla \times \mathbf{H} = \mathbf{J} \tag{4.24}$$

for the intensity $\mathbf{H} = \mathbf{B}/\mu$.[56]

Maxwell then generalized the expression (4.20) for the kinetic energy of two currents. which gives

$$T = \frac{1}{2}\int \mathbf{J}\cdot\mathbf{A}\,d\tau. \tag{4.25}$$

This expression was most important in the new dynamical theory, for $\frac{1}{2}\mathbf{J}\cdot\mathbf{A}\,d\tau$ meant the energy *controlled* by the current in the volume element $d\tau$. Using $\mathbf{J} = \nabla \times \mathbf{H}$ and a partial integration, it could be transformed back into the expression given by the vortex model,

$$T = \int \frac{1}{2}\mathbf{B}\cdot\mathbf{H}\,d\tau, \tag{4.26}$$

in which $\frac{1}{2}\mathbf{B}\cdot\mathbf{H}\,d\tau$ referred to the energy *stored* in the element $d\tau$.[57]

55 Maxwell 1865: 555–60.
56 Maxwell 1865: 550–54, 556–57.
57 Maxwell 1865: 562–63.

4.4.5 Closing the circuit

The Ampère law (4.24) only applies to divergenceless or closed current. More fundamentally, Maxwell's dynamical reasoning implies the restriction to closed currents, because only in this case is the magnetic field motion completely determined by the currents. If there is any elastic yielding of the field mechanism, as Maxwell assumed in his vortex model, then the motion also depends on the deformation of this mechanism. Maxwell's solution to this difficulty was to change the definition of the electric current. In the vortex model he had defined the current as the flux of particles between the vortices. In his 'dynamical theory,' he tried to follow Faraday's notion that the electric current was a variation or transfer of polarization.[58]

Maxwell first defined the polarization or 'electric displacement' **D** as a displacement of electricity in the molecules of the dielectric, referring here to Mossotti's theory of electrostatic induction. Being elastically resisted, the displacement required an electromotive force $\mathbf{E} = \mathbf{D}/\varepsilon$, and implied a potential energy of the medium

$$U=\frac{1}{2}\int \mathbf{E}\cdot\mathbf{D}\mathrm{d}\tau. \tag{4.27}$$

A variation of displacement implied an electric current $\partial\mathbf{D}/\partial t$. Electric conduction occurred when electricity was allowed to pass from one molecule to the next at the rate **j**. Hence, in a medium presenting both inductive capacity and conductivity, the total current was

$$\mathbf{J}=\frac{\partial\mathbf{D}}{\partial t}+\mathbf{j}. \tag{4.28}$$

The resulting expression of the Ampère law was the same as that given in 'On physical lines of force':

$$\nabla\times\mathbf{H}=\mathbf{j}+\frac{\partial\varepsilon\mathbf{E}}{\partial t}. \tag{4.29}$$

We must note, however, an important difference of interpretation. In the old theory, what Maxwell called the 'displacement current' was $-\partial\varepsilon\mathbf{E}/\partial t$ and contributed to the conduction current. In the new theory, the displacement current became a contribution to a divergenceless total current.[59]

In conformity with Mossotti's picture of polarization Maxwell took

$$\rho=-\nabla\cdot\mathbf{D} \tag{4.30}$$

[58] Maxwell 1865: 531.

[59] Maxwell 1865: 554, 560. Cf. Siegel 1991: 145–52.

to represent the density of 'free electricity.' This brought him into grave difficulties, part of which he solved by reversing the sign in Ohm's law (he took $\mathbf{j} = -\sigma\mathbf{E}$). In fact, his equations were not compatible with the conservation of electricity, as is easily seen by taking the divergence of eqn. (4.28). He was here a victim of his well-known plus-minus dyslexia. He tended to place signs in his equations according to the underlying physical idea, not according to algebraic compatibility. Unfortunately, the physical idea under eqn. (4.30) was incompatible with Faraday's concept of electric charge, as Maxwell later realized.[60]

4.4.6 *Electromagnetic light waves*

Fortunately, the most important application of the new theory, the derivation of the equation of electromagnetic disturbances in a non-conducting medium, did not depend on the sign of electric charge. Maxwell combined eqns. (4.23), (4.24), and (4.28), and reached, for the magnetic induction,

$$\varepsilon\mu\frac{\partial^2\mathbf{B}}{\partial t^2} = \Delta\mathbf{B}. \tag{4.31}$$

This is a wave equation with the propagation velocity $(\varepsilon\mu)^{-1/2}$. Maxwell also treated the case of a crystalline medium, and determined how conductivity affected transparency. He now had an electromagnetic theory of light *sensu stricto*, since he could describe the waves directly in terms of the electric and magnetic fields. Moreover, his derivation of the velocity of the waves became independent of any assumption on the underlying mechanism.[61]

The electromagnetic momentum **A** being central to his new approach, Maxwell tried to determine how it propagated. Today's physicist knows that **A** is ambiguous: any gradient can be added to it without changing the measurable fields **E** and **H**, provided that a compensating change of the scalar potential is performed. In 1862 Maxwell thought differently. He believed that **A** was unambiguously defined as the impulse necessary to start a given current. Also, he believed that he could maintain the general validity of Poisson's equation ($\Delta\phi + \rho = 0$). On this erroneous assumption he found that the longitudinal part of **A** could not propagate as a wave, in conformity with the transverse character of light waves.[62]

To summarize, by 1865 Maxwell had all the elements of a powerful dynamical theory of the electromagnetic field based on the following principles:

[60] Maxwell 1865: 561. Maxwell reversed the sign in Ohm's law, presumably to mend his theory of electric absorption (*ibid.*: 573–6); but he kept the plus sign in his study of wave absorption by conductors! On the problem of the sign of charge, cf. Siegel 1991: 148–52.

[61] Maxwell 1865: 577–88. Cf. Bork 1966a; Bromberg 1967; Chalmers 1973; Siegel 1991: 152–7.

[62] Maxwell 1865: 580–2. Maxwell was unaware that as the conjugate momentum of the 'velocity' **J**, the potential **A** is ambiguous, because of the constraint $\nabla\cdot\mathbf{J} = 0$ (see Appendix 9). On Maxwell's confusions about the potentials, cf. Bork 1966a: 847–8; Bork 1967; Anderson 1991; Hunt 1991a: 116–17; Cat 1995.

1. Closed currents control a hidden motion in the field.
2. All current are closed.
3. Charge and current derive from polarization, which is an elastic deformation of the medium under electromotive force.

However, the theory was still hampered by confusions regarding the concepts of electromagnetic momentum and dielectric polarization.

4.4.7 Electromagnetic momentum, revised

When in 1868 Maxwell published the results of his new measurement of the ratio c of the electromagnetic to the electrostatic charge unit, he restated the electromagnetic theory of light 'in the simplest form, deducing it from admitted facts, and shewing the connexion between the experiments already described [for the measurement of c] and those which determine the velocity of light.' The 'admitted facts' were Oersted's electromagnetism, Faraday's law of electromagnetic induction, and Faraday's doctrine of polarization. From them Maxwell extracted four simple 'theorems' expressing in words the integrals of the magnetic and electric intensities on closed curves, the relation between electric intensity and displacement, and the displacement current. All reference to the electromagnetic momentum was gone, and the deduction of electromagnetic plane waves became quite elementary.[63]

Maxwell could not, however, renounce the dynamical foundation of his theory. It was an essential part of his later *Treatise*, in the Lagrangian form already described. There he acknowledged the gap in the definition of the electromagnetic momentum $\mathbf{A}$, and introduced the condition $\nabla \cdot \mathbf{A} = 0$ as a convenient way to remove the ambiguity. With this choice the momentum of a given current in a medium of uniform permeability μ became

$$\mathbf{A}(\mathbf{r}) = \frac{\mu}{4\pi} \int \frac{\mathbf{J}(\mathbf{r}')\mathrm{d}\tau'}{|\mathbf{r}-\mathbf{r}'|}. \tag{4.32}$$

The analogy with the scalar potential in a uniform dielectric,

$$\phi(\mathbf{r}) = \frac{1}{4\pi\varepsilon} \int \frac{\rho(\mathbf{r}')\mathrm{d}\tau'}{|\mathbf{r}-\mathbf{r}'|}, \tag{4.33}$$

justified the alternative name 'vector potential' for $\mathbf{A}$.[64]

Unfortunately, in his derivation of the equation for the propagation of electromagnetic disturbances, Maxwell repeated the error of considering the scalar potential formula (4.33) as generally valid, independently of the choice of $\nabla \cdot \mathbf{A}$. In fact

[63] Maxwell 1868: 138. Cf. Everitt 1975: 108–9; Hendry 1986: 220–6; Siegel 1991: 153–4. This simple formulation of Maxwell's theory was largely unnoticed until its reprint by Niven in *MSP* in 1890 (I thank Bruce Hunt for this remark).

[64] Maxwell 1873a: #617.

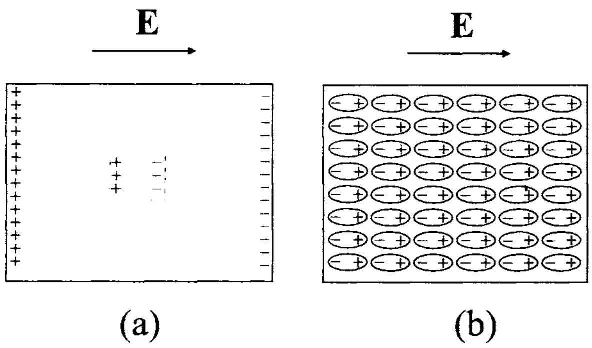

FIG. 4.9. Polarization: (a) according to Maxwell, (b) according to Mossotti.

there is no evidence that he ever combined the equation $\nabla \cdot \varepsilon \mathbf{E} = \rho$ with the electromotive force formula (4.23), except in the electrostatic case. He does not seem to have fully realized that his general assumptions on the electromagnetic field implied a much deeper interconnection of electrostatic and electrodynamic actions than was assumed in continental theories.[65]

4.4.8 Displacement, revised

However, Maxwell managed to clear up his concept of polarization. In the *Treatise* he adopted the positive sign in the relation $\rho = \nabla \cdot \mathbf{D}$, which means that a portion of polarized dielectric is charged positively where the polarization starts and negatively where the polarization ends (Fig. 4.9(a)). This choice agrees with Faraday's definition of positive charge as the starting point of electric lines of force, but contradicts Mossotti's picture of displaced electric charge (Fig. 4.9(b)). Maxwell, like Faraday, avoided the contradiction by considering the concept of polarization as more primitive than the concept of charge. If anything was displaced in the elements of a polarized dielectric, it could not be electric charge. This is an essential point, which must always be kept in mind when reading Maxwell's difficult sections on charge and current.[66]

Maxwell's views then appear to be very similar to Faraday's. Polarization (Faraday's 'induction') is defined as a state of constraint of the dielectric, such that each portion of it acquires equal and opposite properties on two opposite sides. By definition, electric charge is a spatial discontinuity of polarization. Typically, charge

[65] Maxwell 1873a: #783. Several of Maxwell's early readers, including Larmor and J. J. Thomson, inherited Maxwell's confusion on the definition of ϕ. In the same vein, Maxwell gave $-\rho\nabla\phi$ for the force acting on electrified matter (#619), which could be true only in the electrostatic case and contradicted his expression of electric stresses (#108) (see Appendix 6). This mistake is corrected in FitzGerald 1883b, and in the third edition of the *Treatise*.

[66] Maxwell 1873a: ##60–2, #111. See also Maxwell to Thomson, 5 June 1869, *MLSP* 2: 485–6. For a lucid account, cf. Buchwald 1985: 23–34; also Knudsen 1978.

occurs at the limit between a polarized dielectric and a conductor, because by definition a conductor is a body that cannot sustain polarization. As Maxwell explains, 'the electrification at the bounding surface of a conductor and the surrounding dielectric, which on the old theory was called the electrification of the conductor, must be called in the theory of induction the superficial electrification of the surrounding dielectric.'[67]

Conductors cannot sustain polarization. However, they may transfer polarization. This transfer, according to Faraday and Maxwell, results from a competition between polarization build-up and decay in the conductor. A conductor thus appears to be a yielding dielectric: 'If the medium is not a perfect insulator,' Maxwell writes, 'the state of constraint, which we have called electric polarization, is continually giving way. The medium yields to the electromotive force, the electric stress is relaxed, and the potential energy of the state of constraint is converted into heat.' By definition, the electric current is the rate of transfer of polarization. In a dielectric, it is simply measured by the time derivative of the polarization. In a conductor, it also depends on the decay mechanism, the microscopic details of which are unknown. Its expression must therefore be determined empirically (by Ohm's law). Thus defined, the electric current is always closed, for the current in an open conducting circuit is continued through the dielectric.[68]

All of this is quite consistent, and does not involve any of the absurdities later denounced by Maxwell's continental readers. Yet Maxwell's terminology was truly misleading. He called the polarization of a portion of dielectric 'a displacement of electricity.' By this phrase he only meant that a portion of the dielectric, if separated in thought from the rest of the dielectric, would present opposite charges at two opposite extremities. He certainly did not mean that an electrically charged substance was displaced. However, many of his readers understood just that. To make it worse, Maxwell asserted that 'the motions of electricity are like those of an *incompressible* fluid.' Here he only meant that the closed character of the total current made it analogous to the flow of an incompressible fluid. But he was often lent the opinion that electricity *was* an incompressible fluid.[69]

As long as it is used with proper care, the fluid analogy is useful to illustrate the relations between displacement, charge, and conduction. Suppose an incompressible fluid to pervade a space in which a rigid scaffolding has been erected. In 'insulating' parts of this space, the portions of the fluids are elastically linked to the scaffolding. In a 'conducting' part, such links also exist, but when under tension they tend to break down and dissipate their energy into heat; every breaking link is immediately replaced by a fresh, relaxed link. In this illustration, the extension of the links corresponds to Maxwell's displacement (or polarization); the pressure gradient of the fluid to the electromotive force; the flow of the fluid to the electric current; and the discontinuity of the average extension of the links when crossing the limit between conductor and insulator corresponds to electric charge. The analogy

[67] Maxwell 1873a: #60, #111. [68] Maxwell 1873a: #111. Cf. Buchwald 1985a: 28–9.
[69] Maxwell 1873a: #61. See also the fluid-piston illustration of a dielectric, *ibid.* in #334.

properly illustrates the equations $\mathbf{D} = \varepsilon\mathbf{E}$, $\nabla\cdot\mathbf{J} = 0$, $\mathbf{J} = \mathbf{j} + \partial\mathbf{D}/\partial t$, and $\rho = \nabla\cdot\mathbf{D}$. It is, however, misleading when one comes to propagation problems and energy flow, as we will later see.

4.5 *Exegi monumentum*

Around 1867 Maxwell set himself to work on a major treatise on electricity and magnetism. His intention was partly to propel his new theory and Faraday's underlying views. There also was an urgent need for that kind of book. Although the field of electricity and magnetism had grown enormously since Oersted and Ampère, there was as yet no unified presentation of all its experimental, technical, and mathematical aspects. The gap had widened between the practical electricity of telegraphists and the mathematical electricity of learned professors. There was a growing multiplicity of terms, conventions, and theories; and little attempt at uniformization and comparison, despite the high intellectual and economical stakes.[70]

Maxwell was especially sensitive to the neglect of the quantitative aspects of the subject. He believed that the mathematical theories of electricity and magnetism were ripe to be taught in the university, and pressed the Cambridge authorities to introduce them in the Mathematical Tripos. Only the proper reference book was missing, as Maxwell himself judged:[71]

> There are several treatises in which electrical and magnetic phenomena are described in popular way. These, however, are not what is wanted by those who have been brought face to face with quantities to be measured, and whose minds do not rest satisfied with lecture-room experiments.—There is also a considerable mass of mathematical memoirs which are of great importance in electrical science, but they lie concealed in the bulky Transactions of learned societies; they do not form a connected system; they are of very unequal merit, and they are for the most part beyond the comprehension of any but professed mathematicians.—I have therefore thought that a treatise would be useful which should also indicate how each part of the subject is brought within the reach to methods of verification by actual measurement.

Books on electricity were indeed few, and failed to provide a full, systematic introduction to the subject. Auguste de la Rive's *Traité d'électricité* of 1853 was very empirical, had almost no mathematics, and ignored or misrepresented Faraday's theoretical views. Gustav Wiedemann's *Lehre vom Galvanismus* of 1863 gave precise and clear accounts of nearly all works published on the subject, with a fair share of the British views; but its encyclopedic scope and structure made it unsuited to the guidance of students. Maxwell's *Treatise*, published in 1873, filled a major gap in the existing literature.[72]

[70] On the gap between practical and academic electricity, cf. the introduction of Jenkin 1873.

[71] Maxwell 1873a: ix. On the 1867 reform of the Cambridge Mathematical Tripos and on the editorial circumstances of Maxwell's project, cf. Achard 1998.

[72] On the genesis of the *Treatise*, cf. Harman 1995a: 26–33.

4.5.1 Mathematical and empirical foundations

Maxwell's challenge was to expound a new doctrine and at the same time to establish new standards in the treatment of current problems. In order to meet these two conflicting requirements, he carefully separated the basic mathematical and empirical foundations of the subject from more speculative theory. In a preliminary 'on the measurement of quantities' he expounded Fourier's doctrine of dimensions, Hamilton's distinction between scalar and vector, the notions of force and flux corresponding to his older 'intensity' and 'quantity,' various theorems relating the integrals of force and flux, and related topological questions. He regarded the classification of physico-mathematical quantities as a way to short-circuit formal analogies and organize the field of knowledge: 'It is evident that [. . .] if we had a true mathematical classification of quantities,' he had earlier explained, 'we should be able at once to detect the analogy between any system of quantities presented to us and other systems of quantities in known sciences, so that we should lose no time in availing ourselves of the mathematical labours of those who had already solved problems essentially the same.'[73]

Maxwell then defined the basic physical quantities in a neutral manner that could be accepted both by fluid and field theorists. For example, he introduced the quantity of electric charge of a body by means of Faraday's hollow conductors: two charges could be added by bringing their carriers into a hollow conducting vessel and noting the charge of the vessel. He defined the electric potential in Thomson's manner, as the work done on a unit point charge to bring it at a given place. Lastly, he defined the magnetic force **H** and flux **B** in a polarizable substance as the forces acting on a magnetic unit pole (end of uniformly magnetized needle) placed in a small cylindrical cavity, elongated for **H**, and flat for **B**.[74]

With these neutral definitions, Maxwell could conduct much of the mathematical analysis without deciding the nature of electricity and magnetism. This can be seen in his Thomsonian presentation of the potential theories of electrostatics and magnetism. The *Treatise* was in part meant as a source book for computational and experimental techniques for competent electricians, whatever they might think of the essence of electricity. The originators of these techniques were as diverse as their potential users. They could be Laplace on spherical harmonics, Gauss on geomagnetism, Weber on galvanometric measurements, Kirchhoff on circuit theory, Thomson on electrometers, or Maxwell himself on the calculation of inductance.

[73] Maxwell 1873a: ##1–26; Maxwell 1870: 258 (quote). Cf. Harman 1987: 278–87. On dimensions, cf. Jenkin and Maxwell 1863; Everitt 1975: 100–1; d'Agostino 1996: 37–41. On topology, cf. Epple 1998; Harman 1998: 153–6.

[74] Maxwell 1873a: #34 and #63 (for charge), #70 (for potential), ##398–400 (for **B** and **H**). Maxwell and Thomson disagreed on the definition of the electrostatic potential when contact between different metals was involved: cf. Hong 1994a. Maxwell's **B** and **H** corresponded to Thomson 'electromagnetic' and 'polar' definitions of the magnetic field (see Chapter 3, p. 130); however, they referred to two different physical concepts (flux and force), whereas Thomson only meant two different ways of characterizing the same physical entity: cf. Wise 1981a.

These techniques could serve the Mathematical Tripos, German seminars, and telegraphists in the whole industrialized world.[75]

4.5.2 Tolerance

Once equipped with operational definitions and phenomenologico-mathematical theories, the reader of the *Treatise* could enter the realm of higher theory. Maxwell presented the field view, the fluid view, and the relations between the two. Of course, he preferred Faraday's field conception. Compared with the fluid conception, he wrote, it is 'no less fitted to explain the phenomena, and [. . .] though in some parts it may appear less definite, corresponds, as I think, more faithfully with our actual knowledge, both in what it affirms and in what it leaves undecided.' In private, he made fun of the 'learned Germans,' the 'heavy German writers,' or Ampère's 'kind of ostensive demonstration.'[76]

Yet the *Treatise* paid due respect to 'the Newton of electricity' (Ampère) and to the 'eminent' Germans who cultivated action at a distance; and it expounded their theories in sufficient details. This was not only diplomacy: as we will later see, Maxwell integrated some of Ampère's and Weber's atomistics into his own theory. Also, he believed that much could be learn from the comparison between the two kinds of theory:

> In a philosophical point of view, moreover, it is exceedingly important that two methods should be compared, both of which have succeeded in explaining the principal electromagnetic phenomena, and both of which have attempted to explain the propagation of light as an electromagnetic phenomenon [more on this later], and have actually calculated its velocity, while at the same time the fundamental conceptions of what actually takes place, as well as most of the secondary conceptions of the quantities concerned, are radically different.[77]

4.5.3 Field basics

For the essentials of field theory, Maxwell followed Faraday closer than he had ever done. As we have seen, he adopted the field-based definitions of electric charge and current, the concept of conduction as the competition between polarization build up and decay, and the reduction of all electric and magnetic actions to stresses in the field. Even the idea that all currents are closed can be traced back to Faraday's idea of the indivisibility of the electric current (cf. Chapter 3, p. 91). Lastly, Maxwell renounced his earlier theory of magnetism, in which the 'quantity' **B** had sources in

[75] Cf. Maxwell 1873a: Vol. 1, Part 1, Ch. 4 ('General theorems' of potential theory); 2.3.3 (on Thomson's 'Magnetic solenoids and shells'); 1.1.9 ('Spherical harmonics'); 2.3.8 ('Terrestrial magnetism'); 2.4.15 ('Electromagnetic instruments'); 1.2.6 ('Mathematical theory of the distribution of electric currents'); 1.1.13 ('Electrostatic instruments'); 2.4.13 ('Parallel currents').

[76] Maxwell 1873a, Vol. 1: xii; Maxwell to John Clerk Maxwell, 5 May 1855, *MSLP* 1: 294; Thomson to Tait, 1 December 1873, *MSLP* 2: 947; Maxwell to Thomson, 13 November 1854, *MSLP* 1: 255.

[77] Maxwell 1873a: #528; *ibid.*, Vol. 1: xii; Vol. 2, Part 4, Ch. 2 ('Ampère's investigation . . .'); Vol. 2, Part 4, Ch. 13 ('Theories of action at a distance'); Vol. 1: xii.

the magnetic masses of magnets as the electric quantity **D** had sources in charged bodies. In his new theory, **B** was always divergenceless, in conformity with Faraday's notion of magnetic lines of force and with Thomson's flat-cylinder-cavity definition.[78]

For the field equations, Maxwell also depended on Thomson's field mathematics, on the distinction between force and flux, and on the interpretation of Lagrange's equations in terms of energy, force, and momentum. In short, from Faraday's notion of dielectric polarization Maxwell derived the equations

$$\mathbf{D} = \varepsilon\mathbf{E}, \quad \nabla\cdot\mathbf{D} = \rho, \quad \mathbf{J} = \mathbf{j} + \frac{\partial\mathbf{D}}{\partial t} \tag{4.34}$$

for the electric force **E**, the electric displacement **D**, the total current **J**, and the conduction current **j**. From his own theory of magnetization and from the equivalence between an infinitesimal current loop and a magnetic dipole, he deduced

$$\mathbf{B} = \mathbf{H} + \mathbf{I}, \quad \nabla\cdot\mathbf{B} = 0, \quad \nabla\times\mathbf{H} = \mathbf{J} \tag{4.35}$$

for the magnetic force **H**, the magnetic induction **B**, and the intensity of magnetization **I**. From the Lagrangian dynamics of closed currents he obtained

$$\mathbf{E} = \frac{\partial\mathbf{A}}{\partial t} + \mathbf{v}\times(\nabla\times\mathbf{A}) - \nabla\phi, \quad \mathbf{f} = \mathbf{J}\times\mathbf{B}, \tag{4.36}$$

where **A** is the electromagnetic momentum, **v** the velocity of the current carrier, and **f** the electrodynamic force acting on the current carrier. The first formula gives Faraday's induction law if **A** is the vector potential such that $\mathbf{B} = \nabla \times \mathbf{A}$. Maxwell further imposed $\nabla\cdot\mathbf{A} = 0$ in order to simplify the relation between **A** and the total current. Lastly, in the absence of a specific mechanism for the decay of displacement, he admitted Ohm's law $\mathbf{j} = \sigma\mathbf{E}$. In a separate chapter, he gave the formula

$$w = \frac{1}{2}\mathbf{E}\cdot\mathbf{D} + \frac{1}{2}\mathbf{B}\cdot\mathbf{H} \tag{4.37}$$

for the energy density of the field (in the absence of permanent magnetism) and the expression

$$\sigma_{ij} = D_i E_j - \frac{1}{2}\delta_{ij}\mathbf{E}\cdot\mathbf{D} + B_i H_j - \frac{1}{2}\delta_{ij}\mathbf{H}\cdot\mathbf{B} \tag{4.38}$$

[78] Maxwell 1873a: Part 3, Ch. 2: 'Magnetic force and magnetic induction.'

for the stresses in the field. In principle, all mechanical forces of electric or magnetic origin could be derived from these stresses (see Appendix 6).[79]

4.5.4 *Physical ideas and equations*

The sheer number of equations (especially in Cartesian notation) was likely to scare Maxwell's reader. So as to please 'the Chief Musician upon Nabla' (his friend Tait), and for the sake of mathematical power and beauty, Maxwell also wrote his equations in quaternion form.[80] This could hardly help the average reader, as Maxwell himself suspected. A more pedagogical step would have been to eliminate the potentials. Maxwell refused to do so in the *Treatise*, arguing that 'to eliminate a quantity which expresses a useful idea would be rather a loss than a gain in this stage of our enquiry.' He wanted to place the electromagnetic momentum at the forefront of his theory.[81]

In general, Maxwell's use of mathematical symbolism differed essentially from continental or modern practice. For him the equations were always subordinated to the physical picture. He sought consistency, completeness, and simplicity in the picture, not necessarily in the equations. The latter were symbolic transcriptions of partial aspects of the picture, and therefore could not be safely used without keeping the underlying picture in mind. This is quite visible in the way Maxwell treated the electrodynamics of moving bodies. His equations included electromagnetic induction in moving bodies, but not other effects of motion that resulted from his pictures of charge and current. For example, he knew that the convection of electrified bodies constituted an electric current because of the corresponding variation of displacement; but there was no convection current in his equations.

4.5.5 *Microphysics*

Maxwell had another reason not to seek algebraic completeness. He was aware that his theory was essentially incomplete in its treatment of the relation between ether and matter. The general pictures of dielectric and magnetic polarization, and also the idea of currents controlling a hidden motion, implied that ether and matter behaved as a single medium with variable inductive capacity, permeability, and conductivity. Maxwell admitted, however, that some phenomena required a closer look at the interaction between ether and matter. First of all, his picture of electric conduction left the mechanism of polarization decay in the dark. Like Faraday he hoped

[79] Maxwell 1873a: #68, #83, #610 (for eqns. 4.34); #400, #403, #607 (for eqns. 4.35); #598, #603 (for eqns. 4.36); #241 (for Ohm's law); #630, #634 (for eqn. 4.37); #108, #641 (for eqn. 4.38). Maxwell recapitulated the field equations in ##237–38.

[80] Maxwell 1873a: #17, #25, #619. 'Nabla' is an Assyrian harp, of the same shape as Hamilton's ∇; at the BA meeting of September 1871, Maxwell dedicated a poem to Tait, the 'Chief Musician upon Nabla': cf. Campbell and Garnett 1882: 634–6. On the history of quaternions, cf. Crowe 1967. On their use by Maxwell, cf. Harman 1987: 279–82, 1994: 29–30: 1998: 145–53; McDonald 1965; and the related manuscripts and letters in *SMLP* 2.

[81] Maxwell 1873a: #615.

that the study of electric glows, and especially that of electrolysis would shed light on the deeper nature of electricity: 'Of all electrical phenomena,' he declared, 'electrolysis appears the most likely to furnish us with a real insight into the true nature of the electric current, because we find currents of ordinary matter and currents of electricity forming essential parts of the same phenomnenon.'[82]

In his chapters on electrolysis, Maxwell did not follow Faraday's phenomenological approach. As a believer in atomistics, he found it

> extremely natural to suppose that the currents of the ions are convection currents of electricity, and, in particular, that every molecule of the cation is charged with a certain fixed quantity of positive electricity, which is the same for the molecules of all cations, and that every molecule of the anion is charged with an equal quantity of negative electricity.

This assumption accounted for Faraday's law, and could be perfected to explain electrode polarization. But the quantization of charge puzzled Maxwell. It seemed to suggest the existence of 'molecules of electricity,' as if electricity were a discrete fluid. Maxwell bore the contradiction, though not in silence: 'This phrase, gross as it is, and out of harmony with the rest of this treatise, will enable us at least to state clearly what is known about electrolysis, and to appreciate the outstanding difficulties.' He regarded the propounded theory as a provisional mnemonic aid: 'It is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric currents, and so become independent of these provisional theories.'[83]

Yet Maxwell did not doubt that molecular structure played a role in conduction. He also approved Weber's theory of induced magnetism, which required the existence of permanently magnetized molecules. And he took Ampère's and Weber's molecular currents quite seriously. In his opinion, Verdet's finding that magneto-optical rotation had opposite signs in diamagnetic and ferromagnetic bodies excluded Faraday's doctrine that a diamagnetic was nothing but a lesser conductor of magnetism than vacuum. The *Treatise* had a chapter devoted to the improvement of Weber's theory of ferromagnetism, and another to 'the electric theories of magnetism,' including Weber's induced molecular currents. Maxwell emphasized the simplification of the magnetic field equations when all magnetism was reduced to electromagnetism: molecular currents thus became the only sources, and the fields **B** and **H** became identical and divergenceless.[84]

Maxwell also believed that the molecular structure of matter played a role in the propagation of light. He did not trust his field equations for high-frequency vibrations in material bodies. In dielectrics, these equations did not include optical dispersion and implied a relation between optical index and inductive capacity ($n = \varepsilon^{1/2}$)

[82] Maxwell 1873a: #55 (glow), #255 (electrolysis). [83] Maxwell 1873a: #255, #260.

[84] Maxwell 1873a: Vol. 2, Part 3, Ch. 6 ('Weber's theory of magnetic induction,' with a modification explaining residual magnetization); Maxwell to Tait, 23 December 1867, in *SLPM* 2: 336, and Maxwell 1873a: #809 (Verdet excluding Faraday); Vol. 2, Part 4, Ch. 22 ('Electric theory of magnetism'); #835 (simplicity of Amperean view).

that seemed to hold only very roughly. In conductors, they predicted an absorption of light much larger than that measured on gold leaves. In such cases, Maxwell judged, 'our theories of the structure of bodies must be improved before we can deduce their optical properties from their electrical properties.'[85]

Maxwell's equations did not contain the Faraday effect either: their linearity excluded any action of an external magnetic field on the propagation of light. Remember, however, that Maxwell had earlier given a theory of the Faraday effect, based on his vortex model of the magnetic field. In the *Treatise*, he extracted from this model the basic idea of a magnetic vortex motion perturbing the optical vibrations, and cast it in Lagrangian form. He borrowed the unperturbed part of the Lagrangian from the elastic solid theory of light, and assumed the simple expression $k(\nabla \times \partial\boldsymbol{\xi}/\partial t)\cdot\mathbf{H}$ for the energy density of the magneto-optical interaction; $\boldsymbol{\xi}$ represents the elastic displacement of the medium, $\nabla \times \boldsymbol{\xi}$ twice the corresponding rotation, and $k\mathbf{H}$ the vortical motion implied by the magnetic force $\mathbf{H}$. The latter differs from the impressed magnetic force $\mathbf{H}_0$ by the amount $(\mathbf{H}_0\cdot\nabla)\boldsymbol{\xi}$ if the vorticity depends on the displacement $\boldsymbol{\xi}$ in the manner implied by Helmholtz's theory of vortex motion ($\mathbf{H}\cdot d\mathbf{S}$ invariant). Then the optical Lagrangian involves a new term combining $\boldsymbol{\xi}$ and $\partial\boldsymbol{\xi}/\partial t$, from which the magneto-optical rotation is easily deduced. With this semi-phenomenological reasoning, Maxwell avoided atomistic speculation and absorbed the whole effect of matter into one coupling constant, whose value and sign were to be drawn from Verdet's measurements.[86]

This theory of the Faraday effect, and all of Maxwell's attempts to specify the relation between ether and matter, were meant to be provisional. The macroscopic character of his unification of electrodynamics, electrostatic, and optics, conflicted with the empirical need to introduce the molecular structure of matter. Maxwell did not know to what extent his electromagnetic concepts applied at the molecular scale. He avoided microphysical considerations whenever the macroscopic approach proved sufficient.

4.6 Conclusions

Proceeding from Faraday's and Thomson's writings, Maxwell reached the essentials of his electromagnetic field theory stepwise, in three great memoirs. In 'On Faraday's lines of force' his aim was to obtain a mathematical expression of Faraday's field conception. He found the methods of Thomson's field mathematics particularly useful, but modified them substantially. Thomson gave the electric and magnetic (scalar) potentials a central role, as neutral mediators between the mathematics of action at a distance and Faraday's field reasonings. Instead Maxwell made

[85] Maxwell 1873a: ##788–9 ($\varepsilon \sim n^2$ and dispersion); #800 (gold sheets) and also transparency of electrolytes in #799; #789 (quote). Maxwell gave a molecular theory of anomalous dispersion in a Tripos question of 1868 (*SLMP* 2: 419–21, and Rayleigh 1899), also in an 1873 manuscript (*SLMP* 2: 461–2): see Whittaker 1951: 262; Buchwald 1985a: 236; Harman 1994: 11–12.

[86] Maxwell 1873a: #822–7 (Maxwell also included Cauchy's dispersion terms). Cf. Knudsen 1976: 278–81.

the lines of force the central concept of his theory. He threw a geometrical net of lines of force and orthogonal surfaces over Faraday's field, and caught the mathematical field laws directly in terms of the field quantities. He also used Thomson's flow analogy, and extracted from it an essential structural component of his theory: the distinction between intensity and quantity (force and flux). With these modifications of Thomson's methods, Maxwell invented a powerful field-gridding geometry and obtained two circuital laws $\nabla \times \mathbf{H} = \mathbf{j}$ and $\mathbf{E} = -\partial\mathbf{A}/\partial t$ that captured Faraday's intuition of the relations between electricity and magnetism.

In the first part of 'On physical lines of force' Maxwell exhibited a mechanical model of the magnetic field that closely followed Thomson's insights into the vortical nature of magnetism. Unlike the previous flow analogy, this model accounted for the mechanical forces of magnetic origin and for electromagnetic induction. Maxwell soon modified it to include electrostatics and optics, in a manner totally unforeseen by Thomson. This gave the displacement current, the full set of Maxwell's equations, and an expression of the velocity of light in terms of electromagnetic quantities. Although Maxwell acknowledged the artificiality of his model, he firmly believed in the reality of two features: the mutually connected vortical rotations, and the elastic yielding of the connecting mechanism. The rotations represented the magnetic field, and the elastic yielding the electric field (displacement).

In his 'dynamical theory of the electromagnetic field,' Maxwell replaced his vortex model with a dynamical justification of his field equations. He treated the magnetic field as a hidden mechanism, whose motion was controlled by the electric current. The potential **A** thus acquired a central importance as the reduced momentum of the field mechanism dragged by the electric current. Maxwell combined his field equations to obtain a wave equation, and reached a truly electromagnetic optics in which light became a waving electromagnetic field.

The dynamical approach required that the magnetic motion should be determined by the currents only. Accordingly, Maxwell made the displacement current part of the total current. This move brought him closer to Faraday's concepts of charge and current. In the vortex model, the electric current corresponded to the flow of the particles between the vortices and charge to their accumulation. In the new dynamical theory, and more definitely in the *Treatise*, Maxwell defined the electric current as a transfer of polarization, and charge as a discontinuity of polarization. Here polarization was a primitive concept: any attempt to interpret it as a microscopic displacement of electric charge led to absurdities. Maxwell's theory was a pure field theory, ignoring the modern dichotomy between electricity and field.

In the mature form of the *Treatise*, Maxwell's theory had a central core founding the general theory of the electromagnetic field, and a periphery dealing with less understood phenomena. The core contained the pure field theory of electricity with field-based concepts of charge and current, a dynamical derivation of the equations of motion by the Lagrangian method, and the essentials of the electromagnetic theory of light. The periphery included fragmentary mechanisms for the various kinds of electric conduction, and special theories of magnetization and magneto-optical rotation.

The core was essentially macroscopic, in the sense that the basic concepts of field, charge, and current had a macroscopic meaning. It treated matter and ether as a single continuous medium with variable macroscopic properties (specific inductive capacity, magnetic permeability, and conductivity), and avoided speculation on ether models and matter molecules. At the periphery, Maxwell recognized the need for a more detailed picture of the connection between ether and matter. He tried three different strategies. For magnetization, he modified his theory to integrate molecular assumptions; for electrolysis, he proposed a temporary ionic theory that contradicted his general concept of the electric current; for the Faraday effect, his method was essentially based on a phenomenological modification of the optical Lagrangian, although he invoked a deeper molecular mechanism.

By rejecting direct action at a distance and electric fluids, Maxwell distanced himself from continental physics. Whether he did so in a consistent manner has been a major question for Maxwell's commentators. Recent scholarship has established that Maxwell was far more consistent than has usually been admitted. As Siegel has shown in detail, Maxwell's vortex model holds together very well and accounts for all electrodynamic and electrostatic phenomena known to Maxwell. Most of the inconsistencies perceived by earlier commentators of this model can be traced to their failure to distinguish the relevant concepts of charge and current from those proposed in the *Treatise*.[87] Admittedly, there were genuine inconsistencies in the memoir on the dynamical theory due to the unwarranted mixture of Faraday's and Mossotti's concepts of polarization. In the form given in the *Treatise*, however, Maxwell's concepts of charge and current were quite consistent, as Buchwald has most clearly shown. Here Maxwell's readers were often misled by the metaphor of 'displacement of electricity,' which seems to indicate a shift of electric charge (as occurs in the continental concept of polarization), whereas Maxwell only meant something analogous to the shift of a *neutral* incompressible fluid. Charge is not what is displaced, it is a spatial discontinuity in the strain implied in the 'displacement.' As will be seen in the next chapter, the consistency of Maxwell's views comes out clearly in the more pedagogical presentations offered by Maxwell's followers.

Another logorrhea of Maxwellian scholarship has been about the origin of the displacement current. The excessive focus on this question has resulted in a misrepresentation of Maxwell's overall endeavors and achievements in electric topics. As Wise pointed out, Maxwell's first major innovations were an essentially new geometrization of Faraday's and Thomson's field concepts, and the important distinction between quantity and intensity. The former yielded Maxwell's form of the Ampère law ($\nabla \times \mathbf{H} = \mathbf{j}$), and the latter prepared the ground for the dynamical theory. As for Maxwell's path to the displacement current, it may be summarized as follows.

When Maxwell worked out Thomson's notion of a vortical motion in the mag-

[87] Also, some of them were unable to understand the mechanics of the model.

netic field, he introduced the idle wheels as a direct illustration of the current being the curl of the magnetic force. The original purpose of this mechanism was purely electrodynamic. Maxwell knew, however, that both Faraday's electrostatics and the wave theory of light required an elastic medium. He also knew that the mechanical consistency of his model required an elasticity of the rotating cells. When he took this elasticity into account, he found it to imply a new contribution $-\partial\varepsilon\mathbf{E}/\partial t$ to the current $\mathbf{j}$ of idle wheels. The corresponding modification of the Ampère law allowed for open currents.

In such a dense argument, it would be vain to single out a specific reason for Maxwell's introduction of the displacement current. He sought the most complete and consistent theory that would comply with a number of entangled conditions: expression in terms of Faraday's lines of force and the related intensity/quantity pairs ($\mathbf{E}$, $\mathbf{D}$) and ($\mathbf{H}$, $\mathbf{B}$), existence of vortical motion in the magnetic field, integration of the vortical motion in a mechanical model of the ether, possibility of dielectric polarization, identity of the electromagnetic and optical ethers.[88] To make the story even more complex, in his later dynamical theory and in his *Treatise* Maxwell provided a different justification of the displacement current, based on Faraday's concepts of charge and current. Every current became closed and the Ampère law no longer needed to be modified.

Maxwell's electromagnetic theory exemplified a powerful methodology. Important aspects of this methodology can be traced to other British authors. Maxwell praised Thomson and Tait's 'method of cultivating science, in which each department in turn is regarded, not merely as a collection of facts to be coordinated by means of the formulae laid up in store by the pure mathematicians, but as itself a new mathesis by which new ideas may be developed.' This approach included the dynamical ideas through which the 'two Northern wizzards' conducted their mathematical reasonings. It also provided the illustrations and analogies that Maxwell shared with Thomson. The basics of field mathematics were not born in the brains of pure mathematicians. They required the suggestive imagery of flowing liquids and strained solids.[89]

Maxwell's methodology had more original components. He developed the classification of mathematical quantities as a short-cut through the method of formal analogies. He gave more weight to geometrical reasoning than Thomson did, and filled his papers with beautiful figures of curving lines and surfaces. He had an eye for topological relations, as today's field theorists do. Lastly, he inaugurated a moderate kind of mechanical reductionism, in which the connecting mechanism was no longer exhibited. The mere assumption of the existence of such a mechanism implied the existence of a Lagrangian, from which the evolution of empirically controllable quantities could be deduced. Maxwell still hoped, however, for a more detailed mechanical understanding of field processes. For the time being he made sure that Lagrangian dynamics would not be too abstract. He fleshed it out with metaphors, illustrations, and energetics.[90]

Regarding consistency, economy, and pedagogy, Maxwell's *Treatise* was

[88] Cf. Siegel 1975. [89] Maxwell 1873c: 325.
[90] Cf. the penetrating analysis in Harman 1987.

imperfect, even in its core. For example, Maxwell did not fully realize the ambiguity of his potentials; he refused to eliminate them from the final equations; and he misled many of his readers with his metaphor of displacement. In the periphery, he tolerated the contradiction of quantized electric charge, and he occasionally regressed to the elastic solid theory of light. However, the system of the *Treatise* was sufficiently definite to guide further improvements. Maxwell defined a new kind of theoretical physics in which the classification of mathematical quantities, vector symbolism, and Lagrangian dynamics became major construction tools. He also revealed a tension between field macrophysics and the atomic structure of matter, and inaugurated ways of dealing with this tension. His physics was an unended quest. He provided methods that kept theory open and alive.

5

British Maxwellians

5.1 Introduction

Maxwell's electromagnetic theory remained a private enterprise until the early 1870s. The situation began to change after Maxwell's appointment at the head of the new Cavendish Laboratory in 1871 and the publication of the *Treatise* in 1873. This was a slow process, because the *Treatise* was 'a very hard nut to crack' even to Cambridge wranglers, and because in his new capacity Maxwell could not effectively direct theoretical researches. Yet some English-speaking students of electromagnetism, in Cambridge and elsewhere, were now exposed to the new doctrine. Some of them became Maxwell's disciples and apostles.[1]

That Maxwellian studies did not bloom earlier should not be too surprising. In the forms given in 1862 and 1865 Maxwell's theory was too provisional to effectively challenge well-established conceptions. In addition, the man who would have had the strongest power to publicize Maxwell's ideas, Sir William Thomson, did not do so much. His silence even turned into open hostility after Maxwell's death in 1879. Before studying the later reception of Maxwell's electrodynamics, we will first examine why its main inspirer did not endorse it. This will help define Maxwell's originality and dissolve the myth of the evident superiority of his theory.

5.2 Thomson's antipathy

In his Baltimore lectures, delivered in the fall of 1884, Thomson expressed his 'immense admiration' for Maxwell's vortex model and his interest in linking the velocity of light with electromagnetic measurements. Yet he judged Maxwell's electromagnetic theory of light to be 'a backward step from an absolutely definite mechanical motion' as given by Fresnel and his followers. He insisted upon 'the plain matter of fact dynamics and the true elastic solid as giving what seems to me the only tenable foundation for the wave theory of light in the present state of our knowledge.' He could not accept Maxwell's retreat from mechanical models and

[1] Nanson to Maxwell, 5 December 1873, quoted in Warwick [1999], Section 6.3. For an insightful study of the uses of Maxwell's *Treatise* in Cambridge, cf. Warwick, *ibid.*: Ch. 6.

could not regard the Lagrangian treatment of hidden mechanisms as a sufficient mechanical foundation:[2]

I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I can understand it. As long as I cannot make a mechanical model all the way through I cannot understand; and that is why I cannot get the electromagnetic theory. I firmly believe in an electromagnetic theory of light, and that when we understand electricity and magnetism and light we shall see them all together as parts of a whole. But I want to understand light as well as I can, without introducing things that we understand even less of. That is why I take plain dynamics; I can take a model in plain dynamics. I cannot in electromagnetics.

It should be noted that Maxwell himself regarded the more abstract dynamical methods as only provisional and deplored his incapacity to 'take the next step, namely, to account by mechanical considerations for these stresses in the dielectrics.' Also, his first works on the electromagnetic theory depended on specific mechanical models. Yet even there Maxwell differed from Thomson. From his analogies Maxwell extracted distinctions and notions that were alien to the primary field of study, and he believed these to transcend specific geometrical or mechanical models. Essential components of his theory were obtained in this manner: the distinction between flux and force, the displacement current, and the expression of stresses. Thomson distrusted such adventurous use of analogy.[3]

Similarly, Thomson must have felt that Faraday's electrostatics overplayed the analogy between a vacuum and a material dielectric. There was no empirical evidence that a vacuum could be polarized, and Thomson's own theory of dielectrics indicated that polarization charges in material dielectrics and standard electrostatics were sufficient to explain all of Faraday's results. Hence for Thomson there was no vacuum- or air-polarization, and no displacement current. He believed that the transmission of electrostatic force did not involve electric currents, that it was much faster than the propagation of light, and that it probably had to do with the missing compression waves in the elastic solid theory of light. He condemned Maxwell's idea of transverse electric waves as pure fantasy.[4]

From an empirical point of view rigidified in Thomson's studies of telegraphic lines, electrostatic and electrodynamic interactions were essentially distinct. The electrostatic potential was in itself a physical entity, whose propagation from varying electrostatic charges could be discussed separately.[5] On the contrary, for Maxwell

[2] Thomson 1884: 132, 6, 270–1. Cf. Smith and Wise 1989: 463–1; Harman 1987: 267–8, 290–91; Knudsen 1985: 177–8; Siegel 1991: 159–60.

[3] Maxwell 1873a: #111. Cf. Wise 1981: 19–21.

[4] On dielectrics, cf. notes of Thomson's Glasgow lectures by William Jack, 1852–53, quoted and discussed in Wise and Smith 1987: 332–3, Smith and Wise 1989: 226–7, 451. On electrostatic retardation, cf. Thomson 1884: 6, 42. Unfortunately, Thomson never explained his dislike of the displacement current. The present interpretation assumes a deep interconnection between his views regarding dielectrics (in the Glasgow lectures), his much later ideas on potential retardation, and his theory of telegraph cables.

[5] Thomson 1884: 5–6, 41–3. Thomson believed that a spherical conductor submitted to a periodic potential would emit spherical longitudinal waves traveling much faster than light (*ibid.*: 41–2, and Thomson 1896). He also considered the case of periodic motion of an electrified conductor, and argued

any variation of the electric potential implied a dielectric current and therefore an electrodynamic coupling with other currents. When in 1888 Thomson faced the discovery of electromagnetic waves and the ensuing excitement of British Maxwellians, his first reaction was defensive. He still believed that the electrostatic potential propagated separately, and tried to prove that Maxwell's 'ingenious [. . .] but not wholly tenable hypothesis' of the displacement current had absurd implications for the telegrapher's closed conduction currents.[6]

Inside a homogenous conductor without changing electrification ($\nabla \cdot \mathbf{j} = 0$), Maxwell's equations lead to the equation $\mu(\sigma + \varepsilon\partial/\partial t)\partial\mathbf{j}/\partial t = \Delta\mathbf{j}$ for the conduction current $\mathbf{j}$, which differs from the prediction of previous electrodynamic theories by the ε term. Thomson judged this could not be right 'according to any conceivable hypothesis regarding electric conductivity, whether of metals, or stones, or gums, or resins, or wax, or shellac, or india-rubber, or gutta-percha, or glasses, or solid or liquid electrolytes.' A Maxwellian would have replied that the new term was not a matter of conduction: it was a small correction due to the radiation of electromagnetic energy by the variable current. But Thomson seems to have excluded any consideration that would alter the structure of his telegraph theory.[7]

Of course, Thomson did not deny the need to extend electrodynamics to incomplete circuits. However, this could be done without the displacement current, as Helmholtz had already shown (see Chapter 6). Thomson proposed the generalization that gave the simplest equations for the potentials and thus 'simple and natural solutions, with nothing vague or difficult to understand, or to believe when understood, by their application to practical problems, or to conceivable ideal problems, such as the transmission of ordinary telephonic signals along submarine telegraph conductors, and land lines, electric oscillations in a finite insulated conductor of any form, transference of electricity through an infinite solid, &c. &c.' The practical imperatives of the present dominated his approach to electrical problems. He would not adopt more speculative theories, unless they were supported by a plain dynamical ether, the elastic solid or something better.[8]

For a while Thomson could not completely resist the wave of enthusiasm which followed Hertz's 'verification' of Maxwell's theory. In January 1889 he declared that Maxwell's theory marked 'a stage of enormous importance in electro-magnetic doctrine.' In his 1893 preface to Hertz's *Electric Waves* he praised Maxwell's 'splendidly developed theory.'[9] However, he kept speculating on alternative theories and let FitzGerald know how strong his dislike of the newer Maxwellian symbolism

that the phase retardation of the corresponding potential was in principle measurable (Thomson to Heaviside, 6 November 1888, quoted in *HEP* 2: 490, and discussed in Hunt 1991a: 186–7). Cf. Wise and Smith 1987: 340–1; Smith and Wise 1989: 461–3, who insist on the telegraphic context of Thomson's views.

[6] Thomson 1888: 543. Cf. Knudsen 1985: 172–3; Smith and Wise 1989: 477–8; Hunt 1991a: 162–4.

[7] Thomson 1888: 543.

[8] Thomson 1888: 544. Cf. Smith and Wise 1989: 480. Thomson's equations were identical to the Neumann case ($k = 1$) of Helmholtz's equations.

[9] Thomson 1889: 490; Hertz 1893: xiii. Cf. Hunt 1991a: 167.

(Hertz's and Heaviside's) was: 'It is mere nihilism, having no part of lot in Natural Philosophy, to be contented with two formulas for energy, electromagnetic and electrostatic, and to be happy with a vector and delighted with a page of symmetrical formulas.'[10]

5.3 Picturing Maxwell

5.3.1 Lodge's cord and beads

Even for British physicists Maxwell's notions of charge and current were difficult to grasp. An important task of Maxwell's followers was to explain and clarify these conceptions for a wider audience. The first man to do this was Oliver Lodge, a clay merchant's son who had struggled to escape his father's business and become a physicist. Lodge had no great mathematical skill and no Cambridge education (he got his doctoral degree from University College London). Chiefly an experimenter, he reasoned in terms of sophisticated models and pictures that explained or suggested various phenomena without any calculation.[11]

In 1876 his efforts to understand Maxwell's *Treatise* yielded his first model of Maxwellian charge and current. He imagined and constructed the device of Fig. 5.1, in which an inextensible cord circulates over the pulleys ABCD. The weight W corresponds to an electromotive force, the clamp S to a switch (of infinite resistance), and the eight beads typify atoms of matter. The motion of the cord corresponds to Maxwell's total current. In a dielectric, the beads are firmly attached to the cord, and their elastic links with the rigid supports are stretched when the cord is pulled. This stretching represents electric displacement. The excess of cord at A represents positive charge, and the defect of cord at B negative charge. In a conductor, the beads can slide on the cord. Hence the stretching of the supporting threads is smaller, and vanishes when there is no current. Viscous friction between the beads and the cord represents electric resistance.[12]

Lodge extended his device to explain disruptive discharge, electric absorption, charge by induction, and even electrolytic conduction. The model made clear that Maxwellian charge was a discontinuity in a state of strain, and that electricity in Maxwell's sense could not accumulate anywhere. It helped many physicists and engineers understand Maxwell.[13] However, it reinforced Maxwell's metaphor of the incompressible fluid and suggested that displacement was an actual shift of some substance in the direction of the electromotive force, that something was flowing along electric currents.

[10] Thomson to FitzGerald, 9 April 1896, quoted in Thompson 1910, Vol. 2: 1065. After 1888, Thomson still did not understand that Maxwell's theory did not admit instantaneous propagation of *physical* effects. See his 1896 polemic with FitzGerald as discussed in Wise and Smith 1987: 340–2.

[11] Cf. Hunt 1991a: 25–26; Lodge 1931.

[12] Lodge 1876, and a more elaborate form in Lodge 1889: 32–62. Cf. Hunt 1991a: 88–9.

[13] For example, Henry Rowland benefitted from reading Lodge's article: cf. Buchwald 1985a: 78–9. *Modern Views* (Lodge 1889) was a bestseller.

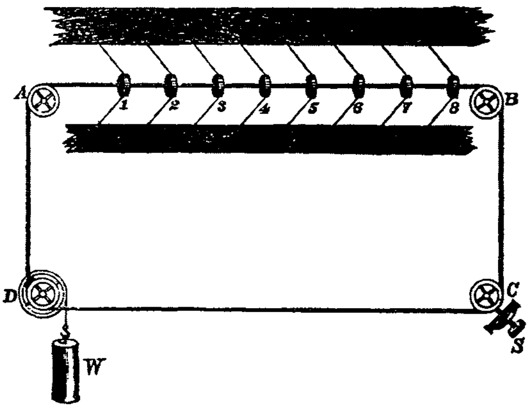

FIG. 5.1. Lodge's cord-and-beads model for a partly dielectric circuit.

5.3.2 Poynting's energy flux

In 1884 John Henry Poynting, third wrangler and Professor of physics at Birmingham, challenged the picture of the electric current as a flow. Unlike Lodge, Poynting was skeptical of any mechanical representation of Maxwell's theory and focused on the more directly observable aspects of the theory: Faraday's lines of force and the energy distribution in the field. With the Cambridge fluency in differential equations and geometrical representations, he had no difficulty answering the question: 'How does the energy about an electric current pass from point to point—that is, by what paths and according to what law does it travel from the part of the circuit where it is first recognisable as electric and magnetic to the parts where it is changed into heat or other forms?'[14]

The question seems obvious to a modern reader. It was not to Maxwell's and Poynting's contemporaries. Energy considerations usually concerned global input and output in a spatially extended system. When Maxwell localized energy in the electromagnetic field, or when the elastic solid theorists expressed the elastic energy, they did not discuss local energy flows. The only exception was the case of light, perhaps as a survivance of older substantial theories. Maxwell himself limited his discussion of energy flow to the case of plane electromagnetic light waves. It was Rayleigh who gave the first considerations of energy flux in a continuum in his *Theory of Sound* of 1877–1878. Poynting was aware of this source when he examined the question of the energy flux in the electromagnetic field.[15]

[14] Poynting 1884: 176. Cf. the obituaries by J. J. Thomson and J. Larmor in Poynting 1920: iv–xxiii, xxiv–xxvi.

[15] Maxwell 1865: 587–8; Poynting 1885a (read on 8 November 1883), where the velocity of sound is derived by consideration of the energy flux following Rayleigh. On the novelty of Poynting's ideas, cf. Buchwald 1985a: 41–3. On the connection with Rayleigh and Cambridge physics, cf. Warwick [1999]: Ch. 6. In his lectures on mechanics published in 1876 (Kirchhoff 1876: 311), Kirchhoff showed that the time derivative of the energy of sound in a given volume was the sum of a surface integral and a volume

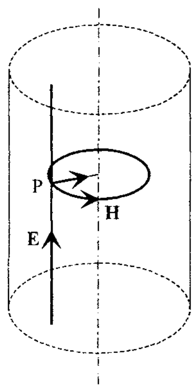

FIG. 5.2. Electric and magnetic line of force passing through a point P of a cylindrical conductor. The arrow at P indicates the direction of the energy flux.

Taking the time derivative of the integral U of the energy density $\frac{1}{2}(\varepsilon E^2 + \mu H^2)$ over a volume V delimited by a surface S, using Maxwell's equations, and integrating by parts, Poynting found

$$\frac{\mathrm{d}U}{\mathrm{d}t} = -\int_V \sigma E^2 \mathrm{d}\tau - \int_S (\mathbf{E}\times\mathbf{H})\cdot \mathrm{d}\mathbf{S} \tag{5.1}$$

for bodies at rest. Since the first term represents energy lost into Joule heat, the second must be identified with the energy flux across the surface. Without hesitation, Poynting took $(\mathbf{E} \times \mathbf{H})\cdot \mathrm{d}\mathbf{S}$ to represent the energy flux across the surface element $\mathrm{d}\mathbf{S}$. The consequences for the energy paths in usual electric circuits were astonishing.[16]

First consider the case of a long cylindrical conductor (Fig. 5.2) conveying a constant current. The lines of magnetic force are circles centered on the axis of the cylinder, and the lines of electric force within the conductor are parallel to this axis. Therefore, the energy flux within the conductor is inward radial, which implies that the energy comes from the surrounding dielectric and is gradually transformed into heat. Consider now the case of a condenser ALBN slowly discharged through a thin wire LMN (Fig. 5.3). If the wire runs along a line of force and if the current is small, the equipotential surfaces remain those of the disconnected condenser. The energy must flow on these surfaces, since it is perpendicular to the electric force. Specifically, energy travels from the condenser through the dielectric to the wire, which it

integral depending on the included sources (his aim was to give a Gaussian proof of the existence and uniqueness of the velocity potential). In his *Theory of Sound* (Rayleigh 1877–1878: #295), Rayleigh reproduced this derivation, and interpreted the surface term as the energy flux across the surface.

[16] Poynting 1884: 176–81. Cf. Buchwald 1985a: 44. Poynting included the motion of current carriers in his balance.

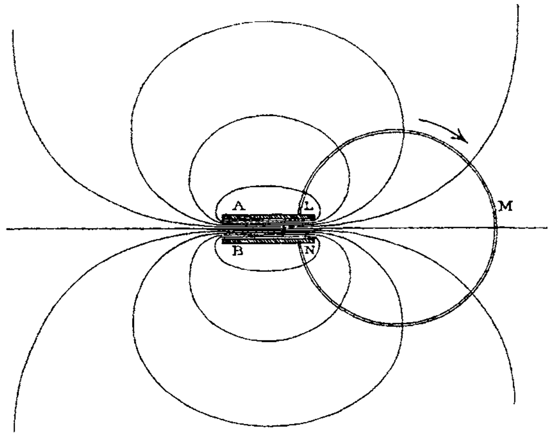

FIG. 5.3. Lines of energy flow for a slowly discharging capacitor (Poynting 1884: 183).

enters perpendicularly. No energy flows along the conductor, against the prevailing intuition.[17]

5.3.3 Moving tubes of force

In his positivistic manner, Poynting regarded Faraday's lines of force and the distribution and flow of energy as the basic properties of the electromagnetic field. In a second paper he tried to connect the energy flow to the behavior of the lines of force. Faraday had already introduced moving magnetic lines of force, so that the electromotive force induced in a linear conductor at rest would be equal to the number of lines cutting the conductor. More generally, Poynting assumed that every circuital electromotive force was due to the motion of tubes of magnetic induction, and also that every 'magnetomotive force' was due to the motion of tubes of electric induction.[18]

Consider in this light the cylindrical conductor of Fig. 5.2. The circular magnetic force around the wire corresponds to a sideways motion of the tubes of electric induction toward the wire. This motion, and a similar motion of the magnetic tubes of induction account for the energy flux into the wire. Poynting then proposed a fitting picture of the electric current: 'The wire is not capable of bearing a continually-increasing induction, and breaks the tubes up, as it were, their energy appearing finally as heat.' He thus maintained Faraday and Maxwell's idea of conduction as a relaxation of polarization, with an essential difference: the polarization now propagated sideways and came from the surrounding dielectric.[19]

[17] Poynting 1884: 181–84. Cf. Buchwald 1985a: 44.

[18] Poynting 1885b. Cf. Buchwald 1985a: 45–9.

[19] Poynting 1885b: 199. J. J. Thomson later regarded Faraday's lateral pressure as the cause of the motion of the tubes: see, e.g., J. J. Thomson 1895a: 277.

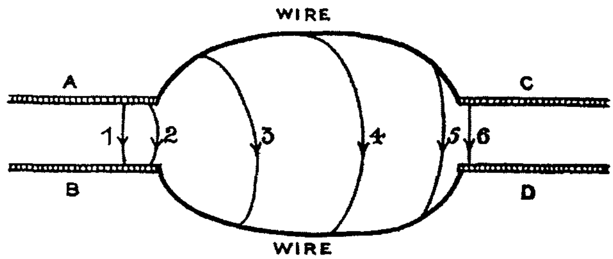

FIG. 5.4. The motion of a tube of electric force during the discharge of a capacitor AB into another CD (Poynting 1885b: 225).

The production of heat is not, however, the most interesting aspect of the electric current. On the simple example of two capacitors (Fig. 5.4), Poynting explained how a pair of wires could transfer energy across space. Originally, the capacitor AB is charged and the capacitor CD uncharged. When the connecting wires are introduced, the induction tubes of the first capacitor move sideways toward the second capacitor, keeping their extremities on the facing sides of the two wires. Opposite electric charges corresponding to these extremities thus travel along the two wires (at their surface), until they are equally distributed between the two capacitors. In this process the tubes are partially dissolved into the wires, which implies a loss of energy into Joule heat. The essential role of the wires, however, is to permit and guide the motion of the induction tubes. Again no energy and no electric charge travels *within* the wires. For an aerial telegraph wire, Poynting explained, the energy travels in the space between the wire and the Earth, with opposite electric charges on their facing surfaces. In a submarine telegraph cable, the energy travels in the insulator between the central copper wire and the surrounding iron sheath.[20]

To cast his ideas into equations, Poynting introduced the vector $\mathbf{A}$ such that $\mathbf{A}\cdot\mathbf{dl}$ gives the total number of tubes of magnetic induction having crossed the length $\mathbf{dl}$ since the origin of time, and the analogous vector $\mathbf{Z}$ in the electric case. The magnetic induction $\mathbf{B}\cdot\mathrm{d}\mathbf{S}$ across the surface $\mathrm{d}\mathbf{S}$ is equal to the total number of tubes having crossed its border. Therefore, $\mathbf{B} = \nabla \times \mathbf{A}$. The electric induction $\mathbf{D}\cdot\mathrm{d}\mathbf{S}$ across the surface $\mathrm{d}\mathbf{S}$ is equal to the number $(\nabla \times \mathbf{Z})\cdot\mathrm{d}\mathbf{S}$ of electric tubes having crossed its border minus $(\int \mathbf{j}\mathrm{d}t)\cdot\mathrm{d}\mathbf{S}$, since by definition the electric current is the number of tubes dissolved in a unit of time. Therefore, $\partial(\nabla \times \mathbf{Z})/\partial t = \mathbf{j} + \partial\mathbf{D}/\partial t$. According to Poynting's principles of tube motion, the electric and magnetic forces corresponding to the motion of the magnetic and electric tubes of inductions are simply given by $\mathbf{E} = -\partial\mathbf{A}/\partial t$ and $\mathbf{H} = \partial\mathbf{Z}/\partial t$. The first equation, up to a gradient term, is the same as Maxwell's induction law in bodies at rest, with a new meaning for the vector potential. The curl of the second equation, together with the above relation between $\mathbf{Z}$, $\mathbf{D}$, and $\mathbf{j}$, retrieves Maxwell's form of the Ampère law, including the displacement current.[21]

[20] Poynting 1885c: 225–7; Poynting 1895: 270–1 (telegraph).
[21] This is a simplified rendering of Poynting 1885b: 212–23.

In general the vectors **A** and **Z** depend on the history of the system, so that no general law of motion can be given for the tubes of force. Poynting does not seem to have been aware of this difficulty. In simple cases he could specify the motion of the tubes, and that was sufficient to convince him and his friend J. J. Thomson of the heuristic power of this picture of the electromagnetic field.[22]

Not all of Maxwell's followers adopted Poynting's notion of moving tubes of force. However, the expression of the energy flux quickly became part of the Maxwellian corpus. Also, Poynting imposed his view of the conduction current 'as consisting essentially of a convergence of electric and magnetic energy from the medium upon the conductor and its transformation there into other forms.' Lastly, he contributed to a clarification of Maxwell's displacement. The term 'displacement,' Poynting explained, was ill chosen because it favored the erroneous view that energy was conveyed along the conductor. Even if a true shift of something was responsible for dielectric strain, Maxwell's **D** did not need to be identical with this shift; it only had to be a function of this shift.[23]

5.3.4 FitzGerald's wheels and rubber bands

Among those who promptly endorsed Poynting's views was Maxwell's Irish follower, George Francis FitzGerald. This tall, humorous man also had 'the quickest and most original brain of anybody,' as Heaviside later judged. He had graduated from and won a Professorship at Trinity College Dublin, which harboured as prestigious a mathematical tradition as Cambridge's. His first contributions to Maxwell's theory—which will be discussed later—were amazing blends of mathematical virtuosity and physical insight. Unlike Poynting, FitzGerald was very philosophical. His personal synthesis of Berkeley's idealism and practical materialism led him to expect a reduction of electromagnetism to matter and motion. He was generally sympathetic to the models of his friend Oliver Lodge: rough and provisional as they were, they could indicate true relations of the ultimate mechanical ether.[24]

Upon reading Poynting, FitzGerald sought a new model of the electromagnetic ether that would illustrate the new ideas on energy flux, electric conduction, and displacement. Maxwell's old vortex model and Lodge's more recent models could not do, since they involved a flow of electricity. However, FitzGerald retained two essential components of Maxwell's model: that magnetic force corresponded to local rotation, and that dielectric strain corresponded to an elastic yielding of the mechanism connecting the rotations. In its two-dimensional version, his model consisted in an array of wheels mounted on fixed axes and connected in pairs by elastic rubber bands

[22] On J. J. Thomson, cf. Chapter 7, pp. 295–300; Buchwald 1985a: 49–53.

[23] Poynting 1884: 192; *ibid.*, with a reference to Glazebrook 1881, for whom **D** was the Laplacian of the actual elastic displacement.

[24] Heaviside to Perry [February 1901], quoted in Hunt 1991a: 8. Cf. Lodge's, Larmor's, and Trouton's contributions to the 'Introductory and biographical' of FitzGerald 1902: xix–lxiv; Hunt 1991a: 6–11.

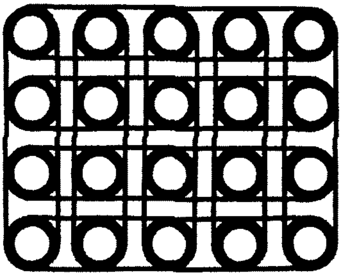

FIG. 5.5. FitzGerald's rubber band model of the electromagnetic field (from Hunt 1991a: 79, used by permission of Cornell University Press).

(Fig. 5.5). The built version was 'rather pretty on a mahogany with bright brass wheels.' FitzGerald used it both for pedagogical and for research purposes.[25]

In this model, the rotation velocity of the wheels represents the magnetic force, and the difference of strain between the two sides of a rubber band represents the electric displacement in the perpendicular direction. In a conductor, the rubber bands can slip on the wheels, thus generating frictional heat. A domain of perfect conduction may be illustrated by removing all the elastic bands in this domain. In order to understand the workings of the model, we may consider the charge and discharge of a condenser. The plates of the condenser and the connecting conductor define an H-shaped region without elastic bands. Charge is obtained by rotating the wheels bordering the channel and bringing back the elastic bands in the channel. The resulting self-locked strain in the region between the two plates corresponds to dielectric polarization. Now suppose that the elastic bands in the channel can slide on the wheels, though with much friction. The strain will be gradually released, its energy being transformed into frictional heat in the channel. In this process the energy travels in the length of the strained elastic bands, that is, in the direction perpendicular to the channel. More generally, the energy flux conveyed by the bands is perpendicular to the displacement, in conformity with Poynting's doctrine.[26]

FitzGerald also discussed oscillatory and sparking discharges, electrostatic induction, and electromagnetic induction. In every case the model faithfully reproduces the predictions of Maxwell's theory. In fact the basic equations of motion are the same. A different rotation of two consecutive wheels implies a different strain of the two sides of the connecting band, or $\nabla \times (\int \mathbf{H} dt) = \mathbf{D}$ in electromagnetic language. If ε measures the elasticity of the bands and μ the angular inertia of the wheels, the bands on a given wheel impress a net torque $\nabla \times (\mathbf{D}/\varepsilon)$, which must be equal to the time variation of the angular momentum $\mu\mathbf{H}$ according to the laws of dynamics. The two circuital laws of Maxwell's theory are thus retrieved.[27]

[25] FitzGerald 1885a, 1885b; FitzGerald to Lodge, 3 March 1894, quoted in Hunt 1991a: 78–9. A three-dimensional extension of the model is sketched in FitzGerald 1885b: 160–1.

[26] FitzGerald 1885a: 143, 145; 1885b: 157–9. For a clear, illustrated explanation, cf. Hunt 1991a: 78–83.

[27] FitzGerald 1885a: 147–8. Instead of Maxwell's **D** and **B**, FitzGerald used their counterparts in MacCullagh's medium, as he had done in 1879 (see below, pp. 190–2).

FitzGerald's model provided an excellent illustration of the central features of Maxwell's theory as Maxwell's followers came to understand it. It showed that displacement was a local change of structure, that nothing circulated along an electric current, that electric charging presumed conduction, and that energy circulated in a direction perpendicular to the electric force. However, as FitzGerald himself emphasized, the model did not represent the connection between ether and matter. Matter was required 'to get a hold on the ether so as to strain it.' It was also necessary to produce electrostatic attractions, because the stress of the rubber bands was linear instead of quadratic. It certainly played a role in magnetized bodies and in the Faraday effect. This raised a difficult question: could there be a simple mechanical representation of Maxwell's system that integrated the effects of matter?[28]

5.3.5 Lodge's cogwheels

Undeterred by this sort of difficulty, Lodge trusted that he could invent a field mechanism for all electromagnetic processes. In 1879 he had already speculated that the ether was made of wheels of positive and negative electricity geared to one another, as in Fig. 5.6. The rotation of the positive wheels (or the opposite rotation of the negative ones) represented the magnetic force, and their elastic yielding corresponded to electric displacement, as in Maxwell's earlier model. Lodge's innovation was the introduction of two electricities instead of one, which he believed to be necessary to explain the double electrolytic motion, the lack of intrinsic momentum of the electric current, and the existence of both positive and negative electric winds, among other things. Ten years later, he published an improved version of this model in his best-selling *Modern Views of Electricity*. Adopting FitzGerald's notion of conduction as a slip in the mechanical connections, he replaced the cogwheels with smooth wheels within conductors (Fig. 5.7). His depictions of basic field processes were similar to FitzGerald's, despite the complication introduced by the two kinds of wheels.[29]

Unfortunately, Lodge multiplied models without clearly stating their limits and interrelations. For the capacitor alone he offered three different models: the cord-and-beads, a hydro-pneumatic device, and the cogwheels. Even though he had the ambition of covering the whole field of electricity and magnetism with a single consistent model, he ended up illustrating various phenomena by a variety of incompatible models. His *Modern Views* prompted Pierre Duhem's famous statement:[30]

[28] FitzGerald 1885a: 142–3: 'I do not intend the model to illustrate at all the connexion between the ether and matter, and indeed think it one of the advantages to be derived from studying this model that it so distinctly emphasizes the distinction between the phenomena, depending on the general properties of the ether itself, and those depending on its connexion with matter.' *Ibid.*: 144, FitzGerald added a clever system of threads to his model in order to represent electrostatic attractions and give a rough idea of the relevant connection between ether and matter.

[29] *BAR* 1879: 258, and Lodge to FitzGerald, 29 February 1880, quoted in Hunt 1991a: 31; Lodge 1889: Chs. 10–11. Cf. Hunt 1991a: 30–1, 89–92.

[30] Duhem 1914: 101. Cf. Hunt 1991a: 87–8.

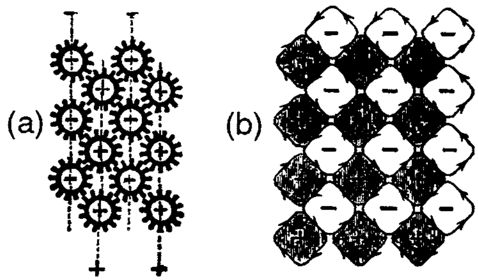

FIG. 5.6. Lodge's cogwheel ether (a), and an improved version (b) (Lodge 1889: 179, 180).

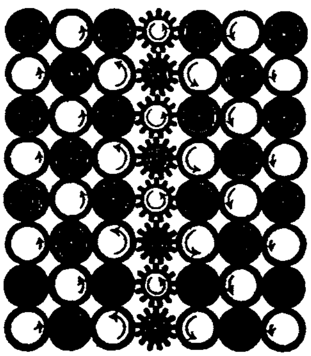

FIG. 5.7. Lodge's illustration of a direct and return current in two conductors (round wheels) separated by a thin insulating layer (cogwheels) (Lodge 1889: 189).

Here is a book intended to expound the modern theories of electricities and to outline a new theory. In it there are nothing but strings running over pulleys, wrapping around drums, going through beads, and carrying weights; and tubes pumping water while others swell and contract; wheels gearing each another and forming pinions for racks. We thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory.

Duhem took Lodge's book to be typical of the British inclination for mechanical models. In reality this was an extreme case, even from a British point of view. Poynting, who was generally suspicious of mechanical models, declared that Lodge's explanations were 'merely hypothetical' and were 'solely of value as a scaffolding enabling us to build up a permanent structure of facts, i.e. of phenomena affecting our senses.' He also found that the cogwheel model contradicted an essential feature of Maxwell's theory, the intimate relation between conduction and displacement. In Lodge's cogwheel model, the displacement current involved an actual displacement of the two electricities, whereas the conduction current involved no displacement whatsoever. Poynting kindly offered a modification of Lodge's model in which the conduction current was a double procession of the cogwheels. Nevertheless, he found his moving tubes of force 'much more easy to deal with' and more likely to guide future research: 'I believe that we may symbolise electric and magnetic actions by means of lines of force and their motions in a way which allows us to think clearly of the phenomena, and though the ultimate nature of the lines of force is unknown, we can only say the same of the ether.'[31]

5.3.6 The vortex sponge

Not even Lodge's dear friend FitzGerald liked the cogwheels. By distinguishing positive and negative wheels Lodge had disturbed the central Maxwellian dogma that electric charge was no more than strain discontinuity. FitzGerald exclaimed: 'Oh! I think your model is horrid!' In his philosophy, ether could only be something very simple: a continuous fluid in motion. William Thomson had shown in 1880 that a mass of ideal fluid could exist in a state densely filled with randomly oriented vortex filaments. Because of the gyrostatic inertia of the vortices, the fluid acquired some rigidity and could propagate transverse waves. In 1885, soon after designing the rubber band model, FitzGerald speculated that the ether was such a 'vortex sponge.' The freedom in the arrangement of the vortices offered an *embarras de richesses* for retrieving the known properties of the ether. Until the end of his short life (he died in 1901), FitzGerald and a few sympathizers struggled to construct a convincing ether out of a vortex sponge. They encountered insurmountable mathematical difficulties. The vortex sponge was the string theory of those days: its basis was attractively simple, it could not be refuted, but it could not be developed far enough to be verified.[32]

5.4 Modifying Maxwell's equations

The electromagnetic field equations of the *Treatise* could not be generally valid, whatever the underlying picture was. The clearest hole in their consequences was the Faraday effect. For this special intervention of magnetized matter, Maxwell had to return to the elastic solid theory of the ether. Yet this effect suggested the rotary character of magnetism, an essential feature of Thomson's and Maxwell's conception of the magnetic field. Aware of this strange situation, Maxwell's followers were much interested in magneto-optics. When in 1876 the Glasgow physicist John Kerr announced a new phenomenon of this kind, FitzGerald immediately set himself to work.

[31] Poynting 1893: 264, 267, 267–8. Cf. Hunt 1991a: 94–5.

[32] FitzGerald to Lodge, late September 1889, quoted and dated by Hunt 1991a: 92–3; Thomson 1880; FitzGerald 1885a: 154–6; 1888: 236–40; 1889; 1899. Cf. Whittaker 1951: 295–303; Hunt 1991a: 96–104. FitzGerald's attempt belonged to a strong variety of British mechanical reductionism, in which all energy had to be kinetic: cf. Topper 1971, and Klein 1972a.

5.4.1 FitzGerald's 'very important step'

Magneto-optical rotations, Kerr thought, would be much larger if they could be produced in a strongly magnetic substance like iron. The only obstacle was the opacity of iron, which he circumvented by using reflection instead of transmission for the polarized light. He observed that the polarization of light reflected on a polished magnet pole was altered by the magnetization.[33] FitzGerald promptly explained Kerr's observations by decomposing the incident light into two circularly polarized components, and invoking the different refraction index of the magnetized iron for these two components. A more fundamental theory required an extension of Maxwell's theory of the Faraday effect including the boundary conditions between two different media. This is what FitzGerald obtained in 1879.[34]

The problem of boundary conditions in optics was reputed to be difficult. Nearly all elastic solid theories required an *ad hoc* omission of some of the dynamically necessary conditions.[35] The source of the difficulty was that transverse vibrations did not remain so after crossing the border between two media. If the refracted vibration was artificially required to be transverse, not all boundary conditions could be satisfied. Today's physicists know that the electromagnetic theory of light provides correct boundary conditions in a very simple manner and thus eliminates the outstanding difficulty of elastic solid theories. Maxwell did not. Deterred by the apparent complexity of the issue, and distrusting his equations when applied to quickly variable phenomena, he gave up the derivation of the laws of refraction.[36]

Light came to FitzGerald from a fellow countryman, James MacCullagh. In 1839 this brilliant mathematician had cut the Gordian knot of optical theory by choosing the potential energy of the elastic medium so that the true boundary conditions of this medium would yield Fresnel's formulas for the intensities of reflected and refracted rays. If $\boldsymbol{\xi}$ denotes the local shift of the medium and ε its elasticity constant, MacCullagh's potential is simply given by $(1/2\varepsilon)(\nabla \times \boldsymbol{\xi})^2$. MacCullagh then used the action principle of another famous Irish mathematician, Rowan Hamilton, to derive the equation of motion

$$\mu \frac{\partial^2 \boldsymbol{\xi}}{\partial t^2} = -\nabla \times (\varepsilon^{-1} \nabla \times \boldsymbol{\xi}) \qquad (5.2)$$

(μ is the density of the medium) as well as the two boundary conditions: continuity of $\boldsymbol{\xi}$, and continuity of the tangential component of $\varepsilon^{-1}\nabla \times \boldsymbol{\xi}$. By itself, this equation of motion excludes longitudinal waves.[37]

[33] Kerr 1876, 1877. If the incident light is linearly polarized, the reflected light becomes elliptically polarized, and the major axis of the ellipse is rotated away from the original plane of polarization: cf. Buchwald 1985a: 102n, 109–10.

[34] FitzGerald 1876. Kerr and Thomson had the same idea: cf. *ibid.*: 14.

[35] This was true for the theories of Poisson, Cauchy, Green, Neumann, and Kirchhoff, but not for Cauchy's labile ether nor for MacCullagh's medium: cf. Whittaker 1851: Ch. 5; Schaffner 1972.

[36] Maxwell to Stokes, 15 October 1864, *MSLP* 2: 186–8; Manuscript notes on the reflection and refraction of light, *MSLP* 2: 182–5. Cf. Harman 1995b: 79–80, 85–6. The solution of the problem was first announced in Helmholtz 1870, and given in Lorentz 1875.

[37] MacCullagh 1839. Cf. Whittaker 1951: 142–4; Schaffner 1972: 59–68, 187–93; Stein 1981: 310–15; Buchwald 1985a: 283–4; Hunt 1991a: 9–10.

MacCullagh was familiar with Green's memoir of 1838, which gave the most general form of the potential of an elastic solid in terms of two moduli for rigidity and compression. In Green's terms MacCullagh's potential corresponds to a negative compressibility, which Green naturally excluded. Aware of this paradox, MacCullagh simply accepted that the ether was very different from any natural solid. Other physicists thought differently, expecially after Stokes had proved, in 1862, that MacCullagh's medium violated the principle of action and reaction: the *absolute* rotation of an element of the medium calls forth a restoring elastic torque. No one took MacCullagh's ingenious theory seriously, until FitzGerald resurrected it in 1879.[38]

MacCullagh's equation for ether motion, FitzGerald discovered, resulted from Maxwell's theory if only Maxwell's displacement was identified with the curl of $\boldsymbol{\xi}$. Indeed, the Ampère law $\nabla \times \mathbf{H} = \partial \mathbf{D}/\partial t$ then implies $\mathbf{H} = \partial \boldsymbol{\xi}/\partial t$, so that MacCullagh's equation (5.2) becomes identical to $\partial \mathbf{B}/\partial t = -\nabla \times \mathbf{E}$, in conformity with the Faraday–Maxwell induction law. Thanks to this equivalence, FitzGerald could write the boundary conditions in the electromagnetic theory of light. He also gave an electromagnetic interpretation of Maxwell's theory of the Faraday effect.[39]

The latter theory was based on a coupling between the optical motion of the ether and the vortical motion produced by the external magnetic field. As Maxwell explained, this was a hybrid approach, because the optical ether motion received no electromagnetic interpretation.[40] By contrast, in FitzGerald's version of the theory all quantities received a double interpretation, an electromagnetic one, and another in terms of MacCullagh's medium. Adding Maxwell's magneto-optical term to MacCullagh's Lagrangian, FitzGerald derived the equations of motion and the Faraday effect in elegant quaternion form: a true Halmitonian feast. Then he used MacCullagh's boundary conditions[41] to derive the effect of the external magnetic field on the reflection of a polarized light wave. He thus retrieved those of Kerr's observations that did not depend on the metallic character of the second medium.[42]

As a referee of FitzGerald's paper Maxwell commented: 'If he has succeeded in explaining Kerr's phenomena as well as Faraday's by the purely electromagnetic hypothesis, the fact that he has done so ought to be clearly made out and stated, for it would be a very important step in science.' FitzGerald followed this advice in the printed conclusion which Maxwell did not live to see:

The investigation is put forward as a confirmation of Professor Maxwell's electromagnetic theory of light [. . .]; if it induced us to emancipate our minds from the thraldom of a

[38] Green 1838; Stokes 1862. Cf. Schaffner 1972: 66, 71–4.

[39] FitzGerald 1879a. MacCullagh's conditions are the continuity of ξ and the continuity of the tangential component of the torque $\varepsilon^{-1}\nabla \times \xi$. In FitzGerald's electromagnetic interpretation, they imply the continuity of the tangential components of **E** and **H**, and the continuity of the normal components of **B** and **D**, as given by the modern electromagnetic reasoning. FitzGerald eliminated the alternative electromagnetic interpretation, in which ξ is identified with **A**: see Appendix 9.

[40] Maxwell to Stokes, 6 February 1879, in Stokes 1907, Vol. 2: 43. See Chapter 4, pp. 172.

[41] However, he dropped the continuity of the normal component of ξ, because it was not consistent with the magneto-optical term. Cf. Larmor's explanation of the difficulty in FitzGerald 1880: 66n.

[42] FitzGerald 1879a. Cf. Hunt 1991a: 15–21.

material ether [it] might possibly lead to most important results in the theoretic explanation of nature.

This statement should not be read as a renunciation of the mechanical ether. FitzGerald only meant to corroborate MacCullagh's intuition that the ether did not resemble any of the substances found in our material world. The ether was no elastic solid, no jelly, no foam, whatever Stokes and Thomson might think; but it could be a mechanical medium of a very different kind, for instance the vortex sponge which later absorbed FitzGerald's hopes.[43]

Not only did FitzGerald's paper redirect the search for the ultimate mechanical medium, but it also inaugurated a powerful strategy for integrating magneto-optical phenomena into Maxwell's theory without entering microphysical speculation. The basic idea was to modify the field equations in conformity with the dynamical foundation of the theory. FitzGerald did that by first writing a Lagrangian giving Maxwell's field equations, and then adding small new terms to this Lagrangian. The first step was in itself an important innovation, for Maxwell had only written the Lagrangian of a system of linear currents, not including the elastic energy stored in electric displacement.[44]

5.4.2 The Hall effect

One weakness of FitzGerald's theory of the Faraday and Kerr effects was that the new term in the Lagrangian was added in a purely *ad hoc* manner, without independent electromagnetic justification. One year later the young American physicist Edwin Hall discovered that a strong magnetic field, when applied perpendicularly to an electric current, implied an electromotive force in the direction perpendicular to both the field and the current. The effect was very small and required refined galvanometry, which explains why it was not discovered earlier. Varying the conditions of the experiment, Hall found that the new electromotive force could be expressed as $h\mathbf{H}_0 \times \mathbf{j}$, where h is a small constant, $\mathbf{H}_0$ the external magnetic force, and $\mathbf{j}$ the electric current.[45]

As Boltzmann pointed out in 1886, the effect is easily justified by assuming that the electromagnetic force acting on the conductor results from a force acting on the current itself. However, Maxwell insisted that electromotive forces and electrodynamic forces were essentially different things, since in his system currents were not the flow of electric charge. As a good Maxwellian, Hall could not consider Boltzmann's kind of explanation. Instead he ascribed the new electromotive force to a fundamental modification of Maxwell's field equations. So did his mentor Henry

[43] Maxwell to Stokes, 6 February 1879, in Stokes 1907: 43; FitzGerald 1879a: 73. Cf. Stein 1981; Hunt 1991a: 20–3.

[44] For FitzGerald, the magnetic induction **B**, not the current **J**, is the generalized velocity (see Appendix 9). This dynamical method does not include the conduction current. Heaviside 1893–1912, Vol.1: ##146–59, showed that the conduction current cannot be obtained by introducing dissipation in MacCullagh's medium: cf. Buchwald 1985a: 68–70. For the general issue of applying Hamilton's principle to the field, cf. Buchwald 1985a: Chs. 6–7.

[45] Hall 1879, 1880a. Cf. Buchwald 1985a: Chs. 9–10.

Rowland, who soon imagined a connection between Hall's effect and the Faraday effect.[46]

Hall's effect, Rowland reasoned, may be seen as the curving of the conduction current by an external magnetic force. Since in Maxwell's theory conduction and displacement currents are on the same dynamical footing, the latter should also be curved by an external magnetic force. Specifically, Rowland added a term $h\mathbf{H}_0 \times \partial\mathbf{D}/\partial t$ to Maxwell's expression of the electromotive force in a magnetic field, and found that the modification implied the magneto-optical rotation calculated by Maxwell. His theory was undoubtedly superior to Maxwell's, for it was entirely electromagnetic, did not require any reference to ether vortices, and related the Faraday effect to a purely electromagnetic phenomenon. He proudly claimed to have given 'a demonstration of the truth of Maxwell's theory of light,' and more generally considered that Maxwell's theory had been 'raised to the realm of fact.'[47]

Rowland did not compare his theory with FitzGerald's. A Cambridge Maxwellian, Richard Glazebrook, soon did that for him. The Maxwell–FitzGerald addition to the field Lagrangian, $(k\nabla \times \partial\boldsymbol{\xi}/\partial t)(\mathbf{H}_0\cdot\nabla)\boldsymbol{\xi}$, implies a corrective term $-2k(\mathbf{H}_0\cdot\nabla)\mathbf{H}$ in Maxwell's expression of the electromotive force. This differs from $2k\mathbf{H}_0 \times (\nabla \times \mathbf{H})$ by a gradient which can be absorbed in the scalar potential. Therefore FitzGerald's new term is just what is needed to submit the displacement current to the Hall effect. For Cantabrigian physicists weaned with Lagrangians, Glazebrook's argument marked a major step in the unification of physics, as well as a major victory of Maxwell's theory.[48]

Magneto-optics remained a major Maxwellian topic well into the 1890s both in England and on the continent. In 1884 Hendrik Lorentz and his student W. van Loghem pursued Rowland's connection between the Hall effect and magneto-optical rotation, and were first to take into account metallicity in the Kerr effect. However, their theory did not explain why iron and nickel had similar Kerr effects despite the opposite sign of their Hall effects. For this reason in 1893 J. J. Thomson and Paul Drude abandoned Hall's idea that the Hall effect implied a new kind of electric field both for conduction and for dielectric currents. Even so, their theories turned out to be incompatible with Remmelt Sissingh's excellent data on the Kerr effect, published in 1891. As Lorentz's student Cornelius Wind demonstrated in 1898, the difficulty could only be solved by replacing Maxwell's unanalyzed, macroscopic displacement with Lorentz's ionic polarization. From a major vindication of Maxwell's theory, magneto-optical researches had evolved into one of its major

[46] Boltzmann 1886b (who used the effect to determine the velocity of electricity); Maxwell 1873a: #501; Hall 1880b. A minor Maxwellian, John Hopkinson, preferred to modify Ohm's law (Hopkinson 1880). Maxwell himself had conceived the possibility of a rotary part of the resistivity matrix in magnets (Maxwell 1873a: #297): cf. Buchwald 1985a: 96. The modern explanation is similar to Boltzmann's.

[47] Rowland 1880a, 1880b, 1881: 261. Cf. Buchwald 1985a: 102–6. Rowland 1880b included a strange reformulation of Maxwell's theory that caused a public rebuttal by J. J. Thomson (1881b).

[48] Glazebrook 1881. See also J. J. Thomson 1888: #43, and Larmor 1893a. Cf. Buchwald 1985a: 111–9. The main purpose of Glazebrook's paper was to take Thomson and Maxwell's vortical interpretation of magnetism to the letter: he identified $\mathbf{H}$ with the vorticity $\nabla \times \partial\boldsymbol{\xi}/\partial t$ in an elastic solid. This attempt had no sequel, probably because it led to a field-energy distribution different from Maxwell's.

insufficiencies. This change was part of a more general historical transition to be explained in Chapter 8.[49]

5.5 A telegrapher's Maxwell

There was, in the flock of Maxwell's followers, one maverick who ignored magneto-optics, questioned attempts at finding the mechanical structure of the ether, judged that 'so-called models' were 'harder to understand than the equations of motion,' and ridiculed the Cambridge fashion for Lagrangians. Yet the changes he brought to the formulation of Maxwell's theory were of the most lasting value. This man was Oliver Heaviside, the son of a wood-engraver, the nephew of the British inventor of the electric telegraph (Charles Wheatstone)—and 'a first rate oddity' even to his closest friends. After a brief attempt at creative writing (including an essay entitled 'Muscular characters'), he devoted himself entirely to electrical science. He spent most of his life as a virtual ermit. He avoided the society of other scientists, and enjoyed denouncing the incompetences of established authorities. His sarcastic wit won him powerful ennemies, and occasionally compromised the diffusion of his works.[50]

5.5.1 Telegraphic circuits

From a scientific point of view, much of Heaviside's originality came from his seven-year experience as a telegraph operator. Unlike other Maxwellians, most of his researches were aimed at solving or easing the solution of practical problems of telegraphy and telephony. In physics and mathematics he had no academic training and acquired his vast knowledge through reading. He was highly impressed by the writings of the knight of the telegraph, Sir William Thomson. Through his expertise in the Atlantic cable project, Thomson had not only placed the art of electric communication on a sound theoretical basis, but he had also enriched electrical science with reliable standards and measuring techniques. Heaviside later praised Thomson's 'invaluable labours in science, inexhaustible fertility, and immense go.'[51]

Heaviside's first papers were devoted to the theory of the electric circuits and apparatus used by telegraphers. Like Thomson and Kirchhoff, he worked directly in

[49] Lorentz 1884; van Loghem 1883 (Lorentz and van Loghem used Helmholtz's version of Maxwell's theory); J. J. Thomson 1893a; Drude 1893; Sissingh 1891; Wind 1898, 1898–1899. Also, Rowland's idea of modifying Maxwell's expression of the electromotive force must be replaced with the idea of modifying the relations involving the constitutive parameters ε and σ. Cf. the thorough study in Buchwald 1985a: 205–9 (Lorentz–van Loghem); 123–9 (J. J. Thomson); 215–7 (Drude); 210–4 (Sissingh); 242–7 (Wind).

[50] Cf. Yavetz 1995: 276 (no magneto-optics); Heaviside to Hertz, 14 August 1889, quoted in Hunt 1991a: 105 (no models); Buchwald 1985c (no Lagrangians); Searle 1950: 96 (oddity); Appleyard 1930 (muscles). For Heaviside's biography, cf. Whittaker 1929; Appleyard 1930: 212–20; Nahin 1988; Hunt 1991a; Yavetz 1995: 5–28.

[51] Heaviside 1885: 418. Cf. Hunt 1991a: 58. On Thomson and the telegraph, cf. *supra* pp. 122–5, and Smith and Wise 1989.

terms of measurable electromotive force, current, resistance, 'capacitance,' and 'inductance,' and avoided speculation on the deeper nature of electricity. His mathematical solutions were extremely thorough, and proceeded elegantly by a constant return to the physical problem. He had the British intolerance for dry mathematical developments and required a physical interpretation for each step of the reasonings. Conversely, the physical meaning of mathematical operations could suggest to him new mathematical methods. For example, he treated the resistance in a circuit and operators such as *Ld/dt* (*L* being the inductance) as part of an operational 'impedance.' This practice led to the 'operational calculus,' a non-rigorous anticipation of modern distribution theory.[52]

With his 'electrical mathematics' Heaviside solved numerous problems of signal propagation. From a practical point of view, his most important result was the derivation of the possibility of distortionless telephony by inductive loading of the lines. Had he not let an American engineer patent this discovery before him, he would have been rich.[53]

5.5.2 *The principle of activity*

Dynamical considerations played an essential role in Heaviside's physics. 'All the physical sciences,' he declared, 'are bound to become branches of dynamics in the course of time, and anything contradicting the principles of dynamics should be unhesitatingly rejected.' However, he made little use of Thomson's flow analogy for telegraphic cables, and no attempt at specifying the hidden mechanisms. He instead relied on the general dynamical concepts of energy, force, and momentum, as developed in some of Thomson's early papers, Thomson and Tait's *Natural Philosophy*, and Maxwell's *Treatise*. From the early Thomson he borrowed the energetic definition of electromotive forces, from TT′ the more general 'principle of activity,' and from Maxwell the notion of the 'electromagnetic momentum' of an electric current.[54]

By the 'Principle of activity' Heaviside meant Thomson and Tait's interpretation of Newton's scholium to his third law:

> If the Activity [*actio*, so translated in the second edition of TT′ to avoid confusion with Maupertuis's action] of an agent be measured by its amount and its velocity conjointly; and if, similarly, the Counter-activity of the resistance be measured by the velocities of its several parts and their several elements conjointly, whether these arise from friction, cohesion, weight, or acceleration;—Activity and Counter-activity, in all combinations of machines, will be equal and opposite.'

Thomson and Tait interpreted this statement as prefiguring d'Alembert's and Lagrange's formulations of mechanics as well as energy conservation. Heaviside

[52] On Heaviside's circuit theory, cf. Yavetz 1995: Ch. 2 (p. 39 for 'electro-mathematical reasoning'). On the operational calculus, see *ibid.*: 306–20, and Hunt 1991b.

[53] On distortionless transmission, cf. Jordan 1982a; Yavetz 1995: 209–18.

[54] Heaviside 1885: 419; 1878: 95–7 (for a use of the water-pipe analogy); 1885–1887: 451: 'Energy definition of impressed forces due originally, if not explicitly, at least substantially, to Sir William Thomson'; Heaviside 1876: 54, 59, and 1878: 97 for the electromagnetic momentum.

retained the idea of balancing the 'activities,' that is, the rates at which the various forces acting in and on the system perform work. He also adopted Thomson and Tait's generalized concept of force, for which the basic equation 'force × velocity = activity' remains true, even when the 'velocity' no longer refers to the motion of a substance.[55]

5.5.3 Maxwell for the many

Although in his early works on circuit theory Heaviside avoided discussing the nature of electricity, his use of the concept of electric momentum betrayed a sympathy for Maxwell's system. He also shared Maxwell and Thomson's belief that the motion responsible for this momentum was located in the magnetic field. However, he said nothing on the nature of the electric current, although he later remembered that he never accepted the fluid picture:

> It so happened that my first acquaintance with electricity was with the dynamic phenomena, and after I had read with absorbed interest that instructive book, Tyndall's 'Heat as a mode of motion.' This may explain why, when it came later to book learning regarding electricity, I had the greatest possible repugnance to all the explanations, and could not accept the electric current to be the motion of electricity (static) through a wire, but thought it something quite different.

Heaviside then believed that electricity was a mode of motion and the electric current something similar to heat flow. He was prepared to accept another non-substantialist view: Maxwell's.[56]

In 1882 Heaviside started a series in *The Electrician* on 'the relations between magnetic force and electric current' according to Maxwell. He wanted to strip the 'higher conceptions' of 'eminent mathematical scientists of their usual symbolical dress' and to make them 'appeal to the sympathies of the many.' To make Maxwell more accessible to the intelligent telegrapher, he invented the modern vector notation, gave geometrical definitions of the curl and divergence operators, and proved the corresponding integral theorems. His method was largely reminiscent of Maxwell's 'On Faraday's lines of force.' For example, he proved Stokes's theorem by means of a net of infinitesimal loops; he introduced a series of vectors **A**, **B**, **C** (and even a fourth one) deduced from each other by 'curling' and representing the vector potential, the magnetic force, and the current; and he used symmetry arguments to determine particular distributions of magnetic force. In Thomson's and Maxwell's manner, Heaviside imparted life to his symbols by relating them to simple geometrical operations or physical processes. His vector notation, and Maxwell's

[55] Heaviside 1893–1912, Vol. 3: 178; Thomson and Tait 1879–1883, Vol. 1: 247; Heaviside 1883–1884: 291; 1885–1887: 435. Cf. Hunt 1991a: 122–3; Yavetz 1995: 131–6, 269–71. On the reference to Newton in TT′, cf. Smith 1998: Ch. 10.

[56] Heaviside 1885–1887: 435, 436. Cf. Yavetz 1995: 143–4.

'curl' and 'convergence' did not only save writing. They provided intuitive guidance in the mathematical thicket of Maxwellian electromagnetism.[57]

In his next series Heaviside argued in favor of Maxwell's way of distributing the magnetic and electric energies in the field. In this context his most decisive insight occurred in 1884 during a study of the currents induced in a conducting core within a solenoid. Having in view applications to electromagnets, transformers, and self-inducting coils, he focused on the energy processes in the core. Combining the Ampère law, Faraday's induction law, and Ohm's law, he computed the time variation of the magnetic energy density, and found

$$\frac{\partial}{\partial t}\left(\frac{1}{2}\mu H^2\right) = -\sigma E^2 - \sigma^{-1}\nabla\cdot(\mathbf{j}\times\mathbf{H}). \tag{5.3}$$

The first term represents the Joule heat and the second an energy flux directed toward the axis of the core. A year later Heaviside generalized this result to include displacement currents, and reached the general expression $\mathbf{E} \times \mathbf{H}$ of the energy flux, independently of Poynting. He described the energy transfers in simple electric systems, and he predicted that quickly varying currents, such as those involved in rapid signaling, would be confined to the surface of the conductors, because the energy coming from the outer dielectric would have no time to penetrate the mass of the conductor before the reversal of the electromotive force. He soon found that the corresponding violation of Ohm's law explained measurements performed by the electrical inventor David Hughes, and claimed priority for the discovery. In reality, the skin effect does not require the Poynting flux nor even Maxwell's theory, and it had been anticipated by several other authors, including Rayleigh, Larmor, and Lamb.[58]

5.5.4 The rough sketch

Heaviside's prediction of the skin effect was part of a major reformulation of Maxwell's theory, modestly entitled 'Rough sketch.' Heaviside started with Ohm's law ($\mathbf{j} = \sigma\mathbf{E}$), Maxwell's electric displacement ($\mathbf{D} = \varepsilon\mathbf{E}$), and magnetic induction ($\mathbf{B} = \mu\mathbf{H}$), from which he built the expressions of the energies dissipated and stored in the volume element $d\tau$ of the medium: $\sigma E^2 d\tau$ and $\frac{1}{2}\mathbf{E}\cdot\mathbf{D}d\tau + \frac{1}{2}\mathbf{E}\cdot\mathbf{H}d\tau$, respectively. He disregarded Maxwell's pictures of charge and current, but retrieved the basic distinction between force and flux, as conjugate factors in energy densities. Then he

[57] Heaviside 1882–1883: 195; *ibid.*: 211–12, and Maxwell 1856b: 206 for Stokes's theorem (in the *Treatise* Maxwell used partial integration, probably for the sake of rigor); Heaviside 1882–1883: 205 for **A**, **B**, **C**, **D**; *ibid.*: 200–1, 224–8, and Maxwell 1878: 140 for combining symmetry arguments and the Ampère theorem. Cf. Yavetz 1995: 66–112. On Heaviside's vector notation, cf. Crowe 1967; Hunt 1991a: 105–7; Yavetz 1995: 85–7.

[58] Heaviside 1883–1884; 1884–1885: 378; 1885–1887: 440–1. On the energy flux, cf. Hunt 1991a: 120–1; On the skin effect, cf. Jordan 1982b; Yavetz 1995: 191–208; and also Chapter 6, pp. 221, 226, for Helmholtz's similar effect.

turned to the activities (rates of producing work) of the forces **E** and **H**. These are obtained by multiplication with the corresponding current. In the electric case, Heaviside simply adopted Maxwell's expression of the current ($\mathbf{J} = \mathbf{j} + \partial\mathbf{D}/\partial t$), which yields:

$$\mathbf{E}\cdot\mathbf{J} = \sigma E^2 + \frac{\partial}{\partial t}\left(\frac{1}{2}\mathbf{E}\cdot\mathbf{D}\right), \tag{5.4}$$

The electric actitivity is therefore equal to the Joule heat plus the electric energy stored in the medium. Heaviside wanted a similar result in the magnetic case. He therefore defined the 'magnetic current' $\mathbf{G} = g\mathbf{H} + \partial\mathbf{B}/\partial t$, the magnetic conductivity g being there only for more symmetry. Then the magnetic activity is

$$\mathbf{H}\cdot\mathbf{G} = gH^2 + \frac{\partial}{\partial t}\left(\frac{1}{2}\mathbf{B}\cdot\mathbf{H}\right), \tag{5.5}$$

in perfect analogy with the electric case.[59]

For the cross-connections between electric and magnetic force, Heaviside wrote Maxwell's form of the Ampère law, and a similar relation between electric force and magnetic current:

$$\nabla\times\mathbf{H} = \mathbf{J},$$
$$-\nabla\times\mathbf{E} = \mathbf{G}. \tag{5.6}$$

This is the modern form of Maxwell's equations, if we forget the magnetic conduction current. Heaviside was proud to have 'murdered' the potentials, which he held responsible for various physical misconceptions. Of course, he knew Maxwell's interpretation of **A** as the electromagnetic momentum. But he accepted the notion only for complete circuits, and rejected its Lagrangian justification. In his opinion the principle of least action was 'the golden or brazen idol' of the Cambridge over-educated, and it interfered with the better physical insight brought by the principle of activity.[60]

For 'dynamical completeness,' Heaviside further introduced the electromotive sources of chemical and thermoelectrical origin, and also permanent magnetism. He did this by means of the impressed forces **e** and **h**, whose activities $\mathbf{e}\cdot\mathbf{J}$ and $\mathbf{h}\cdot\mathbf{G}$ measure the energy brought to the electromagnetic system in a unit of time. Heaviside regarded this definition as an obvious generalization of Thomson's corresponding definition for linear circuits, and noted that it

59 Heaviside 1885–1887: 429–34, 441. Cf. Yavetz 1995: 142–62.

60 Heaviside 1885–1887: 447–448; 1889b: 468 ('Thus Ψ and **A** are murdered'), 483–5 ('On the metaphysical nature of the potentials'); Heaviside 1893–1912, Vol. 3: 175 ('golden or brazen idol'). Cf. Buchwald 1985c; Yavetz 1995: 268–9. The two circuital equations or laws had already played a central role in Maxwell 1861, 1862 and in Maxwell 1868.

had long been well recognized by most writers on electrical subjects, especially since the practical introduction of dynamos, machines, accumulators, etc., which raise the energy transformations concerned in electrical phenomena from being matters of almost purely scientific interest to matters of the extremest commercial importance.

The impressed forces add to the forces determined by the electric–magnetic coupling, so that the full 'duplex equations' read:[61]

$$\nabla\times(\mathbf{H}-\mathbf{h})=\mathbf{J},\qquad(5.7)$$
$$\nabla\times(\mathbf{e}-\mathbf{E})=\mathbf{G}.$$

Using these equations, the activity of the impressed forces can be re-expressed as

$$\mathbf{e}\cdot\mathbf{J}+\mathbf{h}\cdot\mathbf{G}=\mathbf{E}\cdot\mathbf{J}+\mathbf{H}\cdot\mathbf{G}+\nabla\cdot[(\mathbf{E}-\mathbf{e})\times(\mathbf{H}-\mathbf{h})].\qquad(5.8)$$

According to eqns. (5.4) and (5.5), the two first terms correspond to the Joule heat and the energy stored in the field. The vector $(\mathbf{E}-\mathbf{e})\times(\mathbf{H}-\mathbf{h})$ must therefore represent the energy flux. With this generalization of Poynting's theorem Heaviside met his own criterion of intelligibility: 'It is a necessity of a rationally intelligible scheme (even if it be only on paper) that the transfer of energy should be explicitly definable.'[62]

5.5.5 *Moving bodies*

The original form of the duplex equations did not include bodies in motion, for Heaviside was mostly interested in problems of propagation along conducting lines. However, he was well aware of the necessity of terms depending on the velocity of matter. He even was the first Maxwellian physicist to give an exhaustive list of these terms. In 1885 he noted the $\mathbf{v}\times\mathbf{B}$ contribution to the electric force $\mathbf{E}$, as a consequence of Faraday's law applied to moving circuits. Similarly, he introduced a $\mathbf{D}\times\mathbf{v}$ contribution to the magnetic force $\mathbf{H}$: if a displacement current could magnetize a body at rest, then the motion of the body with respect to a constant field of displacement should also have a magnetizing effect. Heaviside then examined the activity of these 'motional forces.' For the electric one, the activity $\mathbf{J}\cdot(\mathbf{v}\times\mathbf{B})$ exactly balances the work $\mathbf{v}\cdot(\mathbf{J}\times\mathbf{B})$ of the electrodynamic force $\mathbf{J}\times\mathbf{B}$. In order to obtain a similar balance in the magnetic case, Heaviside introduced a new 'magnetoelectric force' $\mathbf{D}\times\mathbf{G}$ (with $\mathbf{G}=\partial\mathbf{B}/\partial t$) which was sufficiently small to have eluded observation.[63]

[61] Heaviside 1885–1887: 449, 451. Cf. Yavetz 1995: 154–62. Maxwell had introduced impressed forces only at the level of linear circuits. In Hertz's later formulation of Maxwell's theory, the impressed forces were included in Ohm's law, not in the circuital equations.

[62] Heaviside 1885–1887: 450; 1886–1887: 172.

[63] Heaviside 1885–1887: 448, 446 (with a numerical estimate of the magnetic motional force), 545–6; 1886–1887: 175.

Heaviside also included convection currents in his scheme. Both Faraday and Maxwell admitted the magnetic action of such currents, and Rowland proved it experimentally in 1875 by testing the action of a rapidly rotating charged disk on a compass needle. There was, however, no mathematical treatment of the effects of charge convection in Maxwell's theory, until in 1881 the young J. J. Thomson published an inspired but flawed paper on this subject (see Appendix 10). J. J. Thomson's motivation was to determine the electromagnetic behavior of the charged particles which constituted cathode rays according to Crookes. Adopting Maxwell's displacement, he reasoned that charge convection implied a varying displacement and a corresponding magnetic field. As long as the particle's motion is slow enough, the only change in the electric field is a uniform translation of its lines of force. The intensity of the corresponding magnetic field is proportional to the velocity of the charge, and therefore its energy is proportional to the square of the velocity, which means an increase of the effective mass of the charged particle. In an external magnetic field, there is an interaction energy proportional to the velocity and to the external field, and a corresponding deflecting force (our Lorentz force). These were essential results, to which later electrodynamicists frequently referred.[64]

Unfortunately, the relevant calculations suffered from the slavish following of Maxwell that Heaviside condemned. J. J. Thomson uncritically maintained Maxwell's expression of the displacement current, and sneaked his way around the resulting contradictions. Luckily, he reached the correct form of the final formulas; but the numerical coefficients were wrong. FitzGerald soon showed the necessity of a new contribution $\rho\mathbf{v}$ to Maxwell's dielectric current, where ρ is the charge density and $\mathbf{v}$ the velocity of the electrified matter. It was left to Heaviside to give, in 1885 and 1889, the correct expression of the Lorentz force ($q\mathbf{v}\times\mathbf{B}$), and the correct electromagnetic mass formula for a uniformly charged spherical shell ($q^2/6\pi a$ in rationalized electromagnetic units, with q for the charge, and a for the radius). Heaviside gave the clearest justification of the $\rho\mathbf{v}$ term: it meets Maxwell's requirement that all currents should be closed. Indeed, the divergence of the total current,

$$\nabla\cdot\left(\frac{\partial\mathbf{D}}{\partial t}+\rho\mathbf{v}\right)=\frac{\partial\rho}{\partial t}+\nabla\cdot(\rho\mathbf{v}), \tag{5.9}$$

is identically zero, since charge is conserved during its convection (see Appendix 10).[65]

In their most complete form involving all cases of motion, Heaviside's duplex equations are

[64] Faraday, *FER* 1: #1644, #1654; Maxwell 1873a: ##769–70; Helmholtz 1876 (report on Rowland); Rowland 1878; J. J. Thomson 1881a. Cf. Buchwald 1985a: 74–77 (on Rowland); 269–76 (on theories of convection); Darrigol 1993a: 287–8 (on Maxwell), 303–6 (on J. J. Thomson).

[65] J. J. Thomson 1881a; FitzGerald 1881; Heaviside 1885–1887: 446; 1889. Cf. Buchwald 1985a: 272–3.

$$\nabla \times (\mathbf{H} - \mathbf{D} \times \mathbf{v} - \mathbf{h}) = \mathbf{j} + \frac{\partial \mathbf{D}}{\partial t} + \rho \mathbf{v},$$
$$\nabla \times (\mathbf{E} - \mathbf{v} \times \mathbf{B} - \mathbf{e}) = -\frac{\partial \mathbf{B}}{\partial t}. \qquad (5.10)$$

These equations only determine the electric and magnetic forces. In order to determine the mechanical forces, Heaviside used the principle of activity. The result is in general ambiguous, because there are many different manners to write the energy balance. We will see later how Heaviside found in Hertz a solution to this difficulty.[66]

5.5.6 *The crop*

With his usual computational power and his 'redressed' Maxwell, Heaviside solved numerous problems of wave propagation that later proved very useful, and answered fundamental questions raised by other theorists of electricity. One of his most impressive achievements was the general solution he gave in 1888 to J. J. Thomson's problem of a point charge in uniform rectilinear motion. He found that the electric field was still radial, but compressed toward the meridian plane, to an extent determined by $(1 - v^2/c^2)^{-1/2}$ (see Appendix 10). When the velocity of the particle increases from zero to c, the field evolves from an electrostatic field to an electromagnetic plane wave confined in the meridian plane. Heaviside used this result to show the '*physical* inanity' of the electrostatic potential and reject a suggestion by William Thomson for measuring its propagation.[67]

Originally, no one paid much attention to Heaviside's difficult and lengthy series, except for the insulted telegraph authorities, who managed to have them suspended. In 1887, however, Heaviside convinced William Thomson of the pertinence of his theory of distortionless transmission. The following year, he had the pleasure to read Lodge remarking on 'what a singular insight into the intricacies of the subject, and what a masterly grasp of a most difficult theory, are to be found among the eccentric, and in some respects repellent, writings of Mr. Oliver Heaviside.' Heaviside soon joined the epistolary circle of Lodge, FitzGerald, and Hertz, and convinced them of the superiority of his rendering of Maxwell. FitzGerald was most eloquent in his praise:[68]

> Maxwell, like every pioneer who does not live to explore the country he opened out, had not had time to investigate the most direct means of access to the country, or the most systema-

[66] Heaviside 1886–1887: 174–5 (without the convection current); 1888–1889: 497 (with the convection current).

[67] Heaviside 1888–1889: 490–9; 1889c: 510–11. Cf. Hunt 1991a: 186–7; Darrigol 1993b: 313, 316–318. On Thomson's suggestion, see *supra*, p. 178, note 5.

[68] Lodge 1888a: 236; FitzGerald 1893: 299. On Heaviside's publication difficulties and their relation with his quarrel with William Preece, cf. Hunt 1991a: 137–43; Yavetz 1995: 242–56. On his recognition, cf. Hunt 1991a: 143–51; Yavetz 1995: 259–63 (on W. Thomson's support). Lodge's interest in Heaviside's papers was related to his recent experiments on lightning: see below, p. 204.

tic way of exploring it. This has been reserved for Oliver Heaviside to do. Maxwell's treatise is cumbered with the *débris* of his brilliant lines of assault, of his entrenched camps, of his battles. Oliver Heaviside has cleared those away, has opened up a direct route, has made a broad road, and has explored a considerable tract of country.

5.6 Electromagnetic waves

Great though they were, the British Maxwellians missed the discovery of electromagnetic waves, which is now regarded as the most definitive proof of Maxwell's system. Maxwell himself was remarkably silent on the production of electromagnetic waves. He discussed the characteristic spectrum of a substance in terms of a 'disturbance of the luminiferous medium communicated to it by the vibrating molecules,' with no mention of anything electromagnetic. Plausibly, he did not believe that purely electromagnetic processes could generate waves. His doctrine of closed currents indeed obscured the propagation of interactions. By putting conduction and displacement currents on the same footing, he confused sources and their effects. Considered as a function of the total current, his vector potential did not propagate.[69]

5.6.1 The question

Oliver Lodge, the first man to anticipate the electric production of electromagnetic waves, did not reason in terms of the misleading form of Maxwell's equations. His inspiration came from a primitive version of the cogwheel model, which he described at the 1879 meeting of the British Association. There he assumed the ether to be positive and negative electricity bound together (the two kinds of wheels), and interpreted light as a periodic displacement of the two electricities, with an electrostatic restoring force. The view was opposite to Maxwell's, for it made displacement depend on electric forces, and not vice versa. Yet it suggested that light could be excited electrically. Lodge imagined several experimental devices—none of which would have worked, as we can now judge. His best guess was to use the oscillatory discharge of a condenser, although the frequency of light waves could never have been reached in this manner.[70]

Lodge soon abandoned his project, because FitzGerald convinced him that Maxwell's theory forbade the electric production of electromagnetic waves. In a first paper FitzGerald gave two different impossibility proofs, one based on the non-propagation of Maxwell's vector potential as a function of the total current, the other

[69] Maxwell 1875. Chalmers 1873b; Hunt 1991a: 28–30. However, Maxwell referred to Faraday's 'Thoughts on ray vibrations' (Faraday 1846: 450), according to which the sudden motion of an electrified or magnetized body would produce transverse vibrations of the emerging lines of force (Maxwell 1864: 194). On early, uninterpreted observations of electromagnetic radiation, cf. Süsskind 1964.

[70] This is based on Hunt's reconstruction from the following unpublished documents: Lodge to FitzGerald, 26 and 29 February 1880, quoted in Hunt 1991a: 31–2; Lodge to Larmor, 1 January 1902, which contains extracts from Lodge's notebooks for 1879–80. Cf. Hunt 1991a: 30–33.

on the conservative character of a system of closed currents. In a second paper, he confirmed the lack of propagation with a standing-wave solution of the wave equation for the vector potential. Three years later, he found a similar problem treated in Rayleigh's *Theory of Sound*, with progressive solutions that expressed the emission of waves! FitzGerald had to apologize 'for having ventured to investigate these matters when [he] was so ignorant of what had already been done as to make mistakes requiring such serious corrections as are contained in this paper.' He admitted to having erred in his impossibility proofs by including the displacement current in the sources of the field.[71]

To complete the volte-face, FitzGerald suggested that electromagnetic waves could be produced in measurable amount by discharging a condenser through a circuit of small resistance. The following year, he published the retarded vector potential formula, with a discouragingly small estimate of the energy radiated by an oscillating current loop. In his notebooks he calculated the oscillation frequency of simple circuits, and discussed various ways of detecting the emitted waves. But he did not persevere. As Heaviside regretted, 'he saw too many openings. His brain was too fertile and inventive.' Worse, no one followed up the idea, not even his friend Lodge, who had little time for research in this period of his life. Had there been sustained efforts to produce and detect the waves, they would not necessarily have met success. None of the detecting procedures imagined by FitzGerald would have worked; and that later used by Hertz was based on an unexpected property of the electric spark, as we will later see.[72]

5.6.2 Waves on wires

In early 1888 Lodge, whose skill as a scientific speaker was well known, was asked to lecture on lightning protection. In order to simulate thunderbolts, he discharged Leyden jars through a spark gap. For the sake of visibility, he fed the Leyden jars continuously with a powerful electrostatic machine. His arrangement is represented in Fig. 5.8 (without the dotted line L for the moment). The jars stand on the same, low-conducting, wooden table. At the beginning of a cycle, the Voss machine (top) charges the two Leyden jars slowly until the breaking tension of the gap A is reached. The resulting spark short-circuits the gap A, so that a potential difference appears at the gap B. As long as the latter gap is not too wide, the jars discharge through it. All sparking ceases at the end of this process, and a new cycle can begin.[73]

[71] FitzGerald 1879b; 1880; 1882: 101. Cf. Hunt 1991a: 33–2.

[72] FitzGerald 1882: 100; 1883a; Heaviside, in FitzGerald 1902: xxvi. Cf. Hunt 1991a: 46–7. Retarded potentials had already appeared in Lorenz 1867, but in a different context: cf. Chapter 6, pp. 212–13. FitzGerald's imagined oscillator was of the dipolar magnetic kind, which is very inferior to Hertz's electric dipolar oscillator. In 1884 J. J. Thomson discussed the emission of electromagnetic waves by a perfectly conducting spherical shell returning to electric equilibrium (J. J. Thomson 1884c). He judged these waves to be undetectable because they were emitted too suddenly (in a few periods only, with a wavelength comparable to the radius of the sphere). This may explain why experiments of this kind were not attempted at the Cavendish Laboratory.

[73] Lodge 1888a: 234.

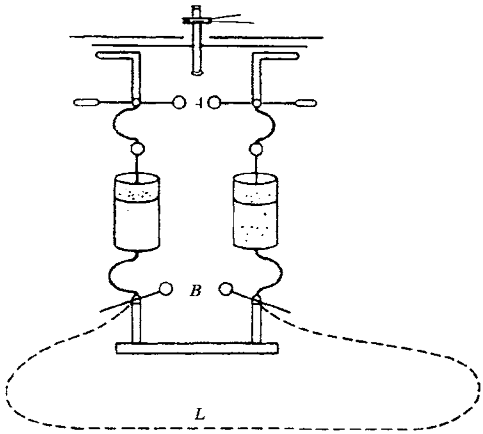

FIG. 5.8. Lodge's experiment of the alternative path (Lodge 1888a: 234).

With this lightning simulator, Lodge proceeded to compare different lightning conductors. He introduced conductors L (see Fig. 5.8) of various shape and constitution, and determined the minimal size of the gap B for which the jars preferred to discharge through the alternative path L. For an audience accustomed to reason exclusively in terms of ohmic resistance, the results were quite counter-intuitive. Even when the resistance of L was a small fraction of an ohm and the gap B was as wide as A, the discharge preferred the gap. Lodge explained this fact by the self-induction of the wire L, which obstructed quickly varying currents. He was more surprised to find out that an iron wire led the discharge better than a similar copper wire. His tentative explanation was twofold. First, the high magnetic permeability of iron, which should have enhanced the self-induction, did not exist for fast-varying currents. Second, iron was better than copper because Heaviside's skin effect, which increases the resistance by confining the current to the surface of the conductor, was more important for the better conductor, copper. Lodge then provided a striking proof of the skin effect by showing that flat conductors conducted the discharge better than round ones.[74]

Lodge knew well that the discharge of a Leyden jar through a small resistance was oscillatory, and he expected the same to be true for the discharge of clouds through lightning. His conclusions largely depended on the high frequency of the oscillations, which enhanced the effect of self-induction. At the Bath meeting of 1888, William Preece, the *bête noire* of the Maxwellians, maintained against Lodge the received wisdom of lightning protection. For this once Preece was right: later studies proved the non-oscillatory character of lightning, and thus ruined a good deal of Lodge's conclusions.[75]

[74] Lodge 1888a: 235–6.
[75] Cf. Hunt 1991a: 146–51; Yavetz 1996 (for the later evolution of the subject).

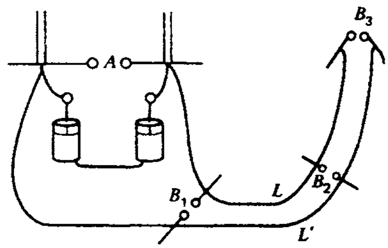

FIG. 5.9. Lodge's 'recoil-kick' experiment (Lodge 1888a: 275).

Aside from the lightning problem, Lodge's focus on oscillatory discharge bore interesting fruit. In a variant of the alternative-path experiment, represented in Fig. 5.9, he had the jars concurrently discharge through the gap A and through a discharger B bridging two long wires attached to the poles of the machine. He expected the sparking at B to cease whenever the gap B was larger than the gap A. Experiment decided differently. Moreover, this sparking proved stronger when the discharger B was farther from the source. Lodge suspected a resonance phenomenon. The high sparking at B, he propounded, corresponded to the 'recoil kick' of reflected waves, and increased when the length of the leads approached the half-wave length. In further experiments showing nodes and anti-nodes on longer wires, Lodge estimated the waves to be 30 yard long. This number agreed with his own estimate of the frequency of the oscillator from its capacity and self-induction.[76]

For continental physicists, Lodge's experiments had little theoretical significance, for they could be interpreted in terms of Kirchhoff's waves of electricity in wires. In contrast, from a Maxwellian point of view Lodge had done no less than produce electromagnetic waves by electrical means. He published his beautiful results in the early summer of 1888, and went on vacation to the Tyrol. During his train ride to the Alps, he read the latest issue of Wiedemann's *Annalen*, and found that a young German physicist, Heinrich Hertz, had obtained 'much better and more striking evidence of these electromagnetic waves.' Lodge swallowed his bitterness, and soon proclaimed his joy over this splendid development.[77]

5.7 Conclusions

Maxwell left his electromagnetic theory in a state full of imperfections and obscurities. The superiority of his views was not self-evident to his contemporaries. The highest British authority on electricity, Sir William Thomson, disliked Maxwell's theory for it ventured far from empirical facts without offering a mechanical repre-

[76] Lodge 1888a: 275; Lodge 1888b, 1888c. Cf. Aitken 1985: 89–95; Hunt 1991a: 148–9. This version of the alternative path was the most obvious to realize, because the commercial Voss machine came with the two Leyden jars already attached to it (for storing the electricity).

[77] Lodge 1888c: appendix written in Tyrol, dated 24 July 1888. Cf. Hunt 1991a: 153.

sentation of basic field processes. According to Thomson, Faraday's concept of charge and Maxwell's displacement current were unwarranted extensions of a partial analogy between vacuum and dielectrics; and there were other ways to deal with open currents, without leaving the conceptual framework of transmission lines. Between the practical concerns of the telegraph and the ideal of a simple elastic solid ether, Thomson tolerated no *via media*. Consequently, he condemned Maxwell's new style of theoretical physics.

Through his indispensable treatise on electricity and magnetism, Maxwell nevertheless managed to transmit his and Faraday's views to a few British physicists. Some of these assiduously perfected and extended his system until it won, in the late 1880s, the preference of most English-speaking electricians. In this process Maxwell's theory was significantly transformed, and acquired several features that are now judged central, for example the four-equation formulation, the Poynting flux, and the electric production of electromagnetic waves.

There were several kinds of Maxwellian works. In the most conservative, Maxwell's equations were blindly applied to computable versions of old problems, for example Arago's disk or rotating conducting spheres. This involved solving systems of differential equations with simple boundary conditions, and therefore incited much Cambridge Tripos activity. To spare the technical details, the ensuing publications have not been discussed in this chapter. However, they contributed to establish a Maxwellian paradigm, and they helped clarify some issues. For example, in 1887 Horace Lamb explained that Maxwell's scalar potential was completely determined by Maxwell's equations alone and generally differed from the electrostatic potential given by Poisson's equation. Maxwell, Larmor, and J. J. Thomson had previously missed this important point.[78]

The most important clarifications of Maxwell's system were obtained either by mechanical pictures or by dynamical, energetic considerations. With his cord-and-beads model, Lodge illustrated Maxwell's concept of charge and current, and various processes in simple electric systems. With wheels and rubber bands, FitzGerald showed that Maxwell's displacement did not have to be the linear displacement of some substance. More likely, this quantity corresponded to local strains of a different kind. Then the electric current no longer resembled the flow of an incompressible fluid. A displacement current meant a variation in the strain of the mechanism transfering rotational motion in the field; a conduction current implied a slipping of this mechanism.

As Lodge and FitzGerald acknowleged, their models were good only to illustrate some aspects of Maxwell's theory. They were, for instance, unable to explain electrostatic attractions. Yet the two friends' ambition was to determine the ultimate constitution of the ether. Somewhat naïvely, Lodge believed that a system of cogwheels, or something similar, was a useful step in this direction. More philosophically, FitzGerald hoped to reduce electromagnetism and optics to the motions of an ideal

[78] Lamb 1887; Maxwell 1873a: #783; Larmor 1884a; J. J. Thomson 1884a. Cf. Darrigol 1993b: 294–7.

fluid. His mathematical power and his rare physical intuition did not suffice, however, to bring the project to fruition.

Not every British physicist shared Lodge's and FitzGerald's trust in mechanical models and pictures. Poynting and Heaviside instead relied on dynamical concepts that had more direct empirical significance. They both determined the energy flux in the electromagnetic field as a necessary consequence of Maxwell's equations and field-energy distribution. They described conductors as sinks and guides for the energy traveling in the surrounding dielectric. For Poynting, the primitive dynamical notion was the motion of tubes of force, which provided an intuitive understanding of basic field processes despite the lack of a mechanical foundation. The role of a conductor, for example, was to guide and partially dissolve the tubes of force moving in the surrounding dielectric.

For Heaviside, the basic dynamical notions were generalized force and velocity, controlled by the 'principle of activity' borrowed from Thomson and Tait. Heaviside required that the activity of the forces **E** and **H** at a given point—that is, their product by the corresponding current—should determine the energy stored and dissipated at this point. He directly expressed the field equations in terms of forces and currents, and completed them to include all cases of bodily motion. Maxwell's potentials were gone, 'as a hip of metaphysics.' In Heaviside's eyes the loss of Maxwell's Lagrangian foundation was largely compensated by a better insight into practical problems. A former telegrapher and a proudly independent thinker, he developed his own efficient methods to study signal propagation. He invented the vector notation, our impedance, inductance etc., and a version of the operational calculus. In his hands Maxwell's theory became more transparent, more complete, and more ready-to-use.

Other Maxwellians pursued the relation between light and magnetism. Old and new effects of that kind offered an opportunity to explore vortical rotation in the magnetic field and eventually to confirm Maxwell's electromagnetic theory of light. FitzGerald started the trend with a remarkable theory of Faraday's and Kerr's magneto-optical effects. His work could be read in a variety of instructive ways: as an indication that MacCullagh's strange rotational medium was the only plausible concept of a mechanical ether, as a proof that the reflection and refraction of light could be treated in a purely electromagnetic manner, and as a general strategy for modifying Maxwell's equations to integrate new, non-linear effects. In the third register, FitzGerald's prescription was to add new terms to the electromagnetic field Lagrangian, so that dynamical principles would be automatically satisfied. The method had powerful adepts in Cambridge, where the abstract dynamics of Thomson, Tait, and Maxwell had a growing influence. It culminated with Glazebrook's noting, in 1881, that FitzGerald magneto-optical Lagrangian term could be justified in a purely electromagnetic manner in relation to the Hall effect. For Maxwellian physicists, this remark meant a major confirmation of Maxwell's theory of light.

Glazebrook's theory did not survive further magneto-optical research in the 1890s. The essentially macroscopic approach with field Lagrangians and effective field equations proved insufficient, and had to be replaced with microphysical considerations. This is not to say that Maxwellian theorists of the 1880s always avoided atomistics. On the contrary, Oliver Lodge and J. J. Thomson devoted much time to the atomistic periphery of Maxwell's system. This Maxwellian microphysics, and its evolution into a different kind of microphysics will be treated in Chapter 7.

By the late 1880s, FitzGerald, Poynting, Lodge, Heaviside, and other British Maxwellians had convinced a large number of English-speaking physicists and electricians that electric fluids and direct action at a distance should be replaced with more philosophical and truly dynamical field notions. The coincidence between the electrostatic/electromagnetic charge units ratio and the velocity of light and the success of derived magneto-optical theories increased the plausibility of Maxwell's theory. Yet direct electromagnetic proofs of its superiority were lacking. Perhaps the Maxwellians were too convinced of the truth of Maxwell's system to pursue such crucial experimenting. The best Lodge did was to show electric waves on wires, which Kirchhoff's theory predicted just as well as Maxwell's.

6

Open currents

6.1 Introduction

Neumann's and Weber's systems dominated continental electrodynamics until the late 1880s. Faraday's experimental researches did not have the intended destabilizing effect, and Maxwell's field theory was mostly ignored.[1] Dielectrics were understood in Mossotti's terms of electricity displaced by distance forces. Diamagnetism was reduced to Amperean currents interacting according to Weber's forces, or to ordinary magnetism according to Becquerel's view. Lines of force were rarely used and only as a descriptive tool, leaving the deeper mathematics to Neumann, Weber, and Kirchhoff. Even the Faraday effect was subsumed under Weberian concepts: in 1858 Franz Neumann's son Carl derived the main properties of the magneto-optical rotation from the interaction of the optical ether particles with the Amperean currents according to a generalized Weber force.[2] In sum, every known experimental fact seemed compatible with the received views.

The drawback of this breadth of German electrodynamics was a certain stagnation of its concepts and methods. The first section of this chapter shows how mild and short-lived were the few German attempts at reforming the foundations of electrodynamics. The second deals with an outstanding exception: Helmholtz's general framework for investigating open currents. With his electrodynamics, Helmholtz wanted to fight the decline he perceived in German physics, to exemplify new methods, and to stimulate new researches. He succeeded quite well, as we will see in the two last sections devoted to Hertz's major discovery and its impact.

Two peculiarities of this chapter are worth noting. A full discussion is given of Helmholtz's studies of what we now call the *RL* and *RLC* circuits. Helmholtz scholars have previously ignored these works, presumably because they seem trivial from a modern perspective. However, they were crucial in bridging Helmholtz's

[1] There were exceptions: Stefan 1874; G. Wiedemann 1882–1885, Vol. 4: 1158–88, 1203; Tumlirz 1883; Mascart and Joubert 1882–1886: Ch. 6; and some French telegraphers, as is documented in Atten 1988a. The most important case, Helmholtz's, is treated in this chapter. More on the French reception is in Coelho Abrantès 1985, and Atten 1988a, 1992.

[2] C. Neumann 1858, 1863. Cf. Knudsen 1976: 262–71.

physiological and physical researches; they established the validity of Ohm's law for transitory currents; and they provided basic techniques to study rapid electric processes. Another singularity of this chapter is the step-by-step account of Hertz's experiments on fast electric oscillations. This is of course justified by the extraordinary character of Hertz's findings, but also by newer insights into the bits of apparatus and types of reasoning that were available to him.

6.2 Continental foundations

6.2.1 *Amperean axioms*

The conceptual basis of continental electrodynamics remained the direct action between two current elements or two particles of electricity. When continental theorists discussed the foundations of electrodynamics, they were usually concerned with the consolidation of Ampère's and Weber's laws, or with the production of alternative laws of the same kind. In France, Ampère's high stature inspired numerous comments on his cases of equilibrium and improvements of his deductions. In other countries, theorists felt free to propose alternative expressions for the forces between current elements as long as the actions between closed currents remained the same. In 1845 Hermann Grassmann had dropped the equality of action and reaction and reached a formula that was simpler from the point of view of his *Ausdehnungslehre*. In 1869 Josef Stefan admitted torques between the elements. Diederik Korteweg and Hendrik Lorentz later explored this possibility more systematically.[3]

The most prolific German writer on theoretical electrodynamics, Carl Neumann, axiomatized the various continental theories, criticized their foundations, and discussed their compatibility with the energy principle in a very systematic manner. In the early 1870s he derived Ampère's law and a range of possibilities for the induction law from the energy principle, the existence of a potential for closed currents, and a few assumptions on the form of the interaction between current elements. He also argued for the compatibility of Weber's law with the energy principle, and protected it against Helmholtz's attacks, as will be seen in a moment. His long and drily mathematical memoirs seem to have had little readership. They embarrassed the encyclopedist of electricity, Gustav Wiedemann, who explained in a footnote: 'A further account of Carl Neumann's very exhaustive memoirs [. . .] is here impossible; extracts from these are not easily given, and I must therefore refer the reader to the original text.'[4]

[3] Grassmann 1845; Stefan 1869; Korteweg 1880; Lorentz 1882. On the French Ampère-mania, cf. Atten 1992. On the alternatives to Ampère's formula, cf. J. J. Thomson 1885: 100–6; Kaiser 1981: 33–4, 56–60.

[4] C. Neumann 1871a, 1873a, 1873c; Wiedemann 1885, Vol. 4: 1104n. Carl Neumann's most durable works were in mathematics.

6.2.2 Retardation

In most of his works Carl Neumann regarded the Ampère forces or the Weber forces as primitive, and did not seek a deeper foundation. So did other continental theorists. There were, however, a few interesting exceptions. The great mathematician Bernhard Riemann had learned physics under Weber, and shared Gauss's interest in a mechanical foundation of electrodynamics. In his Göttingen lectures of 1861, he gave the following simple Lagrangian for Weber's forces between two particles of electricity e and e':

$$L = \frac{ee'}{r}\left(1 + \frac{\dot{r}^2}{C^2}\right). \tag{6.1}$$

He also proposed the variant

$$L = \frac{ee'}{r}\left[1 + \frac{(\mathbf{v} - \mathbf{v}')^2}{C^2}\right], \tag{6.2}$$

in which $\mathbf{v} - \mathbf{v}'$ is the relative velocity of the two particles. The resulting forces, together with Fechner's dualistic view of the electric current, lead to the same laws for the forces between closed currents, but with easier analytical manipulations. In these lectures Riemann pioneered a more abstract kind of mechanical reduction, in which a Lagrangian was given without the mechanism being known.[5]

At the same time Riemann sought a '*constuirbare Vorstellung*,' Gauss's word for a more definite mechanical representation of electrodynamics. In 1853 he had privately speculated on a model for a universal ether. In a manuscript of 1858 he tried to derive the force between two moving particles of electricity from the assumption that the electrostatic potential propagated at the velocity of light. He had in mind a unification of optics and electricity, and hoped that optical and electrodynamic actions occurred through the same medium. However, he withdrew his manuscript before publication, presumably because he became aware of a fatal mathematical error that Clausius had the pleasure of detecting in the posthumous publication of 1867.[6]

Carl Neumann still found the idea interesting, and developed a superficially similar one in 1868. He took the potential between two particles of electricity at time t to be inversely proportional to their distance at the time $t - r/C$, where r is their distance at time t and C $(= c\sqrt{2})$ is twice the ratio between the electrodynamic and the electrostatic unit of charge. This strange assumption had the merit of yielding Weber's law to a first approximation. Clausius promptly noted that it did not mean

[5] Riemann 1875 [1861]: 318–25. Cf. J. J. Thomson 1885: 111–14 (who, however, wrongly gives Clausius the credit for the Lagrangian form); Whittaker 1951: 206; Kaiser 1981: 113–14.

[6] Riemann [1853]; 1867 [1858]; Clausius 1868. Cf. Whittaker 1951: 240–1; Kaiser 1981b: 148–57; Wise 1981b: 288–92 for the philosophical context.

a genuine propagation through a medium. Neumann then ackowledged that his assumption widely differed from Riemann's and had little analogy with light propagation. Not only were the velocities different, but the emitted potential depended on the motion of the receiving particle. Yet Neumann, who cared more for mathematical clarity than for physical plausibility, maintained his assumption. His German colleagues ignored it, and Maxwell ridiculed Carl Neumann's 'altogether unique' theory of the transmission of the potentials.[7]

The most brilliant attempt at introducing retardation in continental electrodynamics is found in a paper published in 1867 by Ludvig Lorenz. Unlike most German electrodynamicists, the Danish physicist doubted that electric current was a flow, and judged that physical hypotheses on the nature of electricity were premature. He divorced Kirchhoff's equations for the motion of electricity from their Weberian foundation, and admitted them as a mathematical expression of empirical laws. As he nonetheless shared Oersted's and Ampère's drive for unity, he tried to bring electricity and optics under the same theory. His strategy was to modify Kirchhoff's equations so that the motion of electricity would become analogous to that of the optical ether.[8]

Lorenz found that without perturbing the validity of Kirchhoff's equations for closed, slowly varying currents, he could replace the relevant potentials with

$$\begin{aligned}\phi_R(\mathbf{r},t) &= \int \frac{\rho(\mathbf{r}',t-|\mathbf{r}-\mathbf{r}'|/c)}{|\mathbf{r}-\mathbf{r}'|}\,\mathrm{d}\tau', \\ \mathbf{A}_R(\mathbf{r},t) &= \int \frac{\mathbf{j}(\mathbf{r}',t-|\mathbf{r}-\mathbf{r}'|/c)}{|\mathbf{r}-\mathbf{r}'|}\,\mathrm{d}\tau'.\end{aligned} \tag{6.3}$$

He meant these formulas to express that 'the whole action which emanates from the free electricity and the electric currents *takes time to propagate*, an assumption that is not foreign to science and should in itself [*an und für sich*] have a certain probability.'

From his previous work in optics he knew that the retarded potentials were the general (physically meaningful) solutions of the wave equations with the sources ρ and $\mathbf{j}$ and the propagation velocity c. Applying the wave operator to both sides of Ohm's law ($\mathbf{j} = \sigma(-\nabla\phi - \partial\mathbf{A}/c^2\partial t)$ and using the conservation of electricity he obtained the following equation for the electric current:

$$\nabla\times(\nabla\times\mathbf{j}) + \frac{1}{c^2}\frac{\partial^2\mathbf{j}}{\partial t^2} + \frac{4\pi\sigma}{c^2}\frac{\partial\mathbf{j}}{\partial t} = 0. \tag{6.4}$$

[7] C. Neumann 1868a, 1868b; Clausius 1868; C. Neumann 1869; Maxwell to Thomson, 1 October 1869, *MSLP* 2: 499. Cf. Maxwell 1873a: #863; Wiedemann 1885, Vol. 4: 1096–7; Kaiser 1981: 163–4 (with a discussion of another attempt by Enrico Betti); Wise 1981b: 294–5; Archibald 1986 for a detailed study.

[8] Lorenz 1867a, 1867b. Cf. Whittaker 1951: 267–70; Kaiser 1981: 157–62; Wise 1981b: 293–4; Kragh 1991.

This equation was identical with the one he had used for transverse ether displacements in his optics, up to the absorption term involving the conductivity σ. Lorenz therefore identified the optical ether with a bad conductor and the optical vibrations with alternating electric currents.

As Maxwell noted in his *Treatise*, Lorenz's equation (6.4) for the motion of electricity is the same as that given by Maxwell's theory. In fact, Lorenz referred to Faraday's opinion that electric actions should be 'contiguous.' But he was not aware of Maxwell's electromagnetic theory of light, published two years earlier, and his theory differed from Maxwell's on essential points. He related light propagation to conduction currents, whereas Maxwell's displacement currents occurred in perfect insulators. Consequently, Lorenz was not able to extend his theory beyond the case of a medium of uniform conductivity. His paper had little influence, although in 1884 Hertz referred to it as the first publication of the retarded potential in electrodynamics (with Riemann for the scalar potential).[9]

By filling vacuum with conduction currents, Lorenz moved toward a field theory of electrodynamics as far as continental physicists could. For nearly all of them, a formula for the action between two particles of electricity remained the basis of the theory, even if the possibility that the action occurred through a medium was more and more frequently evoked. Typical in this regard is the new theory of electrodynamics that Clausius developed in the late 1870s.

6.2.3 Clausius's conservative reform

With his usual critical acumen, Clausius remarked that Weber's law led to wrong predictions if the electric currents did not consist of Fechner's symmetrical flow of positive and negative electricity: a closed, constant current would act on a charged particle at rest, against all experimental evidence. Yet by that time Weber and Carl Neumann had used the unitary view of the electric current, for example in their description of Amperean currents. Clausius emphasized the greater simplicity of this view in metallic conductors, and also noted that in electrolytic currents the positive and negative electricities could not possibly travel at the same velocity. He therefore decided to replace Weber's law with another that would not give the unwanted force between charge and current.[10]

In order to do so in conformity with the electrodynamics of closed currents and the energy principle, Clausius had to give up three of Weber's basic assumptions: that the forces between two electric particles were on the line joining these particles, that these forces were equal and opposed, that they depended only on the relative motion of the particles. To justify this freedom, he proposed that the forces

[9] Maxwell 1873a: #805; Lorenz 1867a: 211–12; Hertz 1884: 314.

[10] Clausius 1877a, 1877b, 1879: 232. In reality, the Weber force between a constant current and a charge at rest is too small to be detectable: see Appendix 4. Riecke 1873 had already noted that Weber's theory implied this force. Lorberg 1878 proved that its absence required a symmetrical flow of electricity (but still preferred Weber's law). Riemann's potential has the same defect (Clausius 1877b: 18). Clausius was also aware of Helmholtz's criticism of Weber's law (Clausius 1875: 658), but did not rely on it.

depended on the motion of the particles with respect to a medium, and that the medium could absorb the missing momenta. His final formula for the force acting on a particle e due to a particle e' at a distance r had the simple vectorial form

$$\mathbf{f} = -ee'\nabla\left(\frac{1}{r}\right)\left(1-\frac{\mathbf{v}\cdot\mathbf{v}'}{c^2}\right)-\frac{ee'}{c^2}\frac{\partial}{\partial t}\left(\frac{\mathbf{v}'}{r}\right), \tag{6.5}$$

and derived from the Lagrangian $(1 - \mathbf{v}\cdot\mathbf{v}'/c^2)ee'/r$.[11]

Clausius developed the consequences of this formula with old-fashioned Amperean mathematics, abounding in multiple derivatives of r with respect to time and the curvilinear abscissae s and s'. Willing though he was to admit the electromagnetic ether, he avoided field mathematics, even in Kirchhoff's ontologically neutral form. With hindsight Clausius's formula is the best approximation to the true field action between two charged particles. Contemporary physicists judged differently. For the Germans, Clausius violated the spirit of the regnant theories. For the British, he was enslaved in the form of obsolete theories. His clever attempt at conservative reformation never took off the ground.[12]

To sum up, continental reflections on the foundations of electrodynamics had limited scope and impact. Most of them were conservative with respect to physical concepts and mathematical techniques. They often degenerated into sterile axiomatization. The few tries at taking a medium into account were isolated or short-lived, and they were unrelated to Maxwell's more powerful attempts in England. Discussions of foundations were completely theoretical, and no one sought to define the experimental conditions of a discrimination between different possibilities. There was, however, one essential exception to this general attitude: Hermann Helmholtz and his disciples.[13]

6.3 Helmholtz's physics of principles

Hermann Helmholtz's passion for physics developed at an early age, while he was still attending the Potsdam Gymnasium. In 1837 he accepted a government stipend to study medicine, with a commitment to serve eight years as a surgeon in the Prussian army. During the four-year training period, he attended courses in philosophy, physics, and physiology at Berlin University. He also conducted researches in

[11] Clausius 1875, 1876, 1877a, 1877b, 1879. The form given in Clausius 1875 maintains the equality of action and reaction, but violates the energy principle for non-Fechnerian currents, as noted in Clausius 1876. For the action between two current elements, Clausius's law implies Grassmann's formula (Clausius 1879: 285). Cf. J. J. Thomson 1885: 108–10; Wiedemann 1885, Vol. 4: 1106–09; Whittaker 1951: 234–235.

[12] Clausius 1877a, 1877b, 1879. For the relation with Lorentz's theory cf. Whittaker 1951: 234 (Clausius's Lagrangian is the first approximation of the interaction term in Schwarzschild's Lagrangian, given in Appendix 9). For German objections, cf. Wiedemann 1885, Vol. 4: 1107–09. For a typical British comment, cf. J. J. Thomson 1885: 110.

[13] A minor exception is Schatz 1880, a dissertation work done under Clausius to analyze the possibility of discriminating between the laws of Clausius, Riemann, and Weber.

Gustav Magnus's private physics laboratory, and read the French classics of mathematical physics with a group of friends. In 1841–1842 he completed his dissertation under the famous physiologist Johannes Müller. While performing his army duty, he did important work on muscular force and heat, which won him the Königsberg physiology chair in 1848. He occupied various positions in the same field until he obtained a physics chair in Berlin in 1871. His interest in physics was constant, even in his physiological works. Like Müller he focused on the physical and chemical processes in living organisms. Following Müller's most radical disciples, he struggled to eliminate the vital force from physiology. For example, his study of muscular contraction demonstrated that the consumption of chemicals accounted for the work and heat produced.[14]

6.3.1 The conservation of force

In works of this kind Helmholtz was led to reflect on the transformations occurring between different kinds of force. His first hints at energy conservation were published in this context, and the capital memoir of 1847 'On the conservation of force' followed soon. This work exploited diverse resources, including the mechanics of French engineers, Ampère's vibration theory of heat, Joule's conversion experiments, Carnot's reflections on the motive power of heat, Laplacian reduction to central forces, and transcendental philosophy.[15]

Helmholtz headed his memoir with a philosophical argument that seemed remote from physics, even to contemporary physicists. However, these considerations were an essential part of his reflections. They determined important aspects of his approach to electrodynamics. In addition, they tell us how direct action at a distance could be as philosophical in Germany as it was unphilosophical in England.[16]

Helmholtz first defined the 'comprehensibility of nature' as the possibility of finding the ultimate, *invariable* causes of natural processes. Then he introduced the two 'inseparable abstractions' of force and matter, matter being that which can only change by motion, and force the cause of motion. The comprehensibility of nature implies the reducibility of physics to forces that depend on the spatial configuration of matter only. Helmholtz further applied the *decomposition principle*, according to which 'the force which two whole masses exert on each other must be resolved into the forces which their parts exert on each other.' In a fully comprehensible world, the resulting elementary forces are 'central forces' acting between two mass points and tending to alter their distance at a rate depending only on the distance.[17]

[14] For Helmholtz's biography, cf. Königsberger 1902–1903; Turner 1972; Cahan 1993: xxi–xxix. On his early work in physiology, cf. Lenoir 1982: 197–215; Kremer 1990: 275–307; Olesko and Holmes 1993.

[15] Helmholtz 1847. On the genesis and the meaning of this memoir, cf. Bevilacqua 1993, and further literature quoted in there.

[16] Cf. Wise 1981b: 295–7; Heimann 1974a for a possible relation to Kant; Bevilacqua 1993: 304–9.

[17] Helmholtz 1847: 15. Cf. Heimann 1974a; Krüger 1994b; Darrigol 1994b. The name 'decomposition principle' is mine.

Being central, the elementary forces derive from a potential and satisfy the theorem of living forces and the impossibility of producing work in a cycle. Moreover, the sum of 'the total living force' (our kinetic energy) and 'the sum of tensional force' (our potential energy) is conserved. If nature is fully comprehensible, no perpetual motion is possible, and 'force' (energy) is conserved. For his less philosophical readers, Helmholtz offered an alternative route to energy conservation, based on the decomposition principle and the empirically known impossibility of perpetual motion. This impossibility, he argued, implied the central character of elementary forces, and therefore the conservation of force.[18]

Helmholtz's original intention was twofold. At the most fundamental level, he sought a unique reduction of phenomena to elementary central forces. At the phenomenological level, he wanted to identify the conserved quantities in the various known conversions of force, verify that known laws complied with the conservation of these quantities or, conversely, use conservation to restrict the form of the laws. The memoir on the conservation of force combined both strategies, and applied them to the whole range of physical and chemical phenomena.[19]

German physicists were relatively slow in appreciating the importance of Helmholtz's memoir. The empiricists were suspicious of a work that contained no original experiments. Among the leading theorists, Franz Neumann was mildly supportive, Weber indifferent, and Clausius frankly hostile. British physicists were far more receptive. Thomson expressed immense admiration for Helmholtz's memoir, and became a close friend of his. There was an obvious congruence between the two men's endeavors: they both admitted that physics was reducible to a frictionless mechanics, they both introduced a conserved quantity in conversions between different kinds of force, they both measured this quantity by the equivalent amount of mechanical work, and they both organized their physics under the resulting conservation principle. There were, however, important differences. For the young Thomson, the reducibility of physics to a conservative dynamics expressed the permanence of divine creations; for the young Helmholtz, it could be proved by transcendental reasoning based on the complete comprehensibility of nature. Also, Thomson had closer connections with the culture of engineers. He was more concerned with the practical aspects of energy conservation, and Helmholtz with the overall unification of physics.[20]

Helmholtz's philosophy of knowledge evolved with his insights into the physiology of vision. In the 1860s he gave up the notion of the comprehensibility of nature in terms of ultimate causes, and replaced it with a broader notion of lawfulness that warranted successful inductions from experience. Also, he became aware of difficulties in his two proofs that all phenomena could be reduced to the action of central forces acting in pairs. The inductive proof failed, since elec-

[18] Helmholtz 1847: 17–27. [19] Cf. Bevilacqua 1993.

[20] On the reception of Helmholtz's memoir, cf. Jungnickel and MacCormmach 1986, Vol. 1: 160–161; Bevilacqua 1994: 90–2. On the polemic with Clausius, cf. Heimann 1974a: 234–235; Bevilacqua 1994. On the differences between Thomson and Helmholtz, cf. Heimann 1974a; Smith and Wise 1989: Chs. 9–10, esp. pp. 306–307 (Thomson's theological argument); Smith 1998: 126–40.

tromagnetic forces, for example, were conservative without being central. The transcendental proof was also insufficent, because only half of the truth of the decomposition principle could be obtained *a priori*: for the motion of an extended mass to be completely known, the forces acting on each mass point must be known, but these forces are not necessarily the sum of forces emanating from mass points.[21]

Consequently, Helmholtz renounced the idea of a unique, final reduction of physics to the play of central forces, and favored a more phenomenological kind of physics. He maintained, however, that mechanical reductions in terms of central forces should remain *in principle* possible. He also made abundant use of the decomposition and the energy principles, as phenomenologically meaningful remnants of the faltered metaphysics of his memoir on the conservation of force.

6.3.2 Potentials

One third of the memoir on the conservation of force was devoted to electricity and magnetism. For electrostatics and magnetostatics, Helmholtz regarded the reduction to central forces as already given, and he immediately identified the total potential with the sum of tensional forces (potential energy). He also showed that the electric tension (*freie Spannung*)[22] of a conductor was equal to the living force gained by a unit of positive electricity while moving from the conductor to infinity. He gave the expression $\frac{1}{2}CV^2$ of the energy of a condenser, where C is the capacitance and V the applied tension, and used it to explain Peter Riess's empirical law for the heat produced by the discharge of a Leyden jar. The similarity with Thomson's physical potential is evident. Both men used energetic considerations to bring together Gauss's mathematical potential and operational concepts of electric tension.[23]

For galvanism, Helmholtz combined global energy balancing and the reduction of the contact tension to central forces. He derived the relation between the electromotive force of a galvanic cell and the heat of the chemical reactions at the electrodes. For electromagnetic forces, he adopted a purely phenomenological approach, since no reduction to central forces was yet available. In the case of a magnet moving under the influence of a galvanic current, he asserted that 'the living force won by the magnet must be provided by the tensional forces [the potential energy] which are consumed in the current.' In a unit of time, the consumption is equal to the product ei of the electromotive force e of the battery and the intensity i of the current. It serves in part to produce the Joule heat Ri^2, and for the rest to increase the kinetic energy of the magnet. According to Neumann, the work done by the electromagnetic forces on the magnet is equal to $-(\mathrm{d}x/\mathrm{d}t)(\mathrm{d}Pi/\mathrm{d}x)$, where x

[21] Helmholtz 1881b. Cf. Bevilacqua 1994; Darrigol 1994b: 219–24.

[22] Defined as the charge acquired by a remote conducting sphere with unit radius after connection with the conductor through a wire.

[23] Cf. Helmholtz 1847: 41–46, 58–61. Helmholtz did not explicitly state that his 'tension' was identical with the potential, although it had the same properties: cf. Bevilacqua 1994: 94–8.

is the displacement of the magnet and P its potential with respect to a unit current in the circuit. In this manner Helmholtz obtained the balance

$$ei = ri^2 - i\frac{\mathrm{d}P}{\mathrm{d}t}, \tag{6.6}$$

which requires the existence of Neumann's electromotive force of induction $\mathrm{d}P/\mathrm{d}t$. His reasoning was quite similar to Thomson's, but came out a few months earlier.[24]

Helmholtz's wording suggested that he had derived the induction phenomenon and its quantitative laws by a mere application of the energy principle. Under the criticism of Clausius and Carl Neumann, he later conceded that he had not done so much. Self-induction had to be taken into account. More problematically, Helmholtz's reasoning implicitly assumed that the internal energy of the current–magnet system did not depend on the position of the magnet. This happens to be true, but is by no means evident. The situation is worse in the case of the coupling between two circuits, which Helmholtz treated in a similar manner, although here the internal energy does depend on the mutual configuration of the two circuits. Defective as they were, these arguments convinced Helmholtz that Neumann's potential should be the central concept of electrodynamics, for it was well adapted to energy considerations.[25]

6.3.3 *Nervous excitation and the RL circuit*

In 1850 Helmholtz returned to electromagnetic induction, but in a different context. In his famous series of experiments that led to the measurement of the velocity of nervous excitation, he improved a method that Claude Pouillet had invented for measuring the short times involved in artillery firing. The idea was to send a current of known constant intensity through a ballistic galvanometer during the time to be measured. The maximum deviation of the magnet, which Helmholtz measured by Gauss and Weber's optical method, gave the quantity of electricity sent through the circuit, from which the duration of the current could be calculated. The main experimental difficulty was to design switches that started and interrupted the current right at the beginning and end of the time interval to be measured. Figure 6.1 represents Helmholtz's *Wippe* (seesaw): the percussion of C on A turns on the time-measuring current through aacc, and opens at ef the primary circuit of the induction coil that excites the nerve.[26]

There was, however, a theoretical doubt about the validity of the method: did the current in the galvanometric circuit gain its whole intensity immediately after the

[24] Helmholtz 1847: 46–57 for galvanism (also 57–8 for thermoelectric currents); *ibid.*: 61–5, for electromagnetic induction.

[25] Clausius 1853, 1854; C. Neumann 1871a, 1873b; Helmholtz 1854, 1873a: 677–9. The first correct expressions of the energy balance in a system of varying currents are in Helmholtz 1870a; C. Neumann 1871a; Thomson 1872a: 441n–2n (manuscript memorandum of 1851); C. Neumann 1873b. Cf. Knudsen 1995 for a very clear discussion.

[26] Helmholtz 1850a, 1850b. Cf. Olesko and Holmes 1993: 74–105.

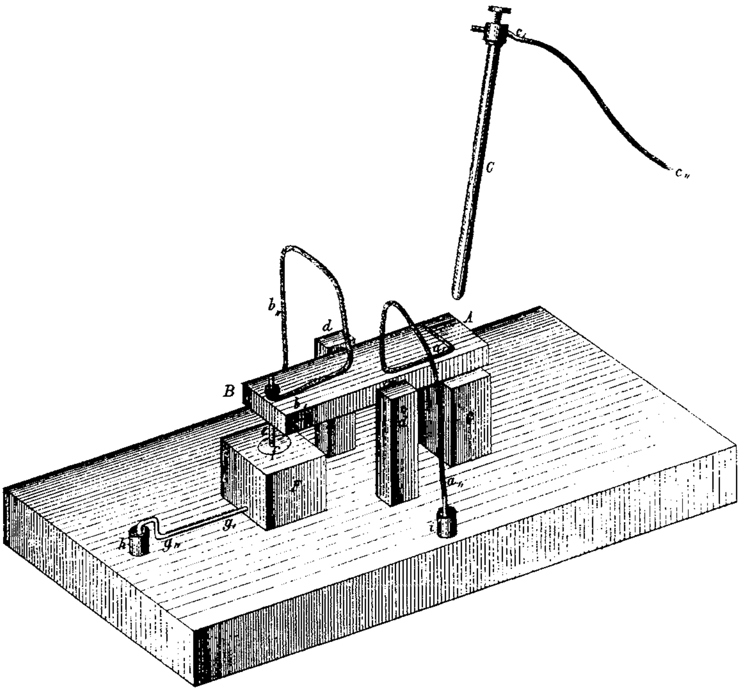

FIG. 6.1. Helmholtz's first *Wippe* (Helmholtz 1850a: plate 5).

circuit was closed? Being in Königsberg, Helmholtz probably consulted Neumann on this electric matter.

Self-induction had to play a role, but Neumann doubted that the transitory current could be computed because Ohm's law had only been established for constant or slowly varying currents. Helmholtz conjectured that if there was a coil in the circuit, the rise of the current would take enough time for the current distribution in the wire to be uniform at any instant and for Ohm's law to apply. Then the current would rise according to the formula

$$i = \frac{e}{R}\left(1 - \mathrm{e}^{-(R/L)t}\right), \tag{6.7}$$

where e is the electromotive force of the battery, R the total resistance of the circuit, and L the 'potential of the current on itself' (Neumann's name for the self-inductance of the coil). In order to avoid a circular recourse to time measurement, Helmholtz verified this law indirectly. For a series of *a priori* unknown but well-defined times, he determined the deviation of the magnet obtained when the circuit was open at those times, and the deviation obtained when the battery was suddenly replaced at the same times with a dead resistance equal to the internal resistance of

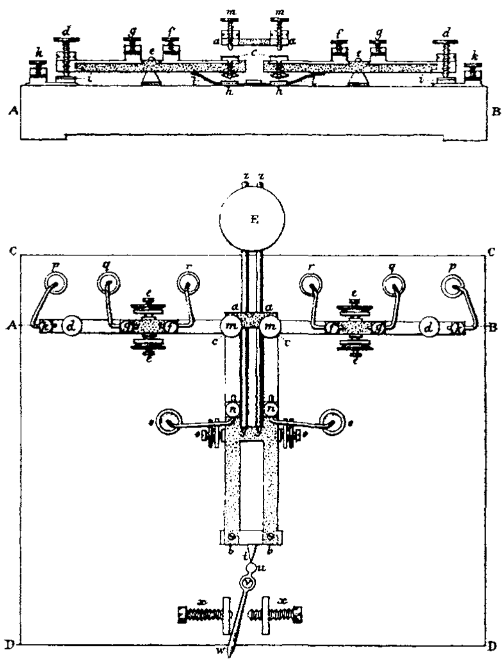

FIG. 6.2. Helmholtz's second *Wippe* (Helmholtz 1851: plate 3). Under the effect of the weight E, the main lever *aabb* rotates around the axis *oo* and acts on the smaller levers *cd*, which in turn rotate around the two axes *ee*. Contacts are thus made at *c*, and broken at *i*. A small, controllable delay between the contacts on the right and left sides is obtained by adjusting the screws m at a different height.

the battery. This was achieved by the ingenious *Wippe* of Fig. 6.2. For the coil used in his physiological experiments, Helmholtz found the ratio *L/R* to be about 0.001 second. This implied a non-negligible correction to the physiological times he had been measuring, which were of the order of 0.01 second. The side results were even more important: Helmholtz provided the first quantitative treatment of self-induction, extented Ohm's law to variable currents, and introduced sophisticated techniques for studying such currents.[27]

6.3.4 Electroshocks and skin effect

Helmholtz's next important contribution to electrodynamics occurred in 1869, after he had completed the philosophical conclusion of his *Physiological Optics*. He then confided to a friend: 'I found that too much philosophizing demoralizes me somehow, and makes my thoughts lax and vague; I shall discipline them again

[27] Helmholtz 1851.

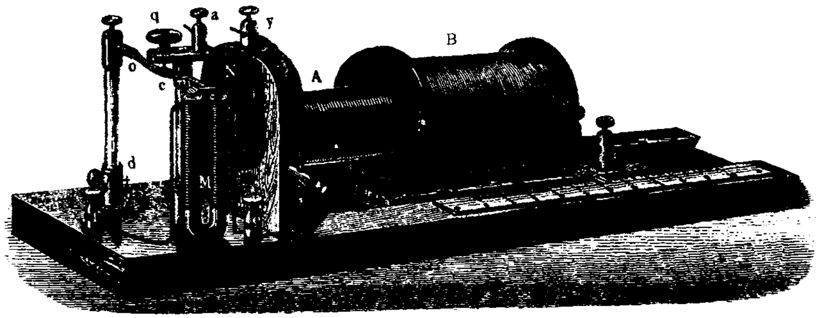

FIG. 6.3. Du Bois-Reymond's induction coil (from Wiedemann 1885, Vol. 4: 8).

for a while through experiments and mathematics, and then, afterwards, I might well return to the theory of perception.' He also believed that German physics was declining, and trusted that he could reverse the tendency through his own works.[28]

Again Helmholtz's inspiration came from physiology. In newer measurements of the propagation of nervous excitation, he observed that the induction coil did not act on the deeper-lying nerves of the human body, as was already known to electrotherapists. He suspected that the propagation of quickly varying currents through conducting masses was hampered by electromagnetic induction. In order to verify this assumption, he compared the penetration of currents of differents durations. His device exploited the standard resources of an electro-physiological laboratory: a Du Bois-Reymond induction coil (wihout the iron kernel) for the production of electric impulses, and the naked nerve of a frog leg muscle as a current detector. At that time there was no better detector for short, low-energy currents. Helmholtz immersed the frog nerve in a bath of salted water, at some distance from a pair of platinum electrodes. Those were connected to the induction coil, directly or through a Leyden jar.[29]

Du Bois-Reymond's induction coil (Fig. 6.3) is made of two cylindrical coils A and B that can slide into each other (so that the coupling can be adjusted). The internal coil A has few turns and is connected to a battery through an electromagnetic 'hammer' Mn that periodically breaks the circuit. The outer coil has many turns so that at each closing or opening of the primary circuit, an intense electromotive force of induction is produced at its ends. The currents obtained in a resistive secondary circuit during the opening of the primary (opening shocks) are

[28] Helmholtz to Ludwig, 28 March 1869, quoted in Königsberger 1903, Vol. 2: 162; Helmholtz to Beseler, 28 May 1868, quoted *ibid.*: 115: 'I see that the younger generation in Germany is not making any substantial progress in scientific, and especially in mathematical physics. The few great names in this branch, which is the true basis of all proper natural science, are old, or begin to recede into the older generation, while there is no new generation rising up to their place; and on this account I must say to myself that if I could get an influence over my pupils in this department, I might perhaps do more important work there than in physiology, where a vigorous school is now in full and growing activity.'

[29] Helmholtz 1869a.

far more sudden and intense than those obtained during its closing (closing shocks), because the formation of the primary current takes time. When the secondary circuit contains a Leyden jar, the closing shocks become oscillatory, with a period much smaller than their overall duration. Helmholtz compared the action of the various kinds of secondary currents on the frog nerve according to its distance from the electrodes. He found that the faster the variation of the current was, the less it could penetrate the conducting solution. This was the first experimental proof of a kind of skin effect.

6.3.5 The RLC circuit with a frog leg

The interpretation of these experiments required a demonstration of the oscillations in the circuit with the Leyden jar. In 1847 Helmholtz had already propounded that the discharge of a Leyden jar through a wire was oscillatory. He then believed that the oscillations would explain why the heat produced by the discharge was the same for every connecting wire. More soundly, he remarked that Wollaston and Faraday had obtained symmetrical electrolysis by electrostatic discharge.[30]

In 1853 Thomson gave the now classical analysis of the *RLC* circuit: the decrease of the electrostatic energy $Q^2/2C$ of the capacitor must be equal to the increase of the magnetic energy $Li^2/2$ plus the Joule heat produced in the resistance:

$$-\mathrm{d}\left(\frac{Q^2}{2C}\right) = \mathrm{d}\left(\frac{1}{2}Li^2\right) + Ri^2\mathrm{d}t, \qquad (6.8)$$

which implies, for a small resistance R, oscillations with the period $2\pi(LC)^{1/2}$ and the damping time RC. In 1859 Bernhard Feddersen confirmed the oscillatory character of the sparking discharge of a Leyden jar by the method of the rotating mirror. No one, however, had been able to study the discharge current in the absence of a spark gap. With frog legs and the Du Bois coil, Helmholtz could do just that.[31]

His device is schematized in Fig. 6.4. A current is started impulsively in the secondary circuit by opening the interrupter K_1. After a small preset time, the switch K_2 is rotated so that the frog leg current detector is inserted in the secondary circuit. For determining the time, Helmholtz used a device he had already applied to an improved measurement of the velocity of nervous excitations: a heavy second pendulum, acting on K_1 and K_2 at two different, adjustable points of its fall. He found that for a periodic set of values of the time, the frog nerve was not excited, which meant that the current periodically vanished. For the coil and jar used in his previ-

[30] Helmholtz 1847: 46. Several investigators had already suspected that the discharge of a Leyden jar could be oscillatory, for example John Henry in 1842: cf. Whittaker 1951: 226.

[31] Thomson 1853b; Feddersen 1857, 1858, 1859, 1908. Kirchhoff 1864 derived the same equation on the basis of Weber's theory, including theoretical values for the capacity and the self-inductance in Feddersen's experiments. Cf. Wiedemann 1885, Vol. 4: 166–77, 1083–7.

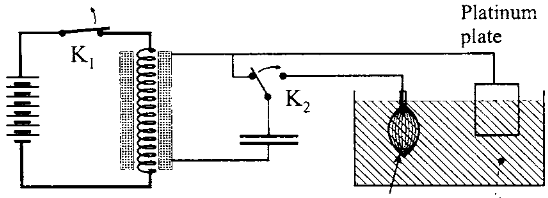

FIG. 6.4. Helmholtz's arrangement for studying electric oscillations.

ous experiment, the period was about 1/2000 second, and the damping time 1/50 second.[32]

6.3.6 The potential law

Helmholtz now knew how to produce and measure oscillatory currents, and he had shown that high frequency prevented the penetration of large conducting masses. The latter phenomenon was of special theoretical interest because it *a priori* involved the yet unexplored electrodynamics of open currents: the propagation of the current most likely implied variable charges at the surface and in the mass of the conductor. Helmholtz attacked this theoretical problem with tremendous analytical power. He was aware of three relevant theories: Kirchhoff's general theory of the motion of electricity in three-dimensional conductors, an extension of Neumann's theory to this case, and Maxwell's rather different theory. 'In the face of conflicting theories,' Helmholtz preferred 'to remain as close as possible to the ground of facts and to leave undetermined the parts of the theory which could not be decided by experiment.' He started with the established laws for closed, linear currents, and sought the most general extension agreeing with the energy principle.[33]

By analogy with the case of closed currents, Helmholtz admitted 'the potential law,' that is, the existence of a potential that yielded mechanical forces by spatial derivation and electromotive forces by time derivation. In other words, he extended Neumann's concept of electrodynamic potential to open currents.[34] He knew this was not an obvious step. The electric current being a kinetic phenomenon, no potential energy existed in the 'tensional' sense of the memoir on the conservation of

[32] Helmholtz 1869b. In 1871 Helmholtz performed a more delicate experiment with a Kohlrausch condenser, measuring the charge of this condenser with a Thomson electrometer after sudden interruptions of the current (Helmholtz 1871, whose main purpose was to refute Blaserna's small value for the propagation velocity of inductive actions).

[33] Helmholtz 1870b: 546. On Helmholtz's electrodynamics, cf. Rosenfeld 1956; Woodruff 1968; Hirosige 1969: 161–7; Buchheim 1971; Wise 1981b: 295–301; Buchwald 1985a: 177–86; Darrigol 1993a: 223–39; Kaiser 1993; Buchwald 1994: 7–42.

[34] Neumann did not admit this extension: instead he accepted Ampère's forces between current elements, which do not derive from a potential. Nevertheless, in 1870 Helmholtz presented the potential law for current elements as Neumann's.

force. Also, the Amperean equivalence of electric currents and double magnetic sheets did not apply to open currents. Helmholtz justified his extension of Neumann's potential by evoking Thomson's and Maxwell's opinion that a definite kinetic energy corresponded to a system of currents, be they closed or not. Then Neumann's potential, which is the negative of this energy, also had to exist.[35]

As Helmholtz later realized, this reasoning is flawed: no potential needs to exist even if the kinetic energy exists. But he had other reasons to favor the potential law: it was the simplest law he could imagine in harmony with the energy principle and the decomposition principle. As we have seen, these principles were powerful remnants of the earlier notion of the comprehensibility of nature. Although Helmholtz no longer wished to reduce electrodynamics to central forces acting in pairs, he still decomposed physical systems into pairs of infinitesimal objects, whose interactions simply derived from a mutual energy.[36]

For closed linear currents, Neumann had given the potential (in electromagnetic units)

$$P = -ii'\int\int\frac{\mathrm{d}\mathbf{l}\cdot\mathrm{d}\mathbf{l}'}{r}. \tag{6.9}$$

In a straightforward generalization to closed (divergenceless) three-dimensional currents, the potential is

$$P = -\frac{1}{2}\int \mathbf{j}\cdot\mathbf{A}\,\mathrm{d}\tau, \tag{6.10}$$

with

$$\mathbf{A}(\mathbf{r}) = \int\frac{\mathbf{j}(\mathbf{r}')\mathrm{d}\tau'}{|\mathbf{r}-\mathbf{r}'|}. \tag{6.11}$$

The electric current is then given by Kirchhoff's generalization of Ohm's law

$$\mathbf{j} = \sigma\left(-\nabla\phi - \frac{\partial\mathbf{A}}{\partial t}\right), \tag{6.12}$$

where ϕ is the electrostatic potential such that

$$\Delta\phi + 4\pi c^2\rho = 0. \tag{6.13}$$

[35] Helmholtz 1870b: 562. Cf. Darrigol 1994b: 228–9.

[36] Cf. Buchwald 1994: 20–4, who sees in the potential law an instance of a more general taxonomy of interactions: Helmholtz and his disciples analyzed all physical processes in terms of interaction energies between pairs of objects of various kinds (charge carriers, current carriers, etc.).

For open currents, Helmholtz showed that the most general expression of the vector potential that complied with the expression (6.11) for closed currents and decreased like $1/r$ far from the currents was

$$\mathbf{A}(\mathbf{r}) = \int \frac{\mathbf{j}'(\mathbf{r}')\mathrm{d}\tau'}{|\mathbf{r}-\mathbf{r}'|} + \frac{1-k}{2}\nabla\xi, \tag{6.14}$$

with[37]

$$\xi(\mathbf{r}) = -\int \nabla\cdot\mathbf{j}(\mathbf{r}')|\mathbf{r}-\mathbf{r}'|\mathrm{d}\tau'. \tag{6.15}$$

To different choices of the constant k correspond different theories of the motion of electricity in conductors. For $k = 1$, Neumann's simple potential formula is retrieved. Helmholtz attributed the resulting laws of motion to Neumann—who, however, had carefully avoided any statement on the induction produced by open currents. The case $k = -1$ yields the equations derived by Kirchhoff's from Weber's theory (cf. p. 72). Lastly, Helmholtz asserted without proof that $k = 0$ gave Maxwell's theory. This requires some explanation.[38]

For $k = 0$, the vector potential becomes divergenceless. Taking the divergence of the expression (6.12) of Ohm's law, we find

$$\frac{\partial\rho}{\partial t} + 4\pi c^2\sigma\rho = 0. \tag{6.16}$$

This equation, which also holds in Maxwell's theory, implies that no charge can subside within a conductor for more than a tiny fraction of time, about 10^{-17} second for copper.[39] Consequently, ξ vanishes and the expression (6.14) of the vector potential becomes identical to Maxwell's, as long as the displacement current remains negligible compared with the conduction current in the conductor. This is indeed the case if the (unknown) dielectric constant of the metal is not too high and if surface effects are negligible (see Appendix 7).[40] Helmholtz therefore had reason to identify the case $k = 0$ with Maxwell's theory, as he only meant that the two theories led to the same equations for the motion of electricity *within conductors* (see also Appendix 7).

Remarkably, Helmholtz discussed the consequences of Maxwell's theory before any British physicist did, and before the publication of the *Treatise*. He had met Maxwell and had experimented with him on a colour-blind man in 1864. Less anecdotally, he admired British physics and favored its diffusion in Germany, for instance by arranging for the translations of Thomson and Tait's *Natural Philosophy* and Tyndall's *Heat as a Mode of Motion*. He was a closer friend to Thomson than he

[37] Helmholtz 1870b: 568–9. Helmholtz used electrostatic units. [38] Helmholtz 1870b: 549.
[39] Helmholtz 1870b: 588, 578, 603.
[40] In his studies on electric propagation, Heaviside assumed this constant to be zero.

was to any German physicist, except perhaps Kirchhoff. We have already noted Helmholtz's affinities with British energetics. He praised Thomson for 'avoiding as fas as possible hypotheses on unknown subjects' and Maxwell for bringing electrodynamics into harmony with the general principles of dynamics.[41]

6.3.7 Exeat Weber's law

Having reached a theory encompassing all previously known theories, Helmholtz applied it to a spherical conductor and to an infinite conductor occupying the half space $x > 0$. In the latter case and for a periodic current with the frequency ν, he found that the current was an exponentially decreasing function of the distance from the surface, the penetration length being $(\sigma\nu)^{-1/2}$. He stated this law without proof (and without formula) at the end of his paper on the penetration of electric shocks.[42]

While performing this kind of calculation, Helmholtz made a very interesting discovery: for negative values of k, which include Weber theory, the equilibrium of electricity in (on) conductors is unstable. This is most easily seen through an analogy with fluid motion. From eqns. (6.12–6.15) follows the differential equation for the vector-potential:

$$\frac{\partial \mathbf{A}}{\partial t} - \frac{1}{4\pi\sigma}\Delta\mathbf{A} - \frac{1}{4\pi\sigma}\frac{1-k}{k}\nabla(\nabla\cdot\mathbf{A}) = -\nabla\phi. \tag{6.17}$$

As an expert on organ pipes, Helmholtz immediately noticed the similarity to the equation for the small perturbations of a viscous, compressible fluid:

$$\frac{\partial \mathbf{v}}{\partial t} - \alpha\Delta\mathbf{v} - \beta\nabla(\nabla\cdot\mathbf{v}) = -\frac{1}{\mu}\nabla p, \tag{6.18}$$

where $\mathbf{v}$ is the velocity of the fluid, μ its density, p its pressure, and α and β the two viscosity coefficients. In this analogy $\mathbf{A}$ corresponds to the velocity, and ϕ to the pressure. Consequently, the relation

$$\nabla\cdot\mathbf{A} = -\frac{k}{c^2}\frac{\partial\phi}{\partial t}, \tag{6.19}$$

which results from Helmholtz's potential formula, means a compressibility k/c^2 for the fluid. A negative value of k makes the fluid unstable.[43]

[41] Helmholtz 1885: 588 (quote); 1873c and 1881b: 56 (on Maxwell). On his meeting Maxwell, cf. Königsberger 1902–03, Vol. 2: 53. On his supporting British physics, cf. Archibald 1989: 287; Cahan 1994: 332–3; Buchwald 1994: 401–2. On his friendship with Thomson, cf. Koenigsberger 1901–1903, Vol. 2: 286; Smith and Wise 1989: 132, 527.

[42] Helmholtz manuscript #644: 'Inductionsströme in Körper' (Akademie der Wissenschaften, Berlin); Helmholtz 1869a: 530. The result does not depend on k as long as the wavelength is much larger than the penetration length.

[43] Helmholtz 1870b: 589–91 (radial currents in conducting sphere), 577–8 (analogy). Kirchhoff was already aware of the instability, but had not published it: see Helmholtz, *ibid.*: 543n.

The instability could be seen directly in the expression for the electrodynamic energy, which Helmholtz found to be

$$-P = \frac{1}{8\pi}\int(\nabla\times\mathbf{A})^2 \mathrm{d}\tau + \frac{k}{4\pi c^4}\int\left(\frac{\partial\phi}{\partial t}\right)^2 \mathrm{d}\tau. \qquad (6.20)$$

When k is negative, there are states of motion which have less energy than the state of equilibrium. By studying a simple case with spherical symmetry, Helmholtz further showed that external perturbations could trigger the instability.[44]

Helmholtz traced this absurd behavior to the conflict of Weber's law with the energy principle. As Weber first showed in 1848, his forces derive from the potential

$$V = \frac{ee'}{r}\left(1 - \frac{\dot{r}^2}{C^2}\right) \qquad (6.21)$$

($\mathrm{d}r/\mathrm{d}t$ being treated as an implicit function of r). Hence they do not permit the production of work in a cycle. However, the velocity-dependent term in the potential acts as a negative correction to the kinetic energy. The effective mass m of a charged particle e moving around the fixed charged particle e' is $m - 2ee'/rC^2$. At the critical distance $2ee'/mC^2$, the mass vanishes and changes sign. This implies grave anomalies, including the possibility of an indefinite increase of velocity at finite distance. Helmholtz had never believed Weber's forces to be fundamental, since in his opinion only central forces could be so. He could now show that Weber's law implied dynamic absurdities.[45]

Having excluded all negative values of k, Helmholtz examined the possibilities of a further experimental determination of this parameter. He solved the problem of motion for a spherical conductor with central excitation and for an infinite cylindrical conductor, and reached a disappointing conclusion: for the frequencies available in the laboratory, the value of k had no measurable effect on the propagation of electricity (see Appendix 7). In this indeterminate situation, Helmholtz recommended 'Maxwell's choice' $k = 0$, which greatly simplified the equations.[46]

6.3.8 Polarization

In the last section of his memoir, Helmholtz studied the effects of electric and magnetic polarization in the space between the conductors. For diamagnetism, he ignored Weber's theory and instead favored Becquerel's theory of diamagnetism, according to which vacuum was more polarizable than diamagnetics. He was also open to the possibility that vacuum had electric polarizability, as Faraday and Maxwell had

[44] Helmholtz 1870b: 578–85, 591–9.
[45] Helmholtz 1870b: 553–4. Cf. Kaiser 1981: 100–08; Archibald 1989: 292–4; Assis 1994: 180–202, siding with Weber.
[46] Helmholtz 1870b: 599–611.

assumed. He praised Maxwell's proof that a polarizable medium could serve to propagate light as 'a result of superior importance.' He maintained, however, the concept of charges and currents directly acting at a distance. Like Poisson and Mossotti, he treated polarization as a local displacement of electricity under electromotive forces. Then he determined the resulting interactions through the potential law (see Appendix 7).[47]

For a finite electric polarizability κ_0 of vacuum, Helmholtz proved that the electric polarization obeyed a wave equation, with the propagation velocity $c_0(1 + \kappa_0)^{1/2}(k\kappa_0)^{-1/2}$ for longitudinal waves, and $c_0\kappa_0^{-1/2}$ for transverse waves. In these expressions, c_0 is the ratio between the electromagnetic and the electrostatic charge units in a fictitious non-polarizable vacuum. In the polarizable vacuum, all electrostatic charges appear to be diminished by a factor $(1 + \kappa_0)^{1/2}$. Therefore, the measured ratio of units is $c = c_0(1 + \kappa_0)^{-1/2}$. In terms of this quantity, the propagation velocities are $c(1 + \kappa_0)(k\kappa_0)^{-1/2}$ for longitudinal waves and $c(1 + \kappa_0)^{1/2}\kappa_0^{-1/2}$ for transverse waves. In the case of infinite vacuum polarizability, longitudinal waves no longer exist, and transverse waves travel at the velocity c, known to agree with that of light. Helmholtz concluded:[48]

> The remarkable analogy between the motions of electricity in a dielectric and those of the luminiferous ether do not depend from the special form of Maxwell's hypotheses; it can be obtained in an essentially similar manner if we maintain the older view of electric actions at a distance.

More generally, Helmholtz came to believe that all results of Maxwell's theory could be obtained by taking the limit of infinite vacuum polarizability in his theory (for $k = 0$). In other words, a limiting case of his theory was empirically equivalent to Maxwell's, although it was based on a totally different picture of electricity. Poincaré later proved Helmholtz's claim to be correct (see also Appendix 7). Intuitively, the convergence of the two theories may be understood by noting that in the limit of infinite vacuum polarizability, any open conduction current is continued by an equal polarization current, which then plays the role of Maxwell's displacement current. Should not, however, the infinite vacuum polarization screen off all electric charge? The answer is no, because Helmholtz's bare charges, being non-measurable, can be assumed to be infinite so as to yield finite renormalized charges.[49]

Despite their empirical equivalence, Maxwell's field theory and Helmholtz's limiting case carried different degrees of conviction. Helmholtz perceived Maxwell's equations as an extreme case in a continuous range of possibilities, from zero to infinite vacuum polarizability. He preferred zero polarizability, which gave the simplest equations and the simplest interaction. In addition, the two versions of Maxwell's theory carried different heuristics, mainly because Helmholtz's concept of polariza-

[47] Helmholtz 1870b: 556, 557, 611–28. [48] Helmholtz 1870b: 558 (quote), 626.

[49] Helmholtz 1875a: 788. In fact, the condition $k = 0$ is not necessary. Cf. Poincaré 1891: Ch. 5; Darrigol 1993a: 237–8. There is an obvious analogy between Helmholtz's charge renormalization and the corresponding operation in modern quantum field theory.

tion was electrical whereas Maxwell's was mechanical. This contrast is best seen by comparing later Helmholtzian and Maxwellian microphysics, as will be done in Chapters 7 and 8. We may also observe that Helmholtz immediately connected the propagation of polarization with the retardation of electrodynamic actions, whereas Maxwell largely ignored this issue (see Chapter 5, p. 202).[50]

Helmholtz published his theory of the motion of electricity in 1870, as a very impressive memoir of hundred pages. He believed that he had produced a general theoretical scheme that included the major theories of electricity for specific values of two parameters (k and κ_0). He regarded the selection among these possibilities as the essential problem of contemporary electrodynamics, and contributed to its solution by proving the energetic absurdity of Weber's theory and by showing that the further determination of the parameters was beyond available experimental means. He saw the immediate future of electrodynamics as further contributions to this problem. His critics decided differently.

6.3.9 Polemics with the Weberians

With his airs of detached objectivity, Helmholtz had attacked a well-guarded fortress. He faced a vigorous response from the old Weber and his friends. We recall that when Weber first proposed his force law in 1846, he gave it only a descriptive value, and anticipated a more fundamental level of explanation. After accumulating successes in the microphysical applications, however, he came to regard this law as truly fundamental. In 1869 he expressed his conviction that the very simple form (6.21) of the potential from which the forces derived had a physical meaning. Two years later, he found this meaning in a new formulation of the energy principle.[51]

Weber first redefined the potential energy U of two electric particles e and e' with the relative velocity $\dot{r}$ as the work of the forces between them when their distance r goes from a critical length ρ to infinity. This energy, unlike the usual potential energy, depends only on velocity. Weber's version of the energy principle required that the total energy of the two particles, kinetic plus potential, should not depend on their relative velocity. Since for zero velocity the usual electrostatic attraction must be retrieved, this condition reads

$$U + \frac{1}{2}\mu\dot{r}^2 = \frac{ee'}{\rho}, \tag{6.22}$$

where μ is the reduced mass. If the force derives from a potential $V(r, \dot{r})$ that vanishes at infinity, then $U = V(\rho, \dot{r})$. According to Weber, this condition could only be met by taking $\rho = 2ee'/\mu C^2$ (where C is a universal constant) and

[50] Helmholtz 1870b: 528 (retardation). On the different heuristics, cf. Hirosige 1969; Buchwald 1985a: 183–6.

[51] Weber 1869, 1871.

$$V = \frac{ee'}{r}\left(1 - \frac{\dot{r}^2}{C^2}\right), \tag{6.23}$$

in conformity with his fundamental law. He commented:[52]

Helmholtz had the right to *tentatively* formulate the energy principle so that my fundamental law [. . .] contradicts it; however, I am equally justified to do the contrary, namely, to *tentatively* formulate the energy principle so that the law not only *agrees* with it but even follows from it.

Weber's redefinition of the energy principle was so peculiar and so incompatible with common notions of conservation—it required, for example, that the energy of a non-isolated system of charges would be conserved—that Helmholtz did not even think it worth a comment. However, Helmholtz had to answer more specific criticism by Weber and his Leipzig friends Carl Neumann and Friedrich Zöllner. To Carl Neumann's remark that Kirchhoff's electrodynamic laws, which led to damaging instabilities, depended not only on Weber's law but also on auxiliary molecular assumptions that could be altered, he opposed that for large enough conductors the instability was independent of molecular processes. To Weber's contention that the dynamical anomalies of Weber's law only occurred for velocities larger than C and for exceedingly small distances at which the law did not need to be strictly valid, he opposed a new example in which these restrictions did not apply. Finally, he invoked Maxwell's vortex model of the electromagnetic field as a proof that electric phenomena could be explained by means of ordinary mechanical forces, without Weber's peculiar velocity dependence.[53]

Helmholtz formulated his replies in a haughty style which upset his opponents. The controversy turned sour, but slowly died off in the 1880s with no clear winner. Helmholtz certainly demonstrated that Weber's law involved anomalous dynamic behavior and thus shed doubt on its fundamental status. Yet he failed to convince Weber and friends that the anomalies were fatal. This would have been difficult, considering the achievements and diffusion of Weber's theory. For Zöllner, national interest was also at stake: Helmholtz was a traitor who cultivated British methods and denigrated the good old German ones.[54]

6.3.10 *Experimentum crucis*

Helmholtz received another criticism from his faithful enemy, the French Adademician Joseph Bertrand. According to Ampère's theory, Bertrand noted, the

[52] Weber 1871, 1874, 1875, 1878: 364. Cf. Archibald 1989: 296–7, 303–04.

[53] C. Neumann 1871a; 1871b: 478; 1875; Helmholtz 1873a: 669–674; 1881c: 687; Weber 1871: 296–8; 1874: 300–01; 1875: 328–34; 1878: 405–11; Zöllner 1872: *Vorrede* (siding with Weber and C. Neumann); Helmholtz 1872: 638–9; 1873a: 674 (on Maxwell). Cf. Hoppe 1884: #354, and Assis 1994: 180–202 (on Weber's side); Wiedemann 1885, Vol. 4: 1087–1095 (on Helmholtz's side): 1087–95; Archibald 1989.

[54] For hurt sensibilities, see C. Neumann 1877; Weber 1878: 412; Zöllner 1876; Helmholtz 1881c. On Zöllner's politics, cf. Molella 1972; Buchwald 1994: 402–04; Cahan 1994.

mere rotation of a current element did not produce any work, whereas Helmholtz's potential depended on the orientation of the element. Helmholtz replied that according to his potential law the interaction of two current elements involved not only an attraction (or repulsion) but also mutually opposed torques. His and Ampère's predictions agreed only for closed currents. For an open-ended linear current i, the variation of the potential with respect to the path of the currents yielded a force $i d\mathbf{l} \times (\nabla \times \mathbf{A})$ acting on the elements $d\mathbf{l}$, in conformity with Ampère's law (when the acting currents are closed), *and also* two forces $-i\mathbf{A}(\mathbf{r}_1)$ and $i\mathbf{A}(\mathbf{r}_2)$ acting at the starting and ending points $\mathbf{r}_1$ and $\mathbf{r}_2$ of the current (see Appendix 7). The latter forces were unknown to Ampère.[55]

According to the energy principle, additional electrodynamic forces should imply additional electromotive forces in moving conductors. Helmholtz verified this correlation in a general electrodynamics of moving bodies. He gave the electromotive force at a point of a conductor moving at the velocity $\mathbf{v}$ as the convective variation of the vector potential, which is (see Appendices 5 and 7):

$$\mathbf{E} = -\frac{\partial \mathbf{A}}{\partial t} + \mathbf{v} \times (\nabla \times \mathbf{A}) - \nabla(\mathbf{v} \cdot \mathbf{A}). \tag{6.24}$$

The gradient term, if it were acting alone in the conductor, would produce a Joule heat equal to the work of the forces acting at the extremities of the current filaments. The theories of Franz Neumann, Weber, and Maxwell do not know of the latter forces; accordingly, they do not include the gradient term in the induction law.[56]

Although Helmholtz's original intention had been to discriminate between Neumann's, Weber's, and Maxwell's theories, he now fully realized that his potential law contradicted all previous electrodynamic theories, whatever the value of the constant k was. Special values of k retrieved the predictions of these theories only for the motion of electricity in conductors at rest. When moving bodies were involved, Helmholtz's theory implied novel ponderomotive and electromotive forces. This did not disturb Helmholtz's faith in the potential law. However, he now admitted the possibility of theories in which electrodynamic forces did not derive from a potential, and he himself proved that energy conservation could be satisfied in a more complicated manner without giving up Ampère's force law. In this new situation the determination of k became a secondary issue, and Helmholtz started instead to imagine experiments that would decide between the potential law and other theories.[57]

[55] Bertrand 1871; Helmholtz 1873a: 679–80; 1873b. In 1868 Bertrand had tried, unsuccessfully, to demolish Helmholtz's theory of vortex motion. In 1872 Bertrand argued that the forces implied by Helmholtz's potential law would destroy any current-carrying wire, which triggered a longer polemic: Bertrand 1872, 1873; Helmholtz 1873b: 699–700; 1874a: 708, 714n, 720n, 721n, 726–728. Cf. Buchwald 1994: 402, 405–06.

[56] Helmholtz 1874a. Cf. Darrigol 1993b: 266–71.

[57] Helmholtz 1874a: 753–759 for the theory based on Ampère's forces; 1873b: 700–1 for crucial experiments.

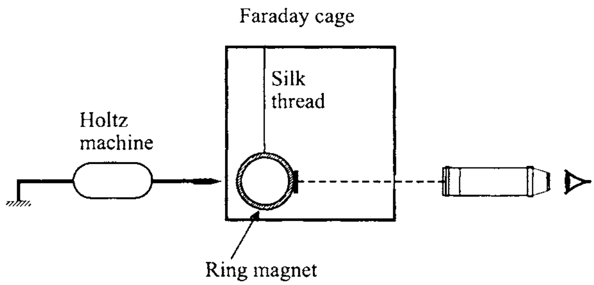

FIG. 6.5. Schiller's experiment.

Hermann Herwig and Zöllner soon claimed, with Carl Neumann's support, that simple electromagnetic rotation experiments excluded the potential law: a rigid current-carrying wire could be set into rotation by a cylinder magnet, even though the relevant potential did not depend on the rotation angle. Helmholtz knew in advance that no experiment based on closed currents could disprove his theory, since in that case the difference between the potential law and Ampère's law vanished. He promptly and drily dismissed the attack, showing that the potential variation of the liquid conductor or the sliding contact through which the current was brought to the rotating conductor accounted for the observed rotation.[58]

Truly crucial experiments were not so obvious, since they necessarily involved open circuits. In 1874 Helmholtz's Russian student Nicolaj Schiller tested the force acting between a delicated suspended ring magnet and a metallic needle connected to an electrostatic machine (Fig. 6.5). According to the potential law, the convection current at the end of the needle had no electrodynamic action, and the current in the needle therefore acted as an open current. The ring magnet could only act on the extremity of this current, since the corresponding vector potential was irrotational. From the negative result of this experiment Helmholtz concluded: 'Either the actions of current extremities predicted by the potential law do not exist, or we need to consider the electrodynamic actions of the convectively transferred electricity besides those predicted by the potential law.'[59]

A few months later Helmholtz performed a more decisive experiment concerning electromagnetic induction in a moving, open conductor. According to the potential law, induction depends on a variation of the electrodynamic potential. Helmholtz therefore imagined the device of Fig. 6.6, in which the conductor bb rotates in a uniform magnetic field. If the magnetic force is parallel to the axis, the electrodynamic potential does not vary, and there should be no induced current according to the potential law. In contrast, other theories of electromagnetic induction, or

[58] Herwig 1874; Zöllner 1874; C. Neumann 1874: 145; Helmholtz 1874b. Zöllner and Carl Neumann judged Helmholtz's solution artificial and his tone arrogant: Zöllner 1876; C. Neumann 1877. Cf. Buchwald 1994: 16–19.

[59] Schiller 1876; Helmholtz 1875: 781. Cf. Buchwald 1994: 33–6.

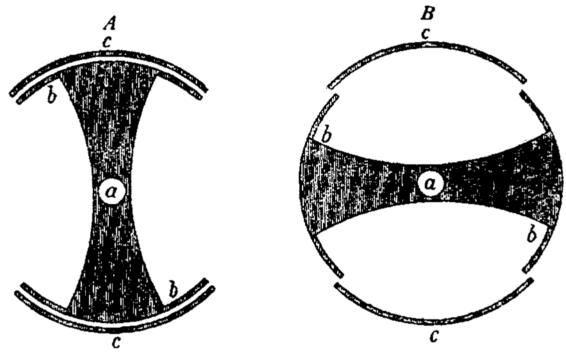

FIG. 6.6. Helmholtz's device for testing the potential law (Helmholtz 1875: 783).

Faraday's rule of the cut lines of force, imply an induced current along bb and electric charges on the curved plates b and b.[60]

The main difficulty was to obtain the necessary sensitivity for the detection of these small charges. Helmholtz imagined a clever procedure of accumulation. Thanks to a rotating commutator, the fixed plates c and c are grounded whenever bb is in the position A, so that the apparatus acts as a capacitance charging under a given electromotive force. In the configuration B, these plates are instead connected to a high-capacity condenser, which accumulates the charges developed in the position A. After a large number of rotations the charge of the condenser is tested with Thomson's quadrant electrometer.

Helmholtz had the unpleasant surprise of finding a charge in agreement with the competing theories. He concluded: 'The potential law is not in accordance with the facts, as long as it only considers the electric motions in conductors and their distance actions.' Only two possibilities were left: either the potential law was given up in favor of Ampère's law, or the currents in conductors were completed with polarization currents in insulators. Helmholtz naturally preferred the second alternative, which he had already explored in his 1870 memoir. After describing his crucial experiment, he explained how a high polarizability of the air between the plates b and c implied the same induced charge as the theories based on Ampère's law. In unpublished manuscripts he developed a full electrodynamics of moving bodies including a polarizable medium (see Appendix 7).[61]

6.3.11 The Berlin prize question of 1879

Helmholtz was the only physicist to be surprised by the outcome of his crucial experiment. Weberians saw a glaring confirmation of Weber's induction formulas. Other Germans satisfied themselves that Franz Neumann's old induction law, without

[60] Helmholtz 1875: 783–7. Cf. Darrigol 1993b: 272–3; Buchwald 1994: 38–41.
[61] Helmholtz 1875: 787; Helmholtz manuscripts #609, #622 (Akademie der Wissenschaften, Berlin).

Helmholtz's unwarranted modification, yielded the correct result.[62] As for Maxwell, he had every reason to believe in the general validity of Faraday's rule of the cut lines of force. Helmholtz's surprise depended on his epistemological prejudice in favor of the potential law. So did his subsequent attitude. The crucial Helmholtzian question became whether a variable polarization of insulators contributed to the electrodynamic potential. In July 1879 the Berlin Academy advertized the following prize question:[63]

The theory of electrodynamics which was brought forth by Faraday and was mathematically executed by Mr. Cl. Maxwell presupposed that the formation and disappearance of the dielectric polarization in insulating media—as well as in space—is a process that has the same electrodynamic effects as an electric current and that this process, just like a current, can be excited by electrodynamically induced forces. According to that theory, the intensity of the mentioned current would have to be taken equal to the intensity of the current that charges the contact surfaces of the conductor. The Academy demands that decisive proof be supplied:

[1] for or against the existence of electrodynamic effects of forming or disappearing dielectric polarization with the intensity assumed by Maxwell
[2] for or against the excitation of dielectric polarization in insulating media by magnetically or electrodynamically induced electromotive forces.

6.4 Hertz's response

6.4.1 Assimilating Helmholtz

Helmholtz soon suggested that his star pupil Heinrich Hertz work on the Academy questions. Hertz had already shown extraordinary experimental skills in solving another prize question regarding the kinetic energy of electricity in motion. The latter problem bore on the deeper nature of electricity, and had already been examined by Weber and Maxwell, among others. It was even more important to Helmholtz, for a special reason: he had shown that in Weber's theory the equilibrium of electricity in a conducting sphere was unstable whenever the radius of the sphere was larger than the square root of the mass of the electromagnetic unit of charge. If this mass was small enough, the instability would occur for small spheres and would thus do more harm to Weber's theory.[64]

The principle of Hertz's determination of the mass of electricity was to measure its contribution to the self-inductance of a coil. By an astute combination of standard techniques of electric measurement and after several improvements, he found

[62] According to Franz Neumann, the integral electromotive force due to the motion of an open linear conductor is equal to the potential of the quadrilateral made by the initial and final positions of the conductor and the traces of its extremities. This is equivalent to Faraday's rule of the cut lines force.

[63] Preussische Akademie der Wissenschaften zu Berlin. *Monatsberichte* (July 1879): 519, 528–9. Quoted in Bryant 1988: 7.

[64] Hertz 1892a: 1; Weber 1864: 235–41; Maxwell 1873a: ##573–7; Helmholtz 1870b: 589–90. On Hertz's biography, cf. McCormmach 1972; Buchwald 1994; Süsskind 1995; Fölsing 1997 (best documented).

that the kinetic energy of the electric flow in 1 mm^3 of a copper wire was less than 0.008 mg · mm for one electromagnetic unit of current. For Helmholtz's sake, Hertz reckoned that if the velocity of electricity was larger than 1 mm/s, the anti-Weber instability would occur for spheres of radius no larger than 0.11 mm.[65]

Important characteritics of Hertz's method as an experimenter can already be seen in this early work. He paid much attention to sources of error, and conceived his apparatus so as to minimize them. He favored simple geometries of his circuits in order to make them computable, and excelled in the relevant analytical calculations. He never regarded a successful device as definitive, and was always prepared to make modifications that could purify the investigated effect. This flexibility later turned out to be essential in his exploration of new, unexpected, effects. In sum, he combined Gauss and Weber's emphasis on precision and computability with Faraday's extraordinary capacity for mutating devices.

In the summer of 1879 Hertz undertook lengthy calculations to determine the best way to tackle the new Academy prize questions, and handed the resulting manuscript to Helmholtz. He imagined three possible tests. In the first, which Schiller had already tried in vain, the dielectric would alter the oscillating frequency of an *LC* circuit when placed inside the coil, as a consequence of the induced polarization. Hertz found the effect to be much too small to be measurable for the frequencies he knew how to produce.[66] The second test concerned the action of a magnet on a dielectric when the latter was submitted to the oscillating electrostatic force of a condenser. This seemed more feasible to Hertz, but required more work and apparatus than he could afford. The third and last possibility was to test electrostatically the polarization produced by an electromotive force of inductive origin, for instance when a dielectric sphere rotates in a magnetic field. In the latter case Hertz was worried that frictional electricity would mask the investigated effect.[67]

Altogether, Hertz judged that the prospects of a successful answer to the academy questions were weak as long as very rapid electric oscillations were not available. Profiting from Helmholtz's neglect of his manuscript, he set himself to a purely theoretical work: the determination of the currents induced in rotating conductors or dielectric spheres. He had already started the calculations in his manuscript on polarization, and he knew he could complete them in time for his habilitation.[68]

In his calculations of the electrodynamic effects of open dielectric currents, Hertz used Helmholtz's electrodynamics, on which he quickly became an expert. Most important, he absorbed the sharp distinction which this theory made between two different kinds of electromotive force, of electrostatic and electrodynamic origins. According to Helmholtz, these two kinds of force, having different causes, did not

[65] Hertz 1880a. Cf. Buchwald 1994: 59–74. Lorenz reached similar precision in Lorenz 1879: cf. Wiedemann 1885, Vol. 4: 1023–4. Hertz 1881 obtained a much lower limit with a different method, based on the inertial inflection of a current in a rotating conductor and akin to the suggestion in Maxwell 1873a: #577.

[66] Schiller 1874. The effect is proportional to R^2/λ^2, where R is the radius of the coil, and λ the wavelength of light at the given frequency.

[67] Hertz [1879]. Cf. O'Hara and Pricha 1987: 121–128; Buchwald 1994: 75–92.

[68] Hertz 1880b. Cf. Darrigol 1993b: 293–4; Buchwald 1994: 95–9.

necessarily have the same effects. By definition they had equal power to drive currents in conductors; but they did not need to be equivalent with respect to dielectric polarization. In his 1870 memoir Helmholtz had explored the possibility that polarization and conduction, being both motions of electricity with respect to matter, responded to the same causes. But the truth of this possibility was an open question, precisely the second question of the Academy prize.

6.4.2 The unity of the electric force

During the three years following his habilitation, Hertz shared his time between the theory of elasticity, the evaporation of liquids, and cathode rays. He did not return to fundamental electrodynamic issues until 1884, when his new appointment at Kiel left him more time for theoretical meditation than he wished. At that time he reflected on the singularity of Helmholtz's distinction between electromotive forces of electrostatic and electrodynamic origins. According to Maxwell's and Weber's theories, polarization and conduction had to occur under both types of force, because they were essentially the same thing: shifts of the electric fluids for Weber, varying strains of the medium for Maxwell. Hertz remarked that in general electrodynamics would be much simpler if 'electric forces which emanate from inductive actions [were] in every respect equivalent to equal and equally directed forces of electrostatic origins.' This was his 'principle of the unity of the electric force,' of which he proceeded to examine the consequences as follows.[69]

According to the principle, an electric force of inductive origin, for example that of a variable magnet, should act on an electrically charged body as electrostatic forces do (this action was too weak to be observed in Hertz's time). According to the principle of action and reaction, this implies that a static charge should act on a variable magnet. Applying a second time the unity of the electric force, the static charge may be replaced by a variable magnet. We thus get a new kind of interaction between two variable magnets. In order to separate this interaction from the usual magnetostatic one, Hertz considered two closed ring magnets of variable intensity, or two closed solenoids fed with variable currents, for which the new force is the only one left. Calling with Hertz the time derivative of the magnetic polarization 'a magnetic current,' the new force is to magnetic currents what Ampère's forces are to electric currents.[70]

Hertz had no illusions about the experimental possibility of detecting this new mechanical force. Instead he examined which modification of the known electrodynamics of closed currents would integrate the new force in a manner compatible with the energy principle (see Appendix 8). Following Helmholtz's example, he introduced a potential from which the new mechanical force derived. Energy is conserved if a new 'magnetic induction force' corresponds to the time derivative of the new potential. In turn, this modification of the magnetic force implies a change in

[69] Hertz 1884. Cf. D'Agostino 1975: 284–96; Kaiser 1981: 164–75; Darrigol 1993a: 243–50; Buchwald 1994: 177–202.
[70] Hertz 1884: 297–9.

the magnetic current, and so forth. Summing up the infinite series of corrections, Hertz found that the final electric and magnetic forces derived from the vector potential **A**, such that (in electrostatic units)

$$\Delta\mathbf{A} = -\frac{4\pi}{c}\mathbf{j} + \frac{1}{c^2}\frac{\partial^2\mathbf{A}}{\partial t^2}. \tag{6.25}$$

He concluded: 'The vector-potentials now show themselves to be quantities which are propagated with finite velocity—the velocity of light.' He noted that Riemann and Lorenz had proposed the same equation, but claimed that only he had shown its necessity on plausible principles.[71]

The unity of the electric force implied a formal symmetry between electric and magnetic currents, and it ruined the physical division between electrostatic and electrodynamic components of the electric force. Calculations in terms of the potential **A** hid both the symmetry and the indivisibility. Hertz therefore eliminated the potentials to write the perfectly symmetric equations (in electrostatic units, and in the absence of sources):

$$\frac{1}{c}\frac{\partial\mathbf{H}}{\partial t} = -\nabla\times\mathbf{E}, \quad \frac{1}{c}\frac{\partial\mathbf{E}}{\partial t} = \nabla\times\mathbf{H}. \tag{6.26}$$

The system of force given by these equations, Hertz noted, was just the same as Maxwell's. Starting from the known electrodynamics for closed currents, and completing it in conformity with the principle of the unity of the electric force, the principle of action and reaction, and the energy principle, Hertz had reached the Maxwell equations in their modern form. Since in his opinion every electrodynamicist implicitly admitted the unity of the electric force, Maxwell's system was shown to be superior, and other theories 'carried in themselves the proof of their incompleteness.'[72]

The argument was extremely ingenious: there may be no more dazzling combination of thought experiments and general principles in the whole of nineteenth century physics. Hertz's 1884 paper was commonly referred to and admired in subsequent electrodynamic literature, German, French, and English. Yet there were some weaknesses in Hertz's reasoning. Hertz noted one of them: one could no more derive Maxwell's equations from the known electrodynamics of closed currents than one could derive electromagnetic induction from the existence of electrodynamic forces, because unsuspected forms of energy could be involved in the interplay of forces. Hertz only claimed to have applied the energy principle in the most natural way, that is, through an extension of Helmholtz's notion of potential. At the same time he emphasized that his derivation of the attraction between magnetic currents 'directly depended on the premises' (the unity of the electric force and the equality of action and reaction), and sufficed to show the

[71] Hertz 1884: 301–10; 310 (quote). [72] Hertz 1884: 311–14; 314 (quote).

superiority of Maxwell's theory, since the latter was the only one to contain this interaction.[73]

The 1884 article had other flaws of which Hertz was not aware. That Maxwell's theory contains the attraction between magnetic currents is only half true. Even in the most complete form later derived by Hertz and Heaviside, Maxwell's theory does not contain an attraction between variable closed solenoids. The reason is that the principle of action and reaction cannot be applied to matter alone in this case: the Hertz force, $\mathbf{D} \times \dot{\mathbf{B}}$, serves to increase the momentum of the ether within the solenoids. For closed ring magnets, however, the force acts on the matter of the magnets, in conformity with Hertz's conclusions.[74]

Another difficulty, noted by Boltzmann and his student Eduard Aulinger, concerns an apparent contradiction in Hertz's paper. At the beginning Hertz suggests that Weber's theory complies with the unity of the electric force for it makes electrostatic and electrodynamic actions 'special cases of one and the same action at a distance emanating from the electric particles.' Yet he later asserts that no theory except Maxwell's contains the attraction between magnetic currents. As a matter of fact, Weber's theory does not contain the Hertz force, because it implies the validity of Ampère's force law, even for variable currents. Therefore, it cannot possibly comply with the unity of the electric force. This can be seen directly in the interaction between two charged particles: according to Weber's fundamental law, the actions of a moving particle and a particle at rest on another particle at rest can be equal without their actions on another moving particle being equal.[75]

Perhaps because he became aware of these difficulties, Hertz never explicitly referred back to his 1884 article in his later writings. He even relinquished to Heaviside the priority for the symmetrical form of Maxwell's equations. Furthermore, he conceived his famous experiments of the years 1886–1887 in terms of Helmholtz's theory instead of Maxwell's. This silencing of the 1884 article has been commonly interpreted as a temporary return to a more skeptical attitude toward Maxwell. In reality, Hertz never ceased to favor the unity of the electric force, and remained convinced that Maxwell's theory had the highest probability.[76]

Hertz returned to the Helmholtzian framework because he shared Helmholtz's belief that even the most probable theories require experimental proof. In the Helmholtzian framework, Maxwell's theory appeared as a special limiting case, and the basic experimental objects were defined without presuming the validity of Maxwell's theory. Therefore, crucial experiments could easily be formulated to decide between Maxwell's theory and other options compatible with already known

[73] Hertz 1884: 313, 314. [74] Cf. Darrigol 1993a: 247–8.

[75] Aulinger 1886; Boltzmann 1886a; Also Lorberg 1886, 1887. Cf. Darrigol 1993a: 244–5; Buchwald 1994: 203–8. A last difficulty: the series which Hertz used to arrive at the wave equation for the vector potential does not exist for progressive waves. This seems to undermine Hertz's derivation of the retardation of electrodynamic actions: cf. Havas 1966. However, the flaw can be mended by assuming that the differential equation for the potential should be generally valid, even if it has only been deduced for stationary systems of force.

[76] On Hertz adopting the Helmholtzian framework, cf. d'Agostino 1975; Cazenobe 1980, 1982, 1983; Doncel 1991; Buchwald 1994.

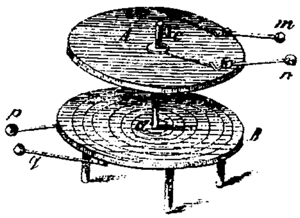

FIG. 6.7. The Riess spirals (from Eisenlohr 1870: 716).

facts and principles. Such was the purpose of the Berlin prize question, which Hertz still kept in a corner of his mind.

6.4.3 *Fast oscillations*

In the fall of 1886, Hertz found a pair of Riess spirals in the physics cabinet of the Karlsruhe Technische Hochschule, and used them to show his students the currents induced in one spiral by the discharge of a condenser in the other. This was a standard experiment since Riess's and Knochenhauer's systematic studies of electrostatic discharge currents in the early 1850s. Typically, a battery of Leyden jars was first charged to a high tension and then discharged through the primary spiral, so that a spark appeared between the terminals of the secondary spiral. The spirals were flat and had only a few turns, in order to prevent parasitic sparking between successive turns (Fig. 6.7). While preparing his lecture demonstration, Hertz made a surprising observation that marked the beginning of a great scientific adventure. Information on the nature of the stimulating surprise being scarce, I propose a reconstruction based on Hertz's background knowledge of spark discharges.[77]

I assume that for the purpose of classroom visibility Hertz used a Ruhmkorff coil to charge his Leyden battery. A Ruhmkorff coil is an induction coil with few turns in the primary, many turns in the secondary, a core of iron threads, and a periodic electromechanical interruptor in the primary (Fig. 6.8). At each interruption of the primary current, a high, impulsive electromotive force of induction is produced in the secondary. The secondary is usually connected to a discharger in which the impulses of the coil produce spectacular sparking.[78]

Hertz used a common kind of discharger, Henley's 'universal discharger' (Fig. 6.9) made of two horizontal copper rods ending in copper knobs (among other possibilities) on the gap side, and in rings or spheres on the outer side. The rods could slide horizontally through tubes attached to the vertical glass columns and connected

[77] Hertz 1892a: 2. On the Riess spirals, cf. Riess 1853, Vol. 2: 277–80; Wiedemann 1985, Vol. 4: 187; Müller and Pouillet 1888–1890, Vol. 3: 853. For the chronology of these experiments, cf. Hertz 1977: 212–14, starting with: '4 Oct.: Experiments on the induction by the discharge of a [Leyden] jar.'

[78] On Ruhmkorff coils, cf., e.g., Wiedemann 1885, Vol. 4: 338–59. Helmholtz had frequently used induction coils in his physiological and electrical researches. Hertz had used Ruhmkorff coils in his experiments on electric discharge in rarefied gases.

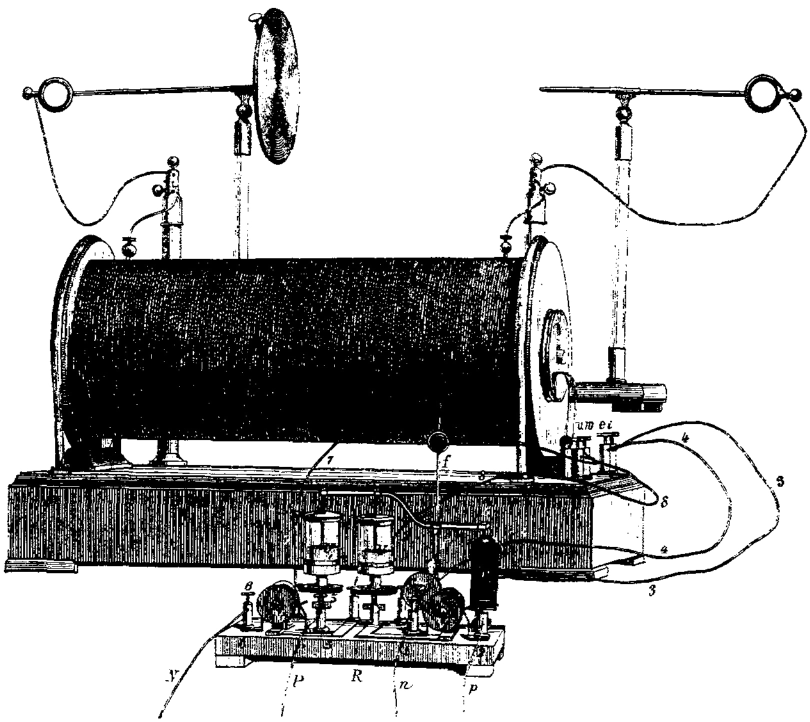

FIG. 6.8. A large Ruhmkorff coil, with a mercury interruptor at the front, and a point-disk discharger behind (Eisenlohr 1870: 783).

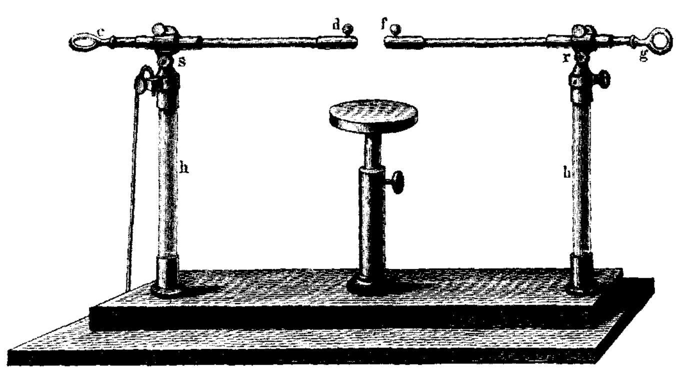

FIG. 6.9. The universal discharger (from Wiedemann 1885, Vol. 1: 142).

to the high-tension source (condenser, electrostatic machine, Ruhmkorff coil, etc.). The outer rings or spheres were there to hold the rods better. Their rounded shape avoided side sparking.[79]

With the Ruhmkorff coil and the universal discharger, Hertz could repeatedly charge his Leyden battery to a potential determined by the gap length.[80] The arrangement is shown in Fig. 6.10(a). As soon as the spark occurs, the Leyden battery discharges through the gap and the primary Riess spiral, and an electromotive force is induced in the secondary spiral. For a large capacitance, Hertz expected that the strong sparking in the discharging gap would quickly reduce its electric resistance to a small value. Then the discharge was fast or oscillatory, and the desired sparking occurred between the terminals of the secondary spiral. For a small Leyden jar, however, Hertz expected that the resistance of the primary spark would impede secondary sparking. He was surprised to observe that such was not the case. He even obtained secondary sparking with no jar at all (Fig. 6.10(b)). Since in that case the quantity of electricity furnished by the coil was very small, only an extraordinarily rapid discharge could explain the sparking in the secondary spiral.[81]

Sparking discharge, as with anything visually spectacular, was a thriving research field, and several physicists had already observed cases of side sparking. Hertz was the only one, however, to pursue systematically the 'singular property' of the electric spark. He remembered that he needed very rapidly varying currents to answer the Berlin prize question. Accordingly, he modified his arrangement in ways that would more directly exhibit the rapidity of the spark discharge. Possibly, he switched to the arrangement of Fig. 6.10(c), in which the electromotive force of induction in the primary spiral is directly tested. Then he replaced the spiral with a variable length of wire W, and the secondary spark gap with a Riess micrometer, that is, a gap whose length can be adjusted down to very small values—in which case the spark must be observed in the dark (Fig. 6.10(d)). Even with a short and thick copper wire, small sparks occurred in the micrometer. The latter experiment is the first described in Hertz's paper on rapid electric oscillations.[82]

The resistance of the wire, Hertz reckoned, was not sufficient to explain the potential difference at the ends of it. Self-induction had to do this, which implied that the electric disturbance had to be shorter than the time taken by it to travel the length of the wire according to Kirchhoff's or Thomson's theory of propagation

[79] On the universal discharger, cf., e.g., Wiedemann 1885, Vol. 1: 142. Hertz referred to this discharger in Hertz 1887a, and quite explicitly in 1887b: 71. It appears in a picture taken by Hertz and reproduced in Bryant 1988: 23. The little round table was used to expose various chemicals to the sparks.

[80] A similar arrangement, with an electrostatic machine in place of the induction coil, was described in the very popular textbook of a former Carlsruhe Professor: Eisenlohr 1870: 717, and also in Jamin and Bouty 1878–1883, Vol. 4: 207. According to R. Appleyard, who was familiar with electric laboratory techniques at the turn of the century, the arrangement (with induction coil and discharge gap) was the standard method of operating the Riess spirals (Appleyard 1930: 118).

[81] This is my interpretation of Hertz 1892a: 2: 'I had been surprised to find that it was not necessary to discharge large batteries [of Leyden jars] through one of the [Riess] spirals in order to obtain sparks in the other; that small Leyden jars—even the impulse of a small induction coil—sufficed for the purpose, provided the discharge had to spring across a spark gap.' (The English translation in Hertz 1893: 2 is incorrect.)

[82] Hertz 1887a: 33.

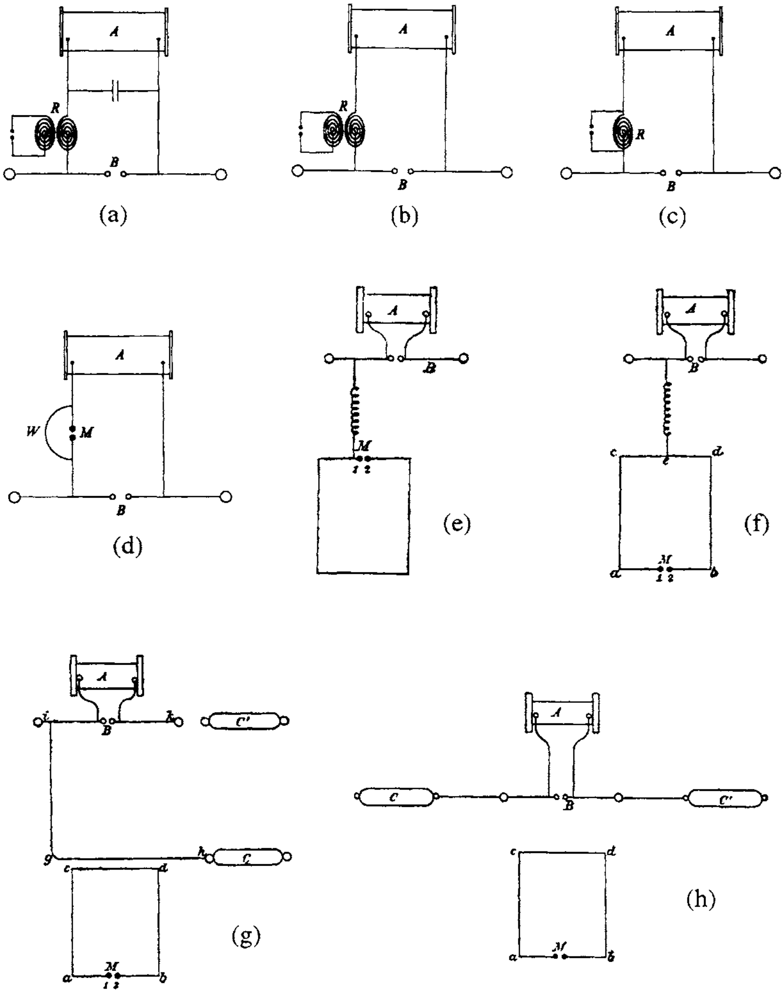

FIG. 6.10. From the Riess spirals to Hertz's oscillator ((e), (f), (g), (h) from Hertz 1887a: 34, 37, 40, 43). *A* denotes a Ruhmkorff coil, *B* a discharger, *R* a Riess spiral, and *M* a Riess micrometer.

along wires. In order to confirm this view, Hertz used the device of Fig. 6.10(e). In this configuration, the side-sparking could only be interpreted 'in the sense that the change of potential proceeding from the induction coil reached the knob 1 in an appreciably shorter time than the knob 2.'[83]

Accordingly, the sparking should disappear when the distances traveled by the disturbances to reach the knobs 1 and 2 are equal. Hertz checked this with the

[83] Hertz 1887a: 33–4.

arrangement of Fig. 6.10(f). He then hooked a piece of wire to one of the knobs and observed that the sparking reappeared. This suggested to him that the disturbance was reflected at the end of the wire and returned to the knob with a delay. If such reflections occurred, Hertz further reasoned, the side-circuit acted as an oscillator under impulsive excitation. Then its symmetry could be broken by a dissymmetrical change of the parameters determining the frequency of oscillation. Hertz verified this by touching one of the knobs with an insulated sphere, thus adding capacity to one of the branches.[84]

In these experiments the side-circuit was excited by a wire connection with the discharger. If, however, the self-induction of a short piece of wire was sufficient to produce high potential differences, then the same had to be true for the mutual induction between two short pieces of rectilinear wires. Hertz indeed observed sparking in the side circuit of Fig. 6.10(g), which is excited by mutual induction between gh and cd. He could enhance the sparking by connecting a large insulated conductor C (taken from an electrostatic machine) to the wire end h. This modification increased the charge accumulated at h and discharged through the wire hgi and the spark gap B.[85]

At that point Hertz suspected that the discharging circuit behaved as a high-frequency oscillator, just as the side-circuit did. For this reason, he brought another large insulated conductor C′ in contact with the end k of the discharger. The increased sparking at M proved that the discharges in the two portions of the discharging circuit were related, in conformity with the existence of electric oscillations. Hertz now understood the essential virtues of his coil–discharger system. The coil serves to charge the capacity of the discharger (mainly that of the two conductors C and C′ when they are present) until the sparking tension is reached at the gap B. Most unexpectedly for Hertz, the spark suddenly brings the resistance of the air gap to zero, and starts an oscillating discharge of the capacity through the self-inductance of the connecting wire.[86]

In this light, it was clear that the conductors C and C′ increased the intensity of the oscillation (but lowered its frequency), and that with respect to the oscillation the wire pair ki–gh played the role of a continuous linear conductor. Hertz therefore adopted the simpler arrangement of Fig. 6.10(h), in which the conductors C and C′ are three meters apart and the wire two millimeter thick. In this configuration he could observe the inductive action when the distance between the side cd of the side-circuit and the wire of the oscillator was as large as 1.5 m.[87]

Hertz also tried a linear side-circuit: a piece of straight wire with a gap in the middle and two spheres at the extremities. He realized that in this case the

[84] Hertz 1887a: 36–9. [85] Hertz 1887a: 39–41. Cf. Buchwald 1994: 227–9.

[86] Hertz 1887a: 41–3. Cf. Buchwald 1994: 230–1. The self of the coil has an exceedingly large impedance for the oscillatory discharge current. However, the coil also has a capacity corresponding to the polarization of its successive layers, as first described in Helmholtz 1869b: 535. This capacity is negligible compared with that of CC′ but not compared with that of the bare discharger (Hertz did not discuss this point).

[87] Hertz 1887a: 43–4. Hertz then regarded the oscillations in the loop-shaped side-circuit abcd as entirely due to electromagnetic induction. Cf. Buchwald 1994: 231–2.

electrostatic induction from the conductors C and C′ to the spheres of the side-circuit also contributed to the sparking. He believed he could shunt this action with a wet thread accross the gap and be left with something never previously observed: the pure electrodynamic action of two open currents. Knowing how to please, he announced to Helmholtz:

I have succeeded in demonstrating quite visibly the induction effect of one open rectilinear current on another rectilinear current, and I may hope that the way I have now found will in time enable me to solve one or another of the questions connected with this phenomenon.

He meant that the determination of the parameters of Helmholtz's theory (k and κ_0) was now close at hand.[88]

By varying the capacities and self-inductions of the primary and secondary circuits, Hertz obtained a broad but distinct resonance phenomenon. This confirmed the oscillatory character of the discharge, and gave a means of optimizing the response of the side-circuit. In later experiments the side-circuit was always tuned to obtain the best sparking. Hertz also identified nodes in his side-circuits, but was not yet able to obtain multiple nodes and measure the wavelength of his oscillations. In order to estimate the period, he had recourse to theory and applied the standard formulas for the capacitance of a sphere and the self-inductance of a wire. For spheres of 15 cm radius, and a wire of 150 cm in length and 0.5 cm in diameter, he found 3.54×10^{-8} second, which was a hundred time smaller than the smallest period obtained by Feddersen with Leyden jars. The corresponding wavelength, according to Kirchhoff's or Thomson's theory of propagation in wires, was the length traveled by light during a period, that is, 10.62 m. Hertz noted that this length was also 'the wavelength of the electromagnetic waves which, according to Maxwell's view, are supposed to be the external effect of the oscillations.'[89]

As Hertz was soon embarrassed to learn, he was not the first to have identified high-frequency oscillations in the spark discharge of an induction coil. Wilhelm von Bezold had already done so in a little-known work of 1870. However, Hertz was the first physicist to have sufficient control of the conditions of the oscillations, as well as a relatively convenient means to detect them. He owed this achievement to 'careful attention to insignificant details' combined with continual recourse to subtle theoretical reasoning. He regarded the laws of the motion of electricity in conductors as essentially known, with the indetermination formalized in Helmholtz's k. Nevertheless, the specific electric motions of his devices depended on unpredictable properties of the electric spark. He selected and amplified surprising aspects of side-sparking in a long series of mutations of his original arrangement. The successive changes were not only informed by known electrical laws, but also by conjectures

[88] Hertz 1887a: 44–5; Hertz to Helmholtz, 5 December 1886, in Hertz 1977, and Süsskind 1995: 107. The reasoning with the wet cord is flawed because the shunt only eliminates the low-frequency component of the electrostatic action.

[89] Hertz 1887a: 46–50 (resonance), 50–4 (knots), 54–8 (theory). Hertz's 'Schwingungsdauer' and 'Wellenlänge' are half the period and half the wavelength. In this paper Hertz did not explicitly refer to Kirchhoff's and Thomson's theory of electric propagation in wires, although he obviously used it several times. Cf. Buchwald 1994: 233–9.

on the role of the spark. Hertz began with the idea of a very sudden electric disturbance. He ended with an oscillatory discharge that he could manipulate, compute, and detect.[90]

From the beginning of his experiments, Hertz knew that not every primary spark was able to induce side-sparking. By trial and error, he determined the optimal size of the gap and the spherical knobs. As he tells us, 'the most insignificant details, often without any apparent connection, resulted in useless sparks.' Efficient sparks had to be 'brilliant white, slightly jagged, and sharply cracking.' Moreover, the size of side sparks depended on their being in view with the primary spark. Hertz judged the latter effect to be worth a separate study. He determined that the ultraviolet light of the primary spark, and no electric perturbation, was responsible for the enhancement of the side spark. He thus discovered what we now call the photo-electric effect.[91]

6.4.4 Answering the Berlin prize question

Hertz's main ambition was still to answer the Berlin prize question on dielectric polarization. At the high frequencies he now knew how to produce, the detection of the inductive effects of dielectric currents seemed an easy matter. In the summer of 1887 Hertz experimented with the device of Fig. 6.11, in which the dielectric block BB (sulfur or paraffin) was submitted to the high-frequency electric force of the plates AA′. He could not yet reach a definite conclusion, because strong sparking occurred in the side-circuit C, whether or not the block was present. He gradually became aware that for the high frequencies he was using, the quasi-closed circuit C no longer behaved as a selective indicator of electromagnetic induction. The difference of electrostatic potential at the small gap could be large enough to cause sparking.[92]

In September 1887 Hertz examined the behavior of a tuned circular side-circuit in the vicinity of his oscillator, both theoretically and experimentally. In Helmholtzian terms, the side-circuit is submitted to two actions, of electrostatic and electrodynamic origins. To a first approximation, the sparking depends on the circulation of the total electromotive force $\mathbf{E}$ along the wire of the side-circuit.[93] This is equal to the circulation α of the electrodynamic part of $\mathbf{E}$ on an imaginary completed circuit, minus $\mathbf{E}\cdot\delta\mathbf{l}$, where $\delta\mathbf{l}$ is the vector joining the two ends of the gap. Consequently (neglecting the variation of $\mathbf{E}$ in $\mathbf{E}\cdot\delta\mathbf{l}$), the sparking should vary like $|\alpha +$

[90] Bezold 1870; Hertz 1887a: 53. Bezold produced Lichtenberg dust figures (which depend on the nature and intensity of the electric perturbation) at different points of a long wire. Cf. Hertz 1892a: 2–3; 59–68 (reproducing part of Bezold 1870).

[91] Hertz 1887a: 35; Hertz 1887b. Cf. Buchwald 1994: 244.

[92] Hertz 1892a: 4–5. I follow the chronology of Hertz's experiments carefully established by Doncel 1991, and adopt his and Buchwald's conclusions based on the recently discovered laboratory notebooks (published in Hertz and Doncel 1995). Hertz's first evidence for retarded action occurred much later (December 1887) than suggested by Hertz 1892a.

[93] This is not so obvious for a resonant circuit. Hertz reasoned in more general terms. Cf. Buchwald 1994: 245–54.

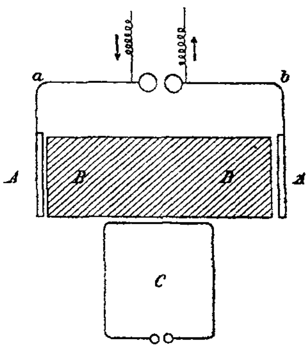

FIG. 6.11. Tentative arrangement for testing the inductive effect of a dielectric current (Hertz 1892a: 5).

$\beta\cos\theta\sin\omega$|, where β is a constant proportional to the intensity of the total electric force, ω the angle between the vector **E** and the axis of the side-circuit, θ the angle between the gap and the projection of **E** in the plane of the side-circuit. The direction of **E** is experimentally determined by the direction of the axis of the side-circuit for which a rotation of the side-circuit in its own plane does not alter the sparking ($\omega = 0$). In this case the sparking is entirely due to the circulation α. For other directions of the circuit's axis, and near the oscillator, the sparking has two minima and two maxima. For a vanishing circulation ($\alpha = 0$), the minima are diametrically opposed and occur when the gap is perpendicular to the electric force. If the electrodynamic circulation does not vanish, the two minima are no longer opposed, and the two maxima have unequal intensity.[94]

Hertz was now ready for the Berlin prize problem. He used the balancing device of Fig. 6.12, in which the conductor C and the dielectric D can be both submitted to the variable electrostatic force of the plates A and A′. The side-circuit B is perpendicular to the horizontal plane of the plate A and A′, and its axis goes through the primary spark gap. Consider first the behavior of the side-circuit when C and D are away. When the micrometric gap f is in one of the diametrically opposed positions in the plane AA′, there is no sparking, which means that the electrostatic and the electrodynamic actions on the circuit both vanish. The sparking is maximum at the highest and lowest positions of the gap, which means that the electrostatic force is horizontal. If the side-circuit is slightly shifted downwards in its plane, the zero-sparking positions of the gap are slightly rotated downwards; the sparking increases at the highest position of the gap, and it diminishes at the lowest position. This is the signature of an electrodynamic action.[95]

Hertz then brought the side-circuit back to its original position, and approached the conductor C from above. The observed changes in the sparking were exactly

[94] The results (with different justifications) are in Hertz 1887c and 1888b.
[95] Hertz 1887c: 103–6.

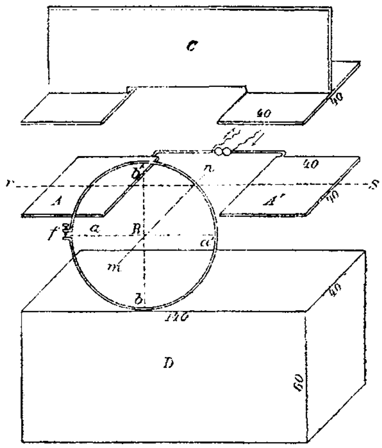

FIG. 6.12. Hertz's apparatus for comparing the inductive actions of a conduction current and a dielectric current (Hertz 1887c: 104).

opposite to the ones observed when shifting down the side-circuit, which meant that C acted as a current running opposite to that of the oscillator. This was just what Hertz expected, for he knew that the conductor C, regarded as an oscillator, was below resonance, and that its forced oscillations were therefore in phase with the source. He now removed C, and placed the dielectric D below AA′. The sparking pattern changed exactly as it would for a conductor below resonance, which meant that the dielectric current had the same electrodynamic action as an ordinary current. Hertz confirmed this result by balancing this action with that of C on the other side. After carefully eliminating any possible ambiguity, he concluded that the first Berlin prize question had to be answered in conformity with Faraday's and Maxwell's views on (material) dielectrics.[96]

6.4.5 *Electrodynamic propagation*

In order to completely demonstrate the superiority of Maxwell's theory in the Helmholtzian framework, two points remained to be established: that dielectric polarization could be produced by electrodynamic means (the second academy question), and that vacuum itself had exceedingly high polarizability. Judging that a separate proof of these points was too difficult, in November 1887 Hertz jumped to a test of the major conclusion that Helmholtz had drawn from them: electromagnetic induction would need a finite time to propagate. He meant to compare the phase

[96] Hertz 1887c: 108–13. Cf. Doncel 1991; Buchwald 1994: 254–61. Hertz (1887c: 112) also gave a more direct proof that the action of D is electrodynamic: he made D much larger than the oscillator, and positioned it so that its upper face touched AA′ and one of its vertical faces was in the vertical plane passing through the symmetry line rs. Then the sides are made of electric lines of force, except where they touch A and A′; hence by Green's theorem D cannot modify the electrostatic force outside its own mass.

of the electric action propagated along a stretched wire with that of the direct action of the oscillator.[97]

Hertz first demonstrated waves on wire with the device of Fig. 6.13. The wire starts from the plate P, which is parallel to the plate A of the oscillator. It curves around from m to n and then runs straight horizontally, in a plane passing through the spark gap. For progressive waves, Hertz used a 70 meter wire ending in the earth outside the building. For standing waves, he kept the wire end free and tuned its length so as to obtain the most distinct nodes. He observed the electric force near the wire with the side-circuits B and C, centered on the horizontal baseline rs drawn from the spark gap. In the stationary case he obtained periodic variations of the sparking when the side-circuit was moved along the base line (with the same orientation). This periodicity gave a half wavelength of 2.8 m, and a velocity of 200 000 km/s (the computed period of the present oscillator being 2.8×10^{-8} s), in rough agreement with earlier measurements of the velocity of electricity along wires. Hertz regarded the latter number only as an estimate, since he doubted the accuracy of his calculation of the oscillator's frequency.[98]

Hertz then proceeded to compare the phase of a progressive wave along the wire and the phase of the direct action of the oscillator. For this purpose, he used orientations of the side-circuit in which the actions through the wire and through the air were superposed. At any distance from the oscillator the two actions can be made of comparable magnitude by adjusting the distance between the plates P and A. Hertz's measurements are best explained in terms of three fundamental orientations of the side-circuit, as shown in Fig. 6.14. First suppose that the oscillator is acting alone (without the wire). In orientation 1, the electric force from the oscillator is perperpendicular to every part of the side-circuit, so that there is no sparking. In orientation two, the electric force is parallel to the gap, and there is strong sparking. This force is mostly electrostatic near the oscillator and mostly electrodynamic far from the oscillator.[99] In orientation 3, the electric force is perpendicular to the gap. Yet there is some weak sparking due to its finite circulation around the loop. In this case the effect is purely electrodynamic.

In order to superpose the wire and the air actions, Hertz originally used orientations of his side-circuit intermediate between 1 and 2. Then the wire contributes to the sparking in a proportion varying with the deviation from orientation 2 and with the distance between the plates A and P. Hertz favored such orientations because the contribution of the oscillator to the sparking was then strong enough to allow interference up to 8 m from the oscillator. However, they had the disadvantage of mixing the electrostatic and electrodynamic effects of the oscillator, which according to Helmholtz's general theory propagated at a different velocity. To his disappointment, Hertz found that the interference between air and wire actions varied with the same spatial period as the wire wave. This meant that the propagation in

[97] Hertz 1892a: 7–8; 1888a: 115. Cf. Buchwald 1994: 262–6.

[98] Hertz's laboratory notes, as analyzed in Doncel 1991 and Buchwald 1994: 266–76. Also Hertz 1888a: 119–22.

[99] The electrostatic force varies like $1/r^2$, the electrodynamic force like $1/r$.

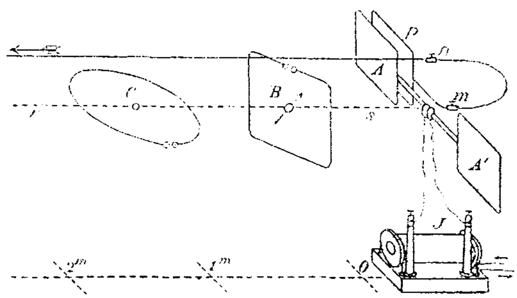

FIG. 6.13. Hertz's device for demonstrating waves in wires (Hertz 1888a: 116).

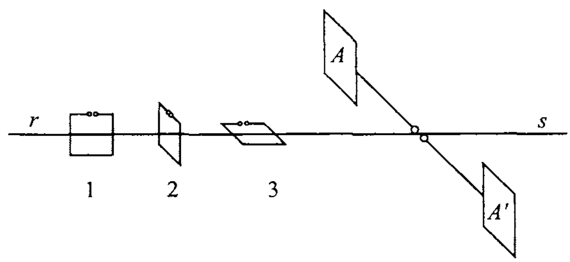

FIG. 6.14. Configurations of Hertz's side-circuit.

air from the oscillator was instantaneous, and indirectly that the air had negligible polarizability.[100]

On 22 December, Hertz repeated this experiment with more careful preparation, but failed again to find retardation of the air action. The next day he tried orientation 3 of the side-circuit, in which the wire action is solely superposed with the electrodynamic action of the oscillator. Although in this case interference could be obtained only up to 4 or 5 meters, it was enough to indicate a spatial periodicity much larger than that of the wire waves. Then the velocity of propagation of the inductive action had to be finite in the air. With great excitement, Hertz soon extended his previous measurements in configurations 1–2 up to 12 m and confirmed the finite velocity of the electrodynamic action. His previous failure now appeared to be due to the dominance of the electrostatic action at smaller distances from the oscillator. By 27 December he was speaking of 'induction waves' or 'air waves.'[101]

The details, however, did not agree with Maxwell's theory. Hertz found that the velocity of the electrodynamic action was 60 per cent larger in the air than in the wire, whereas for a wire made of a good conductor like copper Maxwell's theory

[100] Cf. Doncel 1991; Buchwald 1994: 269–76.
[101] Cf. Doncel 1991: 22, and Buchwald 1994: 289–98 for the original experiments; Hertz 1888a: 125–31 for the published account.

implied the equality of the two velocities. Hertz first tried to justify this difference by a finite value of k in Helmholtz's theory. This could only fail. Helmholtz had already shown that as long as the radius of the wire was a small fraction of the wavelength, the propagation in the wire was independent of k. Moreover, a finite k implied the possibility of two kinds of air waves with different velocities, whereas experiments had only shown one kind. Lastly, the electromagnetic theory of light required that the velocity in air should be equal to the velocity of light; then the polarizability of air (vacuum) had to be infinite or extremely large; in this case Helmholtz's theory is empirically equivalent to Maxwell's, as long as k is not exceedingly high. By the summer of 1888 Hertz had given up any recourse to Helmholtz's k, and he instead hoped that some minor modification of Maxwell's theory would explain the slower velocity of wire waves.[102]

6.4.6 Constructing Maxwellian waves

Hertz's more immediate concern was to give a direct proof of the existence of air waves. He worried that his experiments on the interference of air and wire waves were too complex to carry sufficient conviction. In February 1888 he thought of exploiting the effect of conducting masses on the waves. He had been aware of such effects for several months, but had treated them as perturbing effects to be eliminated. He now realized that metal sheets could be used to reflect the waves and produce standing waves. Within a few days he was able to locate the corresponding nodes and antinodes with a side-circuit, and to confirm his previous value for the wavelength in air (9 m). He published these results in July 1888, with an endorsement of Maxwell's theory: 'Clearly, the experiments amply justify the theory of electrodynamic phenomena that Maxwell first built on Faraday's views.'[103]

Certainly Hertz had performed and analyzed his experiments within the Helmholtzian framework, with a clear-cut distinction between electrostatic and electrodynamic forces. For instance, the experiment on the inductive effect of dielectric polarization presupposed a careful elimination of the electrostatic effect. Also, the proof of finite propagation in air concerned the *electrodynamic* action of the resonator, separated from the electrostatic one. In Helmholtz's theory, this proof only meant that the polarizabilily of air or vacuum had to be large. Hertz nonetheless preferred an infinite polarizability because it permitted the electromagnetic conception of light, and because in this case Helmholtz's theory could be replaced by Maxwell's simpler theory. Since 1884 he had been convinced that Maxwell's theory, with the characteristic unity of the electric force, was the most probable one, and he was ready to adopt it as soon as sufficient experimental indications existed in its favor.

[102] Hertz 1889a: 169 (playing on k); Helmholtz 1870b: 551; Hertz 1889a: 169 (two kinds of waves). That Hertz was aware of the last argument (invoking the high polarizability of the ether) is not certain. As a modification of Maxwell's theory, Hertz proposed to drop the continuity of the parallel component of **E** at the surface of the wire.

[103] Hertz 1888c: 145–6. Cf. Buchwald 1994: 299–310.

In the fall of 1888 Hertz used his version (6.26) of Maxwell's field equations to compute the field of his oscillator in the dipolar approximation, for which the dimensions of the oscillator are negligible compared with the wavelength. With great elegance he derived the now well-known radiation formulas, and drew the corresponding field diagrams with the help of his wife. At large distances from the oscillator the field is that of a purely transverse wave traveling at the velocity c. Most strikingly, the electric and magnetic forces decrease as the inverse of the distance, whereas for a constant dipole they would decrease respectively as the cube and the square of the distance. To this field corresponds, according to Poynting's formula, an outward energy flux proportional to the square of the dipole's strength and to the fourth power of its oscillation frequency.[104]

Having thus characterized the radiation field, Hertz proceeded to reinterpret his propagation experiments in terms of Maxwell's electric and magnetic fields, instead of the Helmholtzian electrostatic and electrodynamic forces. He succeeded reasonably well, except for the interference measurements in orientation 3: in this case the computed phase variation near the resonator was much smaller than what he had measured. Ironically, these were the very measurements from which Hertz had first suspected a finite propagation velocity in air.[105]

With this theoretical study, Hertz completed the construction of the concept of electric waves. He had first shown the finite-velocity propagation of electromagnetic induction, then manipulated the waves with reflectors, and finally determined the essential characterics of their development in space on the basis of Maxwell's theory. There was only one flaw in this harmonious blend of facts and theory. The velocity of wire waves appeared to be smaller than that of air waves, against the predictions of Maxwell's theory. In July 1889, Oliver Lodge noted that Hertz had miscalculated the frequency of his oscillator by a factor $\sqrt{2}$. This made the wire velocity nearly equal to the velocity of light, and the air velocity 60 per cent larger than that of light. Had he trusted the new value of the frequency, Hertz would have suspected his measurements of the wavelength in air. He did not, however, because the frequency calculation was based on formulas that had only been established for slowly varying currents. Hertz only started to doubt his measurements in 1891, well after he had found that the discrepancy disappeared for shorter waves. His complete retraction came in 1893, after Edouard Sarasin and Lucien de la Rive had repeated Hertz's long-wave experiments in the great hall of the Rhone waterworks at Geneva and found complete agreement with Maxwell's theory. Most likely, Hertz's earlier wavelength measurements had been perturbed by reflections on the walls of the lecture room in which he was experimenting.[106]

[104] Hertz 1889a. Cf Buchwald 1994: 304–21.

[105] Hertz 1889a: 164–5. Cf. Buchwald 1994: 320–1 for the irony.

[106] Lodge and Howard 1889: appendix, and also Poincaré to Hertz, undated, Deutsches Museum, #3001; Hertz to Lodge, 21 July 1889, in O'Hara and Pricha 1987: 93 (no trust in frequency formula); Hertz 1889b (short waves); Sarasin and de la Rive 1893. Cf. Hertz 1892a: 9–11 (doubts); 1893: 14n (retraction); O'Hara and Pricha 1987: 5–6; 17–18 (FitzGerald and Trouton involved); Fölsing 1997: 443–9.

6.5 The impact of Hertz's discovery

Hertz's discovery soon caught the attention of physicists all over the world. The production of electric waves by electric means was an enormous claim. The experiments could easily be repeated: Ruhmkorff coils were a very common device, and the rest of the apparatus could be made from copper wires and foils within a few hours. There were difficulties with the adjustment of the primary spark and the observation in the dark of the much weaker secondary sparks, but Hertz had given enough intructions to overcome them with some patience. By 1889 many physicists had confirmed Hertz's results, except sometimes for the velocity difference in air and wires. As generally occurs in 'repetitions,' improvements were made, for instance alternative detectors, or Lecher's double wires, along which better-defined standing waves were produced; side-discoveries were announced, for example multiple resonance by Sarasin and de la Rive; and conflicts ensued about their proper interpretation.[107]

6.5.1 British enthusiasm

Regarding the theoretical significance of the discovery, British physicists were the first to react publicly. FitzGerald called Hertz's experiments 'a splendid verification of Maxwell's theory' even before the reflection paper was published. Presiding in September the mathematics and physics section of the British Association meeting in Bath, he announced: 'The year 1888 will ever be memorable as the year in which this great question [whether electric forces are propagated through a medium] has been experimentally decided by Hertz in Germany, and I hope, by others in England.' He went on, explaining how Hertz's experiments proved the existence of the electromagnetic ether. His eloquence transformed the meeting into a posthumous triumph for Maxwell.[108]

In reality, Hertz's discovery was no surprise for FitzGerald, who had earlier reflected on the electric production of electromagnetic waves. This was true for the Maxwellians in general. Lodge perceived Hertz's experiments as an improvement on his own experiments on wire waves. Heaviside remarked to Hertz: 'I have been long familiar myself with waves in dielectrics that your experimental result I take without surprise almost as a matter of course.' Heaviside quickly noted, however, that it was 'very different with many people.' Only he and his few Maxwellian friends had fully grasped the implications of Maxwell's theory on electric propagation, which were only implicit in Maxwell's text. This is why FitzGerald was so eager to advertize Hertz's discovery as Maxwell's triumph. As he must have fore-

[107] Lecher 1890; Sarasin and de la Rive 1890. Cf. Hertz 1892a: 13, 17–19; O'Hara and Pricha 1987: 6–7; Fölsing 1997: 438–43 (multiple resonance, which Alfred Cornu used to discredit Hertz); Poincaré 1904a for an excellent popular review of early works on Hertzian oscillations. About a recent repetition of Hertz's experiments, cf. Buchwald 1994: 163–6.

[108] FitzGerald to Hertz, 8 June 1888, in O'Hara and Pricha 1987: 23–4; FitzGerald 1888: 231. Cf. Hunt 1991a: 158–9.

seen, Maxwellian physicists immediately gained much wider attention. They became the heroes of a new era of British physics.[109]

In this early reception of Hertz's experiments, the possibility of practical applications played scarcely any role. Hertz and FitzGerald, and most other physicists, were only concerned with the fundamental meaning of the new discovery. Misled by the analogy between light and electric waves, they could not imagine that Hertzian telegraphy would be any better than optical telegraphy. In fact, the later success of wireless transmission depended on several unpredictable circumstances, the availability of an extremely sensitive detector (the coherer) and the ability of the waves to travel around obstacles, cross clouds and even defy the curvature of the Earth. At the turn of the century, the spectacular success of Guglielmo Marconi's system helped to widen and accelerate the diffusion of Maxwell's theory. Scientists as important as Lodge, Poincaré, and Cohn were involved in this technological adventure. Yet the stir had little effect on the contemporary evolution of fundamental electrodynamics—and therefore need not be discussed in the present book.[110]

6.5.2 Hertz's Maxwell

Whereas in England Hertz's discovery only changed the relative importance of an already established theory, in Germany it dramatically upset received conceptions. Hertz himself pioneered a new kind of theory that resulted from the clash between German and British conceptions. His first step was the already mentioned calculation of the field of an oscillating electric dipole. There he wrote Maxwell's equations directly in terms of the electric and magnetic fields, but without the source terms. He was only concerned with propagation in vacuum from a point source, or along a cylindrical wire of large conductivity. He did not discuss the nature of electric charge and current, and used uncritically expressions such as 'free quantities of electricity' or 'waves in wire' that betrayed the persistence of continental concepts in his mind.[111]

Hertz became more of a Maxwellian in the following year, 1889, after corresponding with FitzGerald and Heaviside and experimenting on the skin effect of his wire waves. He understood that according to the Maxwellian view 'the electric force that determines the current is not at all propagated in the wire itself, but under all circumstances penetrates from outside into the wire and spreads into the wire with comparative slowness and according to laws similar to those of temperature change in a heat conductor.' He appropriated Heaviside's remark that the velocity along wires was better determined in the case of two parallel wires: the electromagnetic wave is then guided between the two wires, whereas in the case of

[109] Hertz to Heaviside, 13 July 1889, in O'Hara and Pricha 1887: 66. On Lodge's reaction, cf. *ibid.*: 87–8, and *supra*, Chapter 5, p. 205. On FitzGerald's strategy, cf. Hunt 1991a: 160–2.

[110] Cf. Aitken 1985; Poincaré 1904; Fahie 1899; Appleyard 1930. On Marconi and his competitors, cf. Hong 1994b, 1996.

[111] Hertz 1889a: 152, 165.

a single wire the motion of the wave partly depends on unspecified, remote conductors. Hertz thus assimilated the central Maxwellian dogma of the primacy of field processes.[112]

Yet Hertz, like most of Maxwell's continental readers, failed to understand Maxwell's pictures for electric displacement and current. As he wrote in 1892, 'Many a man has thrown himself with zeal into the study of Maxwell's work, and, even when he has not stumbled upon unwanted mathematical difficulties, he has nevertheless been compelled to abandon the hope of forming himself an altogether consistent conception of Maxwell's ideas. I have fared no better myself.' In particular, Hertz did not see how Maxwell's polarization, being a 'displacement of electricity,' could be directed from the positive to the negative plate of a condenser. He overlooked that Maxwell's 'electricity' was electrically neutral, that charge only meant a discontinuity in the strains implied by the displacement of 'electricity.' Out of despair, he decided that Maxwell's pictures belonged to the 'gay garment with which we arbitrarily clothe nature.' After discarding hypothetical fluids, displacement, and potentials, he declared: 'To the question "What is Maxwell's theory?" I know of no shorter or more definitive answer than the following: Maxwell's theory is Maxwell's system of equations.'[113]

In 1890 Hertz offered his own systematic exposition of Maxwell's theory, an impressive model of epistemological order and clarity. He first admitted the existence of the electromagnetic ether and characterized its state by the electric forces **E** and **H**, operationally given by the forces acting on unit electric and magnetic poles.[114] He assumed the energy density

$$w = \frac{1}{2}\varepsilon E^2 + \frac{1}{2}\mu H^2, \qquad (6.27)$$

where ε and μ are the 'dielectric constant' and the 'magnetizing constant.' In Hertz's absolute units these constants are dimensionless, and their value is *one* in the case of a vacuum. Then Hertz posited the field equations for bodies at rest:

$$\frac{1}{c}\frac{\partial \mu \mathbf{H}}{\partial t} = -\nabla \times \mathbf{E},$$
$$\frac{1}{c}\frac{\partial \varepsilon \mathbf{H}}{\partial t} = \nabla \times \mathbf{H} - \frac{\sigma}{c}\mathbf{E}. \qquad (6.28)$$

where c is a constant with the dimension of velocity, and ε/σ is the relaxation time of the electric force.[115]

[112] Hertz 1889c: 172; Heaviside to FitzGerald, 30 January 1889, in O'Hara and Pricha 1987: 39; Hertz to FitzGerald, 20 July 1891, *ibid.*: 47.

[113] Hertz 1893: 20, 28, 21; Hertz 1890a: 208–10. Cf. Heimann 1971; Buchwald 1985a: 192–3; Darrigol 1993a: 251–7.

[114] In the presence of matter, Hertz placed his poles in Thomson's infinitely narrow cylindrical cavities.

[115] Hertz 1890a: 210–220. For simplicity I give the formulas for isotropic media only, and I rationalize Hertz's units.

Hertz regarded these propositions as a complete foundation for the electrodynamics of bodies at rest. He did not try to deduce them by *a priori* means, nor to found them separately on experiments: 'Each separate formula,' he explained, 'cannot be proved by experiment, but only the system as a whole.' At that stage, the concepts of charge and current were not yet defined, neither formally nor operationally. Hertz introduced them only in a second part, as 'names and definitions' which did not add anything to the empirical content of the theory. Their sole purpose was 'to permit more concise expression and partly to permit connections with the older views of electricity.' The electric and magnetic 'polarizations' **D** and **B** were defined as $\varepsilon\mathbf{E}$ and $\mu\mathbf{H}$; the 'true electricity' as $\nabla\cdot\mathbf{D}$, the 'free electricity' as $\nabla\cdot\mathbf{E}$, the 'true magnetism' as $\nabla\cdot\mathbf{B}$, the 'free magnetism' as $\nabla\cdot\mathbf{H}$, the 'electric current density' as $\sigma\mathbf{E}$.[116]

Hertz thus renounced the Weberian, substantial, view of electricity in favor of Maxwell's view that charge and current derived from field concepts. However, for Hertz the derivation of charge and current concepts was purely formal, whereas for Maxwell it rested on a specific picture. This difference explains why, for instance, two definitions of electric charge could coexist in Hertz's system, whereas Maxwell tolerated only one.[117] Also, Maxwell gave much importance to the physico-mathematical distinction between flux and force (quantity and intensity), whereas Hertz insisted that the vectors **D** and **B** corresponded to alternative descriptions of ether states already defined by the vectors **E** and **H**.[118]

Hertz's system further differed from Maxwell's on the issue of mechanical foundation. Hertz ignored the Lagrangian derivation of electrodynamic equations, and regarded the omnipresence of the vector potential in Maxwell's *Treatise* as a 'rudimentary phenomenon of a mathematical nature.' This attitude is a little surprising, considering that in the same period Helmholtz was trying to subsume all physics under Hamilton's principle. It should be remembered, however, that Hertz's argument of the unity of the electric force and his later concern with propagation placed the forces **E** and **H** in the foreground of the theory. Moreover, Hertz was not completely satisfied with the Lagrangian formulation of mechanics, as is seen in his own later attempt at refounding mechanics.[119]

6.5.3 *Hertz's electrodynamics of moving bodies*

Hertz dispensed with the Lagrangian method even for the determination of the mechanical forces acting on charge or current carriers. Energetic considerations sufficed to determine these forces from the field equations as soon as the motion of the medium was taken into account. Hertz proved this in a sequel to his reformulation of Maxwell's theory, on the basis of the two following assumptions:

[116] Hertz 1890a: 210, 223, 224–32.
[117] Hertz 1890a: 227–8. Maxwell also introduced the divergence of **E**, but only as the 'apparent electricity,' a mathematical intermediate with no physical significance (Maxwell 1873a: #83).
[118] Hertz 1890a: 224; 1890b: 258.
[119] Hertz 1890a: 209. Hertz 1894: 22–9 (against Hamilton's principle).

1. The velocity of the ether is continuous and is identical with the velocity of matter whenever matter is present.
2. The lines of force corresponding to the polarizations **D** and **B** follow the motion of the medium. More precisely: if these lines were under the influence of this motion only, they would always pass through the same particles of the medium.

Hertz knew that the first assumption had been discredited by Fizeau's interferometric experiment of 1851: the drag of the ether by running water was only partial. For the second assumption, he had no *a priori* justification, since he had discarded any picturing of **D** and **B**. He satisfied himself that the two assumptions led to a formally complete theory, whose predictions agreed with all know electrodynamic experiments.[120]

In order to derive the fundamental field equations, Hertz adopted the notion of convective variation of a flux which Helmholtz had invented to determine the forces acting on a three-dimensional current (see Appendix 5). In a moving medium, the variation of a flux **F** at a given particle of the medium is given by the variation at a fixed point of space, minus the convective variation. In symbols, this gives

$$\frac{\mathrm{D}\mathbf{F}}{\mathrm{D}t}=\frac{\partial \mathbf{F}}{\partial t}-[\nabla\times(\mathbf{v}\times\mathbf{F})-\mathbf{v}(\nabla\cdot\mathbf{F})], \tag{6.29}$$

where **v** is the velocity of the medium. With this notation, Hertz's general field equations simply read:[121]

$$\begin{aligned}\frac{1}{c}\frac{\mathrm{D}\mu\mathbf{H}}{\mathrm{D}t}&=-\nabla\times\mathbf{E},\\ \frac{1}{c}\frac{\mathrm{D}\varepsilon\mathbf{E}}{\mathrm{D}t}&=\nabla\times\mathbf{H}-\frac{\sigma}{c}\mathbf{E}.\end{aligned} \tag{6.30}$$

The velocity-dependent terms are of two kinds. The terms with the double vector product lead to Heaviside's motional forces $\mathbf{v}\times\mathbf{B}$ and $\mathbf{D}\times\mathbf{v}$. The former force yields the electromagnetic induction in a body moving in a magnetic field. The latter explains an effect first demonstrated by Wilhelm Röntgen in 1885–1888: the action on a magnetic needle of a non-electrified dielectric disk rotating between the plates of a condenser. The remaining velocity-dependent terms correspond to the convection current $\mathbf{v}(\nabla\cdot\mathbf{D})$ already introduced by FitzGerald and Heaviside and confirmed by Rowland's experiment with the electrified rotating disk.[122]

[120] Hertz 1890b: 256–9.

[121] Hertz 1890b: 259–63; Helmholtz 1874a: 730–4. Cf. Darrigol 1993b: 318–24, 338–40.

[122] Hertz 1890b: 263–5, 274–5; Röntgen 1885, 1888, 1890. In 1903, Alexander Eichenwald showed that the measured value of the latter effect complied with Lorentz's theory, for which only a fraction of **D** rotates with the disk: cf. Whittaker 1951: 400.

In order to determine the mechanical forces of electric and magnetic origins, Hertz examined the variation of the electromagnetic energy of a volume element of the moving medium. He found two contributions: the Poynting flux across the surface of the element, and terms $\sigma_{ij}\partial v_j/\partial x_i$ corresponding to the work of Maxwell's stresses σ_{ij} during the deformation of the element (see Appendix 6). In conformity with the concept of contiguous action through the ether, Hertz derived all electromechanical forces from these stresses. He retrieved Maxwell's relevant formulas, with minor corrections and additions. The main addition was an old friend of Hertz, the force $\mathbf{D}\times\dot{\mathbf{B}}$ that acts on variable ring magnets.[123]

With this electrodynamics of moving bodies Hertz brought Maxwell's theory to the highest degree of formal perfection. Yet he knew that his assumptions about the relationship between ether and matter were too simple to be true. They contradicted well-known results of the optics of moving bodies. Moreover, they implied a mechanical force $\partial(\mathbf{D}\times\mathbf{B})/\partial t$ (the Hertz force + Maxwell's $\dot{\mathbf{D}}\times\mathbf{B}$) that could act on the ether even in the absence of matter. This meant a violation of the equality of action and reaction when applied to matter alone. That the ether would have inertia and carry momentum seemed very unlikely to Hertz, even though he made it carry energy and stresses.[124]

6.5.4 Hertz and Heaviside compared

There are obvious similarities between Hertz's and Heaviside's versions of Maxwell's theory. Both physicists criticized Maxwell's notion of electricity as an incompressible fluid; they avoided mechanical pictures of field processes in general; they discarded the Lagrangian foundation of the field equations; they eliminated the potentials; they emphasized the electric–magnetic symmetry; and they insisted on local energy balancing. Initially, Heaviside suspected that Hertz had used more of his works than he admitted in print. The suspicion quickly vanished, for Hertz soon gave Heaviside credit for the duplex equations (1885) and let his own contribution of 1884 be forgotten. In turn Heaviside admired Hertz's electrodynamics of moving bodies for its dispensing with Hamilton's principle. Heaviside's most systematic and detailed analysis of Maxwellian field energetics, published in 1991, relied on Hertz's notion of the fully dragged ether (see Appendix 6). Naturally, Heaviside shared Hertz's worries about the optics of moving bodies.[125]

Nevertheless, Hertz's electrodynamics was too formal and too abstract for Heaviside's taste. Whereas Hertz cared only for formal completeness and empirical adequacy, Heaviside required a dynamical foundation on generalized forces, velocities, and 'activities.' To him the displacement **D** and the induction **B** had central physical significance, for their time variations gave the generalized velocities. Accordingly, he reproached Hertz for eliminating the coefficients ε and μ from the

[123] Hertz 1890b: 275–84. The Hertz force is formally identical to Heaviside's magneto-electric force.
[124] Hertz 1890b: 284–5.
[125] Hertz 1890a: 209–10 (priority to Heaviside); Heaviside 1891–1892. Cf. Buchwald 1985b; Darrigol 1993b: 324–7; Yavetz 1995: 133n.

field equations in vacuum: 'Can you conceive of a medium for el. mag. disturbances which has not at least *two* physical constants, *analogous* to density and elasticity? If not, is it not well *to explicitly symbolize them*, leaving to the future their true interpretation?' Heaviside's ether was a genuine mechanical medium, even though its exact mechanical make-up was unknown. In particular, this medium could provide for the momentum that Hertz deplored missing in his electrodynamics of moving bodies.[126]

6.5.5 German Maxwellians

With his modernist abstraction, Hertz did not quite resemble a British Maxwellian, not even the one who was the least inclined to models and pictures. Yet his renunciation of the old German views was very thorough. He rejected the electric fluid or fluids, and regarded the ether field as the primary concept from which all other notions derived. This revolutionary fever was contagious. After Hertz's experiments, the number of German physicists interested in Maxwell's theory grew spectacularly. Some of them produced systematic expositions of the theory adapted to a German audience. With no loss of empirical adequacy, they could have remained in the framework of action at a distance and used Helmholtz's reinterpretation of Maxwell's theory in terms of an infinitely polarizable ether. None of them did. They all adopted Maxwellian field monism.[127]

Not even Helmholtz cared to defend his own polarization theory. In his 1892 lectures on the electromagnetic theory of light, he started with the Maxwell–Hertz equations, and noted that electric charge, like energy, was conserved without being a substance. Unlike Hertz, he greatly admired Maxwell's use of the Lagrangian method. Since the 1880s he had wanted to subsume all physics under the principle of least action. This principle was a sufficient condition of mechanical reducibility, and shared the energy principle's virtue of being expressible directly in terms of physically controllable variables. In it Helmholtz saw a powerful instrument for structuring and extending theories. In 1892 he succeeded in finding an action function for the Maxwell–Hertz equations in the most general case, including moving bodies. A few weeks before his death, in September 1894, he was still working on a simplification of the variational procedure.[128]

In 1891–1893 Boltzmann published his Munich lectures on Maxwell's theory. Despite his exceptional sympathy for British physics and his deep understanding of Maxwell's vortex model, Boltzmann agreed with Hertz that Maxwell's concepts of charge and current were irremediably obscure. In his lectures he adopted Hertz's

[126] Heaviside to Hertz, 8 December 1890, in O'Hara and Pricha 1987: 80–1; Heaviside to Hertz, 13 September 1899, *ibid.*, and Heaviside 1893–1912, Vol. 1: 108 for forces acting on the ether (cf. Darrigol 1993b: 327). A more technical difference between Hertz and Heaviside concerned the way of inserting impressed forces in the field equations: Heaviside's way reflected his dynamical viewpoint.

[127] Cf. Darrigol 1993a.

[128] Helmholtz 1897 (1892–1893 lectures): 92–3, 99; Helmholtz 1886, 1892, [1894]. Cf. Klein 1972a; Darrigol 1994b: 235–7.

view that electricity was a 'thing of thought, serving to picture [*versinnlichen*] the integrals of certain equations.' But he did this reluctantly, and expressed his hope that the mystery of electric motion would soon be solved—that his own lectures would soon be obsolete. He also criticized Hertz's operational definitions of **E** and **H**, and 'his predilection for nudity.' In his opinion the theory could not be formulated without reference to mechanics. He offered a dynamic foundation for the field equations, and a wardrobe of elaborate mechanical models for the main electrodynamic phenomena. For a sample, the reader may take a look at Fig. 6.15.[129]

Subsequent German expositors of Maxwell's theory payed lip service to Boltzmann's mechanical illustrations, but no doubt found them more complicated than the *explanandum*. They adopted Hertz's minimalism, or returned to the source, Maxwell's *Treatise*. Paul Drude, a disciple of Voigt and a reader of Ernst Mach, belonged to the phenomenological tradition and appreciated Hertz's economical way: 'It seems to me that, for the apprentice, the economy brought by mathematics in the description of facts should prevail over presentations that try to meet the needs of the philosopher of nature, not those of the experimenter.' In his best-selling *Physik des Aethers* (1894), Drude shared Hertz's nominalism about charge and current, and distanced himself from mechanical foundations: 'From the observed facts one cannot deduce the necessity, nor the expediency of a mechanical representation.' In his view, the reverse reduction, from electromagnetism to mechanics, had an equal chance of success. Perhaps the ether itself was an unnecessary survival of mechanical reductionism: 'Just as well as we ascribe the mediation of forces to a particular, space-filling medium, we could also dispense with the medium, and attribute to space itself the physical properties currently attributed to the ether. Physicists have not yet considered the latter view, because by "space" they mean an abstract representation without physical properties.'[130]

August Föppl, an engineer–physicist, published an equally popular book on Maxwell's theory in the same year 1894. Föppl shared Drude's distaste for Boltzmann's mechanistic debauchery, but found Hertz's ascetism equally unattractive. His sources were Maxwell and his 'most outstanding follower,' Oliver Heaviside, who had reached 'the most immediate and clearest representation of Maxwell's concepts.' Föppl adopted Heaviside's vector notation, and some of his dynamical notions, especially the impressed forces. He had little interest in the Lagrangian formulation of the theory, and endorsed Mach's view that the origin of fundamental laws was empirical. Yet he gave a full and clear account of Maxwell's pictures of charge and current, including the notion of conduction as a relaxation of polarization, and the metaphor of the incompressible fluid. For anyone who might believe

[129] Boltzmann 1891–1893, Vol. 2: 23 (thing of thought), 13–14 (operational definitions); 1895: 413 (nudity). Cf. Kaiser 1982: 5*–32*; Darrigol 1993a: 257–64. Boltzmann's interest in Maxwell's theory antedated Hertz's experiments. In 1873 he measured the dielectric constants of various transparent bodies in Helmholtz's laboratory, and found that Maxwell's relation between optical index and dielectric constant was approximately verified: cf. Buchwald 1994: 208–14.

[130] Drude 1894: VII, VI, 9. Cf. Darrigol 1893a: 264–267.

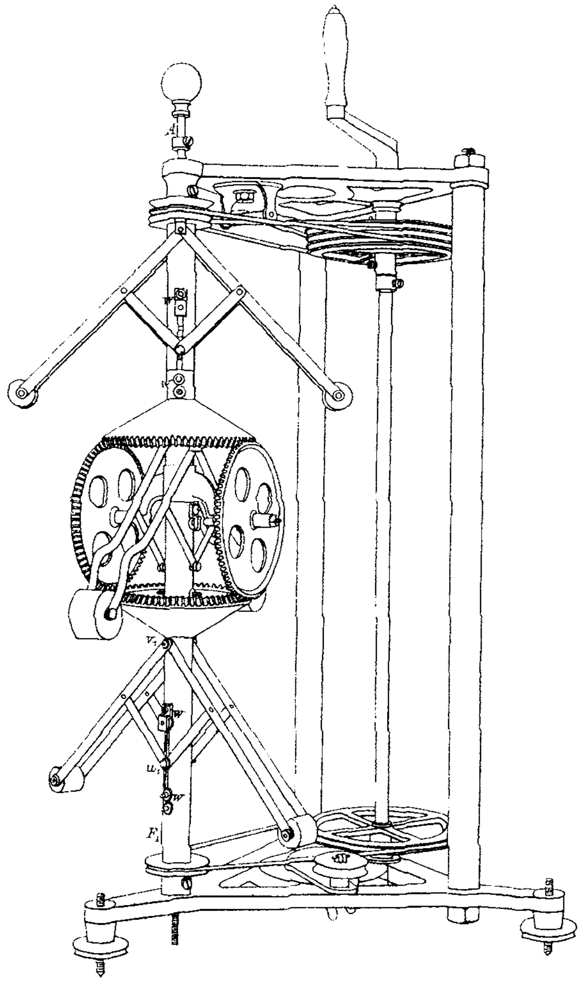

FIG. 6.15. Boltzmann's model for two coupled *RLC* circuits (Boltzmann 1891–1893, Vol. 1: plate).

that Maxwell's concept of electricity was too British to cross the Channel, Föppl is a clear counter-example.[131]

Another German Maxwellian source was Emil Cohn's *Das elektromagnetische Feld*, published in 1900. A phenomenologist and an admirer of Mach, Cohn remained as close as possible to experimental and engineering practices. He based the theory on the operationally defined fields **E** and **H** and on the electric and mag-

[131] Föppl 1894: V. Cf. Bromberg 1972; Holton 1973b; Darrigol 1993a: 267–71.

netic lines of force. Following Faraday, he defined electric charge as the ending or surging of electric lines of force. Inspired by his experimental study of the dielectric properties of bad conductors (with Leo Arons in 1886), he shared the Maxwellian view that a conductor was a leaking dielectric, and he identified the conduction current with the dissipative decay of electric lines of force. Cohn meant all of this to be purely phenomenological and avoided further picturing of field processes. He ignored Maxwell's electric displacement and the imaginary fluid just as he did the continental electric fluids. Most radically, he judged the concept of the ether to be superfluous. In his view an electromagnetic wave was not the propagation of a disturbance in the ether as previous Maxwellians would have said, but 'the propagation of the electromagnetic field.' The modern grammar of 'propagation' and 'field' was Cohn's invention. It became common usage after the *Encyklopädie der mathematischen Wissenschaften* had consulted Cohn on this matter.[132]

In sum, Hertz's discovery triggered a true intellectual revolution in Germany. None of the German Maxwellians tried to rescue Weber's substantial concept of electricity. They all adopted Maxwell's idea of the primitivity of the field concept and the derived character of charge and current. Some of them went so far as to accept the Maxwellian picture of conduction as a field relaxation process. The rest took an agnostic view, and refused to make pictures of charge and current. The continental and Maxwellian pictures of electricity had annihilated one another in their minds.

This radical break prompted epistemological reflection. Boltzmann moralized: 'It is certainly useful to set up Weber's theory as a warning example for all times that we should always preserve the necessary mental flexibility.' He emphasized the need of a plurality of approaches, including both mathematical phenomenology and diverse picture-based theories. Hertz grew suspicious of physical pictures, and ascribed to Maxwell's system of equations 'a life of its own.' This system, together with some operational definitions, was a self-sufficient whole unambiguously representing electromagnetic phenomena. Other notions, although they might call earlier pictures to mind, were nothing but 'names.' Drude and Cohn perceived a vindication of Mach's view that we should never forget the origins of our concepts and attribute to them a metaphysical inevitability. They insisted on a direct empirical anchoring of concepts, to the point of questioning the ether.[133]

These reflections reactivated the issue of mechanical reductionism. German physicists were now confronted with at least four conceptions of the mechanical foundation of electromagnetism: reduction to distance forces acting in pairs; reduction to contiguous actions; mechanical analogies *à la* Thomson; and the requirement that the fundamental equations should derive from the least-action principle. Drude, Cohn, and Föppl refused to make a choice, and denied that a mechanical

[132] Cohn 1900a; Cohn and Arons 1886. Cohn first developed his conception of the field in Cohn 1890. Cf. Darrigol 1993a: 271–6.

[133] Boltzmann 1904: 162; Hertz 1896 (1889): 318. Mach was less critical of the ether than his followers. He even speculated that the ether could solve the paradox of absolute motion: cf. Mach 1901: 241–2.

foundation was at all required. At the other extreme, Boltzmann forcefully defended the three last conceptions of a mechanical foundation, and was tormented by the failure of the second. Helmholtz selected the more delicate fourth possibility. And Hertz dreamt of a fifth: he planned a new mechanics that would purge the remnants of distance-action metaphysics and form a more suitable framework for electrodynamics. In short, the older, neo-Kantian physicists wished to maintain the foundational role of mechanics, while the younger, Machian ones made electromagnetism stand on its own feet.

6.6 Conclusions

By the 1860s continental electrodynamics had grown more conservative and academic. At the close of his physiological optics, Helmholtz decided to revive the dormant waters of German physics. His experience in physiology, his philosophy of the energy principle, and his openness toward British physics determined the originality of his approach. In his electrophysiological experiments, he was confronted with very rapid electrodynamic processes for which the available theories were silent or incompatible. With induction coils, Leyden jars, commutators, and frogs' legs he developed laboratory techniques that permitted a first step into the unknown field of open-ended currents. He answered the corresponding theoretical challenge with a general framework, including the predictions of previous theories as particular or limiting cases.

Such generality could only be obtained by renouncing detailed pictures for the electric current or the ether. Helmholtz borrowed Franz Neumann's notion of electrodynamic potential, and generalized it to a 'potential law' for pairs of currents elements. This law was ontologically neutral, had formal simplicity, and led to forces that automatically satisfied the energy principle. In the electric currents Helmholtz included polarization currents contributed by dielectrics and perhaps by vacuum itself. The most general theory that complied with known laws for closed currents involved only two unknown parameters: the constant k in the potential formula, and the polarizability of vacuum. Specific choices of these constants retrieved Weber's and Maxwell's laws for the motion of electricity in bodies at rest, despite the extreme disparity of the conceptual bases.

Helmholtz's theory played a major historical role by providing a framework for devising crucial experiments, both imaginary and real. The nature of these experiments changed in the course of time. In his fundamental memoir of 1870 Helmholtz showed that the Weber case ($k = -1$) and any negative value of k led to the instability of the equilibrium of electricity in conductors, and he determined that available experimental means could not decide among the other values of k. In response to subsequent criticism, he turned his attention to crucial experiments involving the motion of conductors: in this case his potential law implied forces unknown to other theories, unless the polarizability of vacuum was very large. In 1875 a clever experiment of his own—the first quantitative experiment ever done on open currents—

decided against the new forces. Then the potential law had to be given up, or vacuum had to be polarizable. Helmholtz preferred the latter possibility, which brought him closer to Maxwell's theory. He nevertheless called for a third kind of crucial experiment that would test the electrodynamic activity of polarization currents and their presence in vacuum.

Being based on the notion of electrodynamic potential, Helmholtz's theory was closest to Franz Neumann's and partly shared its phenomenological outlook. This kinship involves a spurious paradox: Neumann's phenomenology implied a highly rigid conception of experiment, whereas Helmholtz and his disciples constantly imagined new devices. The difference is easily understood by remembering that Neumann kept his theory incomplete whenever experimental facts were unavailable. In contrast, Helmholtz strove for completeness, and achieved it thanks to general organizing principles: the decomposition principle, the energy principle, and Hamilton's principle. He thus turned a narrow mathematical phenomenology into a physics of principles. Within the constraints imposed by the principles, he conceived several ways of completing the theory, and imagined new experiments to select from among these possibilities.

From Helmholtz, Hertz inherited the physics of principles with its Kantian overtones, the framework for crucial experiments, a few laboratory techniques adapted to quickly varying currents, and the Berlin prize question on polarization currents. His success in answering the latter question, and his subsequent demonstration of the finite propagation velocity of electrodynamic actions in air, also depended on more personal qualities. He was highly attentive to details that could grow into new effects. He was exceptionally able at transforming devices to purify and amplify the effects, analyse their causes, and put them to new service. He did not let his theoretical preferences inflect his results. This exploratory dimension of Hertz's experimentation reminds us of Faraday. Yet Hertz did not share Faraday's suspicion of mathematical theory. On the contrary, he frequently relied on advanced theory in the analysis and modification of his devices. Helmholtz's highly mathematical framework provided the necessary concepts, laws, and questions.

A long-debated issue is whether Hertz intended to confirm Maxwell's theory. The Berlin prize question of 1879 on dielectric currents was explicitly about the validity of Maxwell's theory. In addition, after 1884 Hertz believed that a theory directly based on Maxwell's field equations was most likely to be true, in the name of the unity of the electric force. However, he refused to close the issue prematurely, and sought a clear conception of how to empirically discriminate between Maxwell's theory (or an empirically equivalent one) and alternative theories. This is why he adopted the Helmholtzian framework for all his experimental researches of the years 1886–1887. The discovery of fast oscillations, the demonstration of the electrodynamic effects of a varying dielectric polarization, and the first proof of retardation in air (December 1887) all belong to this category; they were interspersed with temporary results that contradicted Maxwell's theory. Only after the experiments with reflecting screens (February 1888) did Hertz leave the Helmholtzian framework and adopt Maxwell's field equations. By that time he regarded Maxwell's theory as confirmed, despite the persistent difficulty with the velocity of waves in wires.

In the following months Hertz assimilated a basic tenet of Maxwell's system: the primacy of the electromagnetic field and the derived character of charge and current. Yet he ignored Maxwell's pictures and dynamical foundation. His highly abstract reformulation rested on two field equations, two operational definitions, and an energy formula. He treated the ether as a medium of unknown constitution, and introduced charge, current, and polarizations as useful 'names' for certain mathematical symbols. Some German writers on Maxwell's theory adopted Hertz's nominalism and even extended it to the ether. Others required dynamical foundations and pictures of the basic ether processes. But none tried to rescue the old substantial view of electricity. Helmholtz's attempt at reviving German physics had succeeded beyond hope.

7

Conduction in electrolytes and gases

7.1 Introduction

Around 1890, Hertz's waves occupied the forefront of electrodynamics. In this limited context, the deeper nature of the electric current, or the precise relation between ether and matter, did not need to be known. Maxwell's British and German followers could confine themselves to the macroscopic field phenomenology favored by Maxwell. Yet they expected the atomic structure of matter to play a role in electric conduction, in the magnetic properties of matter, and in a number of optical phenomena. On the continent, Weber had championed this belief much earlier. No one could ignore his microscopic explanations of magnetism, no matter how unpopular the electric fluids had become after Hertz's discovery. Even Clausius and Helmholtz, who favored a phenomenological approach to physics, could not dispense with atomistic considerations.[1]

There were essentially two conceptions of how atomism should be introduced in electrodynamics. According to the natural Maxwellian tendency, the molecules of matter acted on the ether by modifying its *mechanical* properties. This modification was itself to be described mechanically, without recourse to electric concepts. Electric charge and current were treated as macroscopic emergent concepts depending on the average properties of the modified ether. The charge of an atom or intramolecular currents were meaningless notions.[2]

Continental physicists held the opposite view. Weber pictured metallic conduction as a jumping of electric particles from molecule to molecule, magnetism as molecular currents, and atoms as systems of orbiting electric particles. The more sober Clausius and Helmholtz still admitted electrified molecules, and even atoms of electricity. This German difference persisted even after Hertz had shaken his colleagues' faith in the electric fluids.

[1] On Maxwell's theory and atomism, see Chapter 4: pp. 170–2; On Weber's atomism, cf. Chapter 3: p. 107; On Helmholtz and Clausius, see *infra*: pp. 268, 273.

[2] Examples of this view are in J. J. Thomson 1883b (see *infra*: p. 292), Larmor 1894 (see Chapter 8: p. 336), and in Lodge 1885b, 1889. A notable exception is FitzGerald's (difficult) conversion, in 1882, to the idea that 'the interactions between the molecules of matter and the ether are of the same character as the electromagnetic actions with which we are acquainted' (FitzGerald 1882: 101, quoted in Hunt 1991a: 40).

The continental approach to molecular electrodynamics was generally more successful than the Maxwellian one. In the late 1880s this superiority became evident. At the same time, Hertz's experiments seemed to confirm Maxwell's basic notions of charge and current. By 1895 several physicists managed to resolve the resulting tension, both in England and on the continent. This happened in two different contexts: the empirical studies of electrolytic and gaseous conduction, and the theoretical synthesis of optics and electromagnetism. The present chapter treats the former context, the next one the latter context.

The first section explains how German physicists came to understand the electrolytic current in terms of a convection of what we now call ions: molecule parts carrying integral multiples of a universal quantum of charge (it must be remembered that Faraday's original definition of ions was purely macroscopic). Then we will turn to a first long phase of the history of electric discharge in rarefied gases in which it was believed that the discharge current had nothing to do with electrolysis. The third section describes how Arthur Schuster introduced gaseous ions in the 1880s, how J. J. Thomson managed to reconcile them with Maxwellian views, and how these two physicists defined a new experimental microphysics. The last section is devoted to the studies of cathode rays and the discovery of the electron.

7.2 Electrolysis

Despite their depth and thoroughness, Faraday's researches on electrolysis had controversial aspects. They were only the starting point of a long, complex evolution. Retrospectively, it seems that Faraday's law, Ohm's law, and the laws of thermodynamics almost necessarily lead to modern dissociation theory. Historically, there was no such necessity: for a long time the students of electrolysis preferred to sacrifice one of these laws rather than give up established chemical dogmas. As late as 1885, Oliver Lodge perceived electrochemistry as a very confused field 'with the repulsive character attached to any borderland of science.' Surveying this jungle is no easier for the historian. The following section gives a few landmarks, emphasizing the physical aspects that were relevant to the general evolution of electrodynamics.[3]

7.2.1 *Wandering ions*

Faraday's views on electrolysis were based on an intimate connection between electric current and chains of decomposition and recomposition within the electrolyte. Faraday's law expressed this connection in a quantitative manner, by stipulating that

[3] Lodge 1885b: 723. There is unfortunately no authoritative history of electrochemistry. The richest sources are Ostwald 1896 and Wiedemann 1882–1885, Vol. 1: 183–298, 729–95 (galvanic cells), Vol. 2: 463–625 (electrolysis), 626–862 (electrode polarization), 863–933 (energetics), 924–1002 (theory). A convenient summary of parts of Ostwald's history is in Le Blanc 1896. Insightful historical remarks are also found in Hittorf 1878.

the amount of decomposition in an electrolytic cell was the same for a given quantity of electricity and was in proportion with chemical equivalents. As to the chemical nature of the products of decomposition or 'ions,' Faraday judged from the substances freed at the electrodes and from received chemical conceptions, according to which a salt was the combination of a metallic oxyd (alkali) and a nonmetallic oxyd (acid). In this view, in a solution of sulfuric acid the water, not the acid, was decomposed. In a solution of soda sulfate, the salt was decomposed into the soda and sulfuric acid found at the electrodes while the water was also decomposed to yield the emitted hydrogen and oxygen.[4]

In 1839 Frederick Daniell, a London chemistry Professor, convinced Faraday that the old theory of salts was not compatible with the law of electrochemical equivalents. He electrolysed a solution of sulfuric acid and a solution of soda sulfate in series in the same circuit, and found that the amounts of oxygen and hydrogen developed in the two cells were the same. Consequently, the simultaneous decomposition of water and soda sulfate could not occur in the second cell without violating Faraday's law. Daniell proposed that the true ions were sodium and a new 'sulfion' ion SO_4, and that no direct decomposition of water occurred. In this view the observed hydrogen and oxygen, as well as the alkali and acid, were only secondary products of the actions of sodium and sulfion on water. More generally, Daniell did not regard water as directly decomposable, and he attributed the conductivity of solutions entirely to the solute.[5]

In 1844 Daniell compared the variations of the concentrations of the solute near the two electrodes, and found the transference of the ions in the solution to be dissymetrical. To any believer in Grotthus's decomposition chains, this was a surprising result: it seemed obvious that the positive and negative ions could only migrate at equal and opposite velocities, for the jumping of their atoms from one molecule to the next had to be synchronized (see Fig. 7.1(a)). The anomaly remained unsolved for ten years, until Wilhelm Hittorf, of Münster, developed the proper set of ideas and techniques. With a simple diagram Hittorf explained that the obvious need not to be true: in Fig. 7.1(b) the atoms of the cathion (in black) are continually paired with the atoms of the anion (in white), even though the latter drift faster than the former. With ingeniously designed cells, Hittorf measured the transference number (velocity ratio) for a number of electrolytes, at various concentrations and temperatures. His results confirmed Daniell's identification of the ions in saline solutions, and the secondary role of the solvent.[6]

Originally, Hittorf believed that the migration velocities of the ions depended on their chemical affinity. This agreed with the received wisdom that electrolytic conduction involved chemical decomposition. Hittorf soon realized, however, that in this view a minimal electromotive force was necessary to overcome the chemical affinity, whereas the observed transference numbers did not depend on the electromotive

[4] *FER* 1: ##742–747 (January 1834). Cf. Ostwald 1896: 480–578, esp. 520–1.

[5] Daniell 1839, 1840. Cf. Ostwald 1896: 596–609.

[6] Daniell and Miller 1844; Hittorf 1853. Cf. Ostwald 1896: 609–11, 814–19; Wiedemann 1885, Vol. 2: 582–93.

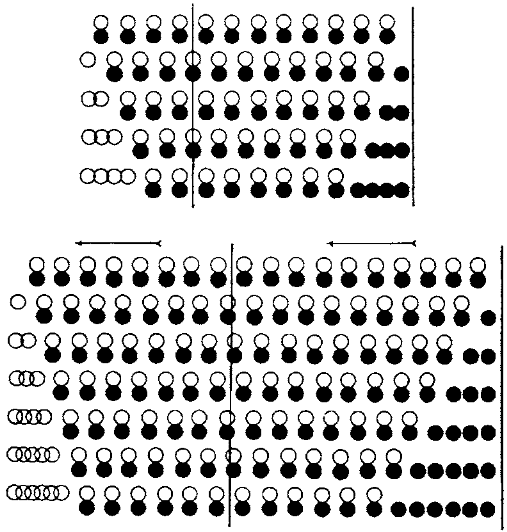

FIG. 7.1. Hittorf's diagrams for electrolytic decomposition (Hittorf 1853).

force, no matter how small it was. In 1856 he concluded that electrolytic conduction had nothing to do with chemical affinity and obeyed Ohm's law just as well as metallic conduction. This was a major, new insight: at that time it was commonly believed that Ohm's law did not apply to electrolytes, for a variety of reasons having to do with electrode polarization and secondary reactions. Measuring conduction by the transference of ions, Hittorf reached the opposite conclusion.[7]

Presumably inspired by Hittorf, in 1857 Clausius designed a theory of electrolysis that assumed the general validity of Ohm's law. If, Clausius reasoned *ab absurdo*, the molecules of electrolytes were at rest before a current was sent, then Ohm's law could not apply and a minimal electromotive force should be necessary to start the Grotthus chain of decomposition. Therefore one had to imagine an ongoing, erratic motion of the molecules, in conformity with Clausius's earlier kinetic theory of matter. In this picture collisions continually occurred between the molecules, sometimes resulting into the separation of charged 'molecule parts' (what we would now call ions). Such a part could in turn collide with the part of opposite sign of another molecule and combine with it, thus freeing another molecule part of the same sign. In the presence of an external electromotive force, the dissociating collisions became more frequent, due to the orientation of the polar molecules. Most important, this force implied a global drift of the molecule parts, which Clausius identified with the electrolytic current. All of this occurred no matter how small the electromotive force

[7] Hittorf 1856: 45–6; 1858: 52. Characteristically, Weber and Kohlrausch 1856 (#19) relied on the relation between resistance and affinity in their absolute determination of the mechanical force acting on the ions.

was, in conformity with Ohm's law. Conductivity had to increase with temperature, as had been observed on most electrolytes.[8]

When they are read today, the works of Daniell, Hittorf, and Clausius seem quite persuasive. They failed, however, to convince most contemporary experts in the field of electrolysis. Daniell's and Hittorf's views contradicted the old theory of salts, which still had strong supporters, for example Gustav Magnus. The separation of electrolytic conduction from chemical decomposition, advocated by Hittorf and Clausius, contradicted another dogma: the stability of compound molecules under normal physical circumstances, including fusion and dissolution. Electrolytic phenomena were so complex that numerous strategies could save these dogma, at least temporarily. In the decade following Hittorf's works, there were about as many theories of electrolysis as there were investigators in the field.[9]

This situation changed thanks to the support Wiedemann gave to Hittorf in his *Galvanismus* and when Friedrich Kohlrausch, the son of Rudolph and a brilliant student of Weber's, applied the spirit of the *Maassbestimmungen* to electrolytic conduction. The main obstacle to a direct resistance measurement was the long-known 'polarization' of electrodes, a cumulative surface phenomenon that depends on the quantity of electricity that has passed through the electrode. In general, the associated electromotive force and resistance prevent precise measurement of the resistance of electrolytic solutions. Kohlrausch's artifice was to use a quickly alternating current in the measuring bridge: then there is no time for polarization to build up, and nothing perturbs the resistance of the solution. Kohlrausch first verified Ohm's law, and measured the resistance of a large number of electrolytes at various concentrations and temperatures.[10]

Kohlrausch's most important conclusion, published in 1876, was 'the independent migration of the ions': in a sufficiently dilute solution the contribution of the two ions to the conductivity are mutually independent and additive. His empiricist attitude deterred him from an atomistic interpretation of this law. However, he used his mentor's absolute units and Hittorf's transference numbers to determine the absolute velocity of the ions when submitted to a given electromotive force. Physicists could now astonish chemists by revealing them wonders of the unseen: a potential slope of 1 volt/cm urged the hydrogen ion at 1 cm/hour through nearly pure water, the sodium ion at 13 mm/hour, the chlorine ion at 20 mm/s. . . .[11]

Kohlrausch's views were universally accepted in Germany. They were less successful in England. In his British Association report of 1885, Lodge rejected the

[8] Clausius 1857. Cf. Ostwald 1896: 869–1; Wiedemann 1882–85, Vol. 2: 941–2; Whittaker 1951: 335–6.

[9] On the conflict between Hittorf and Magnus, cf. Hittorf 1859 and Ostwald 1896: 830–5, 840–58. On other theories, cf. *ibid.*: 819–24, 830–40, 858–68; Wiedemann 1882–1885, Vol. 2: 935–41.

[10] Wiedemann 1863, 1874; Kohlrausch and Nippoldt 1869; Kohlrausch and Grotrian, 1875; Kohlrausch 1876a. Cf. Ostwald 1896: 884–97. Wiedemann performed important research on electrolysis. His 1852 studies on endosmose (electrolytically induced flow of a fluid through porous bodies) were useful to Hittorf. In 1856 he confirmed some of Hittorf's results, and showed that the velocity of the ions depended on the viscosity of the fluid. Cf. Wiedemann 1882–1885, Vol. 2: 167–8 (endosmose), 589–93 (on Hittorf), 946–947 (viscosity).

[11] Kohlrausch 1876b: 143; 1879: 4, 197–207; *ibid.*: 206, and Lodge 1889: 87 for the numbers.

German proofs of the independent migration of ions. In conformity with his beads-and-string model of the electrolytic current, he offered an alternative theory in which the velocities of the two ions were always equal and opposite. Even the general validity of Ohm's law for electrolytes seemed suspicious to the Maxwellians. In 1886 FitzGerald still thought it worth experimenting on this point.[12]

A young Swedish physicist, Svante Arrhenius, took Kohlrausch's conductivity measurements very seriously. In the late 1880s, he concluded from them that the molecules of dilute solutions were almost completely dissociated. He explored the consequences of this assumption with the help of Jacobus van t'Hoff's recent gas theory of solutions. Wilhelm Ostwald and van t'Hoff championed the dissociation theory as the editors of the *Zeitschrift für physikalische Chemie*, founded in 1887. In the next 15 years their ideas gradually conquered the field of electrochemistry, despite the persisting resistance of many chemists and physicists.[13]

7.2.2 Energetics

The above works illuminated what Hittorf called 'the wandering of ions.' Other electrolytic studies discussed the fate of ions at the electrodes from an energetic viewpoint. Helmholtz pioneered this approach in his memoir of 1847 on the conservation of force. He assumed that the Joule heat produced in a galvanic circuit could only originate in the chemical reactions at the electrodes, or in the polarization of the electrodes. For a non-polarizable galvanic cell (a Daniell or a Bunsen cell) placed in a resistive circuit, the electromotive force is a constant, and it is numerically equal to the mechanical equivalent of the Joule heat developed after one unit of electricity has gone through the circuit. Helmholtz equated this heat with that of the chemical transformations occurring in the cell. So too did Thomson in 1851. The result is the Helmholtz–Thomson rule: 'The electromotive force of an electrochemical apparatus is in absolute measure equal to the mechanical equivalent of the chemical action on one electrochemical equivalent of the substance.'[14]

Chemical heat measurements confirmed the Helmholtz–Thomson rule in a few simple cases including the Daniell cell. Difficulties in other cases were usually attributed to unaccounted secondary processes. In 1873 Helmholtz returned to galvanism to deal with a serious anomaly of that kind. In principle a single Daniell element could not continuously electrolyse water because the chemical heat for the formation of an equivalent of water was higher than the difference between the combustion heats of zinc and copper equivalents. There could only be a temporary polarization current. Yet refined galvanometry showed a small residual current. The

[12] Lodge 1885b: 731–40; FitzGerald and Trouton 1886, 1887. Lodge's and FitzGerald's positions were in part determined by their friendship with the influential chemist Henry Edward Armstrong, who rejected the German ionic theories. Cf. Dolby 1976: 314–15, 332–33.

[13] Cf. Ostwald 1896: 1067–1124; Whittaker 1951: 343–348; Crawford 1996 (on Arrhenius); Hiebert 1978 and Barkan 1990 (on Nernst); Dolby 1976 (on the British resistance to the dissociation theory).

[14] Helmholtz 1847: 46–57; Thomson 1851c: 477. Cf. Ostwald 1896: 749–766; Kragh 1993: 409–10.

simplest escape was to assume a violation of Faraday's law and a small metallic conductivity of the electrolyte. Helmholtz instead proposed that the polarization of the electrodes was continually destroyed by a mechanism that did not involve the production of free oxygen and hydrogen. Specifically, he imagined a constant flux of dissolved (neutral) oxygen from the anode to the cathode, and had the oxygen thus occluded in the platinum cathode combine with the nascent hydrogen. He confirmed this view by pumping off the dissolved gases and artificially reintroducing hydrogen or oxygen.[15]

In more refined experiments performed around 1880 Helmholtz found that the residual current never completely vanished, even when the amount of dissolved gas was quite negligible. In 1877 he had encountered another anomaly of the same kind in a study of concentration cells. In this case the two electrodes are made of the same metal and the only asymmetry is the difference of concentration of the solute at the two electrodes. Originally, Helmholtz held the chemical forces between the solute and the solvent responsible for the electric energy. However, by rigorous thermodynamical reasoning he found that the electromotive force was proportional to the logarithm of the concentration ratio. Hence the electromotive force did not depend on the global strength of the solution, and could not possibly match the chemical heat of dissolution. The Helmholtz–Thomson rule was violated.[16]

Such remarks are the probable starting point of Helmholtz's thermochemistry. A little after Gibbs, but independently, he understood that the evolution of chemical reactions, and the electromotive force of galvanic cells were both determined by the 'free energy' of the implied chemicals, not by their combustion heats. The Helmholtz–Thomson rule now appeared to be flawed by the neglect of the heat exchanged by the cell with the environment during the electrolytic process. However, the energy principle and Faraday's law were unshaken.[17]

7.2.3 Atoms of electricity

In conformity with the stronger program of the memoir on the conservation of force, Helmholtz also tried to reduce galvanic phenomena to the play of central forces. In 1847 he assumed that 'various chemical substances had different attractive forces for the two electricities.' This explained Volta's contact force between two different metals: electricity in the two metals was in equilibrium if and only if the difference of their 'free tensions' (electrostatic potentials) was equal to the difference of the kinetic energies that a unit of electricity would gain in entering each metal. For electrolytes, Helmholtz imagined that the molecules to be decomposed had parts charged with a universal quantity of electricity (our ions). Two plates of different metals connected by a conducting wire and immersed in an electrolyte would orient and

[15] Helmholtz 1873a. Cf. Kragh 1993: 410–11. On the verifications of the Helmholtz–Thomson rule, cf. Wiedemann 1882–1885, Vol. 2: 863–73; Ostwald 1896: 766–91.

[16] Helmholtz 1883a, 1883b: 99–101; Helmholtz 1877, 1882b. Cf. Kragh 1993: 412–14; Whittaker 1951: 341–3; Ostwald 1896: 978–83.

[17] Helmholtz 1882a, 1882b, 1883b. Cf. Kragh 1993. On Gibbs, cf. Klein 1972b.

redistribute these molecule parts by virtue of Volta's contact force. Then Helmholtz distinguished two cases.[18]

If the electric force acting on the molecule parts next to the electrodes was too small to transfer their electricity to the metal (or the converse), a state of equilibrium was gradually reached and the current ceased after a while: this is how Helmholtz interpreted electrode polarization. In the contrary case, the charged parts were neutralized and the corresponding chemical was freed at the electrodes (or induced a secondary reaction). This process could go on if the net energy produced by these two neutralizations, which Helmholtz identified with the chemical heat of dissociation, was positive.

Helmholtz's main concern was to reconcile the contact and chemical theories of the galvanic cell. In his compromise the electrolytic current was started under the effect of Volta's contact force (between two metals, or between metal and electrolyte). But the energy necessary to maintain the electrolytic current was provided by the chemical reactions at the electrodes, in conformity with Davy's and Faraday's views. There was no inconsistency, because the chemical energy and the Volta effect both derived from the attractive force between chemical atoms and electricity. In the simple case of a Daniell cell, the chemical energy and the Volta force both depended on the different attractions of electricity for copper and zinc. Consequently, the electromotive force of the cell had to be the same as the Volta contact force (with opposite sign). The key was to distinguish between two aspects of 'force': the accelerating force and what we now call energy. Contact tension belonged to the first aspect, electrochemical decomposition to the second.

Helmholtz's clever reasoning failed to close the controversy between contact and chemical theorists. Most Germans favored a form of the contact theory, and most of their British colleagues supported Faraday's purely chemical theory. However, in the 1860s William Thomson found direct evidence of the contact force by observing the deviation of a highly electrified needle placed under the contact line of half-rings of copper and zinc. Electrometric measurements convinced him that the contact potential difference sufficed to explain the electromotive force of the Daniell cell. From then on his views on galvanic phenomena were similar to those of his friend Helmholtz.[19]

Maxwell did not follow Thomson. In his *Treatise* he offered a new argument against the contact theory. He believed, against Thomson's thermoelectric theory, that the Peltier heat developed or absorbed at the junction of two different metals when crossed by an electric current simply represented the work performed by this current to cross the contact potential difference at the junction. This gave a contact force of a few microvolts, much too small to explain the electromotive force in the corresponding galvanic cell. Although Maxwell's argument received little attention in his lifetime, it became the focus of a vivid discussion between his and Thomson's

[18] Helmholtz 1847: 48–9, 56–7. Cf. Kragh 1993: 409–10. Helmholtz used the expression 'compound atom' for what we call a molecule.

[19] Thomson 1862. Cf. Hong 1994a: 238–43. On the continuing controversy between contact and chemical theory, cf. Ostwald 1896: Ch. 17: 909–13; Wiedemann 1882–1885, Vol. 2: 970–1002.

supporters in the 1880s. The protagonists never reached agreement. The differences in Maxwell's and Thomson's systems were too basic: they involved the very definition of the electric potential. Moreover, any attempt at measuring the successive potentials in a galvanic circuit necessarily introduced new interfaces with unknown contact potentials.[20]

Meanwhile, Helmholtz essentially maintained his picture of galvanic processes, which could easily accommodate Hittorf's and Clausius's insights into the process of electrolytic conduction. In most of his physics of the Berlin period, Helmholtz relied on the organizing power of general principles and avoided discussion of the deeper nature of electricity. He remained convinced, however, that at the borderland of chemistry and physics atomistic considerations were unavoidable. Around 1880 he developed his microscopic picture of electrode polarization with the concept of an electric double layer. In this view the polarization current consisted in an accumulation of charged molecule parts next to each electrode, leading to electric double layers of molecular thickness. These double layers provided the finite potential jumps that balanced the original potential difference of the electrodes. They explained why polarized electrodes behaved as condensers of extremely high capacity, as was already known to Varley and Maxwell. They bore on many other phenomena, including contact electricity, friction electricity, the flow of electrolytes through porous membranes, and the surface tension of mercury electrodes.[21]

Since 1847 the primitive entity of Helmholtz' electrochemistry was the electrified atom, carrying a universal quantum of charge (or integral multiples of it). Helmholtz publicly reasserted this view in his Faraday lecture of 1881:[22]

> If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity. As long as it moves about in the electrolytic liquid, each ion remains united with its electric equivalent or equivalents.

In this period Helmholtz frequently used the word 'ion' in the atomistic sense. He was neither the first one—Hittorf had already done so in 1878—nor the most militant one: Arrhenius was. The growth of this usage marked a wider acceptance of the electrified atoms and molecule parts.

In 1834 Faraday had briefly mentioned how such a notion could justify his law of electrochemical equivalence, but only to criticize atomistic speculation. Maxwell had a different attitude, as we saw in Chapter 4. When he wrote the section of his *Treatise* on electrolysis, he was seduced by Clausius's kinetic theory of electrolytes and could not avoid the conclusion that Faraday's law, when expressed in terms of Clausius's dissociated molecules, required the constant value of molecular charges.

[20] Maxwell 1873a: #249. For Thomson's interpretation of the conflict, cf. Thomson 1897. For a perceptive history, cf. Hong 1994a.

[21] Helmholtz 1879, 1880, 1881d. On mercury electrodes, cf. Whittaker 1951: 340–1. On endosmose, cf. note 10, above.

[22] Helmholtz 1881a: 69. In the sequel I use the expression electrolytic quantum to refer to Helmholtz's atom of electricity (or Stoney's 'electron') in a neutral manner. In his lectures Helmholtz favored the expression 'elektrisches Elementarquantum' (cf. Lenard 1920: 34).

'For convenience of description,' Maxwell called this value '*one molecule of electricity*.' He, however, judged the phrase 'gross' and 'out of harmony with the rest of [his] treatise.' He hoped that the true, theory of electric currents would banish molecular charges.[23]

In 1881 Helmholtz did not doubt the superiority of Maxwell's theory over its continental rivals. Yet he did not fully understand it, as he told his British audience: 'I confess I would be at a loss to explain without the help of mathematical formulae what [Maxwell] considers as a quantity of electricity, and why such a quantity is constant, like that of a substance.' Helmholtz felt justified to maintain the old substantialist terminology, if only for convenience. He believed that Maxwell's theory had to integrate the notion of atomic charge in order to explain the laws of electrolysis. Helmholtz's authority was immense at that time, in Germany as well as in England. His Faraday lecture soon became a canonical reference for anyone entering atomistic electrodynamics. Nonetheless, Maxwellian physicists had trouble assimilating the 'atoms of electricity,' as will be seen in a moment.[24]

7.3 Discharge in rarefied gases

Electrolysis was only one of the research topics from which Faraday expected insights into the nature of electricity. The other was electric discharge in rarefied gases. For the most part, historians have only explored the latter topic in so far as the discovery of the electron belonged to it. In contrast, this section aims at an accurate description of the goals and resources of the main actors in this field. This implies the resurrection of exotic effects and fantastic theories that have nothing to do with modern studies of gas discharge. However, the essential narrowing of this field around 1890 and the concomitant rise of ionic physics cannot otherwise be understood.

7.3.1 Faraday and a German friend

In 1838 Faraday predicted: 'The results connected with the different conditions of positive and negative discharge [in gases] will have a far greater influence on the philosophy of electrical science than we at present imagine, especially if, as I believe, they depend on the peculiarity and degree of polarized condition which the molecules of the dielectrics acquire.' He himself spent several months studying the various forms of discharge, with the intention of confirming his general view of contiguous action. In one series of experiments he studied the appearance of the discharge from an electrostatic machine in an evacuated vessel and discovered what is now called 'the Faraday dark space,' which separates the purple haze of 'the negative discharge' from the reddish light of the 'positive discharge' when the pressure of the air is less than half a centimeter of mercury (Fig. 7.2). He believed that he

[23] *FER* 1: #869; Maxwell 1873a: #260. See Chapter 4: p. 171. [24] Helmholtz 1881a: 60.

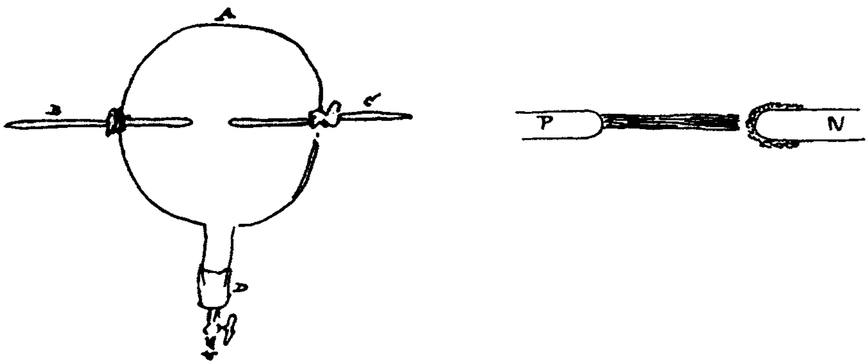

FIG. 7.2. Faraday's vessel for studying electric discharge in a rarefied gas (*FD* 3: #3114); and the appearance of the discharge (*FD* 3: #3137).

had found a new kind of disruptive discharge that could teach more about the role of the particles of the dielectric in the build-up and decay of polarization.[25]

Yet British physicists left the colourful beauties of gas discharge to enlightened amateurs, even long after Maxwell had recommended the topic 'to those who desire to discover something on the nature of electricity.' Progress was slow, mostly because of the embarrassing complexity of the discharge. It depended on many factors, among which were the shape of the tube, the form of the electrodes, the pressure and nature of the residual gas, the kind of electric source, and the intensity of the current. It exhibited strange, beautiful regularities that did not necessarily obey any simple laws. Typical in this regard were the striations of the positive light, which attracted much attention after John Peter Gassiot's pioneering study of them, but inspired as many different theories as there were investigators.[26]

The first physicists who brought some order in the bric-a-brac of gas discharge were Julius Plücker and his disciple Wilhelm Hittorf. Plücker benefitted from the mercury air pump (1855) and exceptional glass-blowing skills of Heinrich Geissler. In the continuity of his earlier studies on diamagnetism, he studied the changes of the glow (Fig. 7.3) and the positive light under the effect of a magnetic field. The discharge, he concluded in 1860, behaves 'as a bundle of elementary currents, which, under the influence of the magnet, change their form, as well as their position within the tube, *according to the well-known laws of electromagnetic action.*' For example, if the cathode was limited to a point, the negative glow turned into a bright line of light along the magnetic line of force passing through this point, because this was

[25] *FER* 1, series 13 (February 1838): #1523; *ibid.*: #1544–1560; *FD* 3: #3137 (21 June 1836). In the experiment described *ibid.*: #3174 (25 June 1836), Faraday used the smallest pressure he could obtain with his pump ('less than 0.4 inch'); the dark space was then about 1/16 of inch wide. Cf. Whittaker 1951: 349–50; Hiebert 1995: 95–7.

[26] Maxwell 1873a: #56. Cf. Hiebert 1995: 97–100 for a description of the studies by William Grove (barrister by training) and Gassiot (a wine merchant). For a few striation theories, cf. Wiedemann 1882–1885, Vol. 4: 581–584.

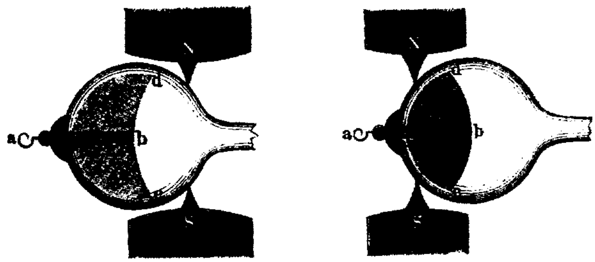

FIG. 7.3. Two of Plücker's observations of the glow light in a magnetic field (Plücker 1858). The glow light is sharply delimited by the line of force cd that goes through the tip of the cathode.

the only line along which the current was not disturbed by electromagnetic forces. Plücker proudly announced to his old friend Faraday that he had 'illuminated the magnetic curves.' (Remember that Faraday had used the same expression in a less literal sense upon discovering magneto-optical rotation). Plücker also observed that the negative discharge excited the fluorescence of the glass of the tube. He generally assumed that the light produced by the discharge was a secondary phenomenon: 'I find it most probable that, properly speaking, *electric light does not exist; the light which we see belongs to the gas, rendered incandescent by the thermal action of the current.*' As to the nature of this current, Plücker dared not speculate. He only knew that it obeyed the same electromagnetic laws as in other cases of conduction, in conformity with Faraday's views on the unity of the electric current.[27]

7.3.2 *Hittorf's* Glimmlicht

Some ten years later, Hittorf focused on the glow light (*Glimmlicht*) around the cathode. With sufficient exhaustion (below 2 mm of mercury), the glow light appeared to be formed of three layers: a first thin one next to the surface of the cathode, then a comparatively dark space (later called 'Crookes space' by the British) that increases with the degree of exhaustion, and lastly a luminous space that gradually vanishes into the Faraday dark space (Fig. 7.4). Experimenting with a L-shaped tube, Hittorf was surprised to find that the negative glow, unlike the positive light, was unable to pass the curve of the L. He inferred that the glow was formed of rays (the *Glimmstrahlen*). In conformity with this view, he found that solid objects placed between a point cathode and the fluorescing glass walls were able to cast well-defined shadows.[28]

Hittorf further varied the shape and the position of the electrodes in his tubes, so

[27] Plücker 1858, 1859, 1860: 256, 269. Cf. Whittaker 1951: 350–1; Hiebert 1995: 102–17; Dahl 1997: 49–55. On Geissler, cf. Kangro 1972. His pump brought the pressure down to a fraction of a millimeter.
[28] Hittorf 1869a: 1–10. Cf. Whittaker 1951: 351; Hiebert 1995: 117–24; Dahl 1997: 55–6.

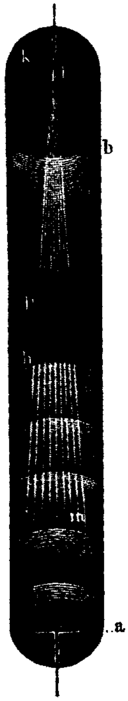

FIG. 7.4. General outlook of the discharge in a rarefied gas (from E. Wiedemann 1883). k denotes the cathode, surrounded by a thin luminous layer; kb the Hittorf–Crookes dark space; bp the glow light; ph the Faraday dark space; ha the striated positive light; a the anode; l the beam of the mostly invisible cathode rays (in 1869 Hittorf did not know yet that these rays could penetrate the positive light).

that the proportions of the two kinds of light would be different. Comparing the currents in two dissimilar tubes when fed in parallel by the same source, he found that the resistance of the glow light to the passage of the current was much higher than that of the positive light. This property, the ability to excite fluorescence, and the propagation in rays made the glow light a very peculiar phenomenon. Yet Hittorf found two reasons to believe that the glow was the process by which the gas conveyed the electric current near the cathode. First, by bringing the anode close to the cathode, he observed that the glow light resisted the penetration of the positive light (Fig. 7.5). More decisively, he showed that the glow rays were curved by the magnet just as lines of current would be (Fig. 7.6). He concluded that the electric discharge involved two modes of propagation of electricity. The first mode, corresponding to the positive light, was akin to metallic or electrolytic conduction. The second, corresponding to the glow light, was peculiar to gases and therefore deserved special attention. The glow rays, Hittorf added, suggested a transfer of electricity by wave

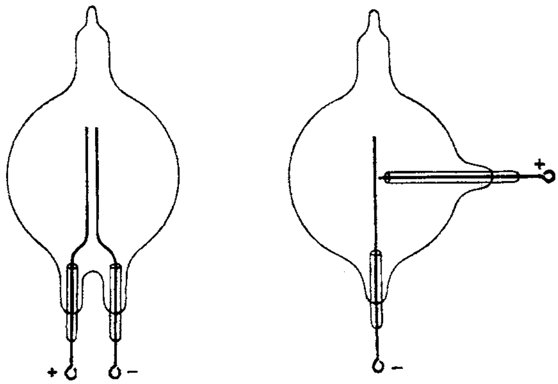

FIG. 7.5. Hittorf's tubes for showing the impenetrability of the glow light (Hittorf 1869).

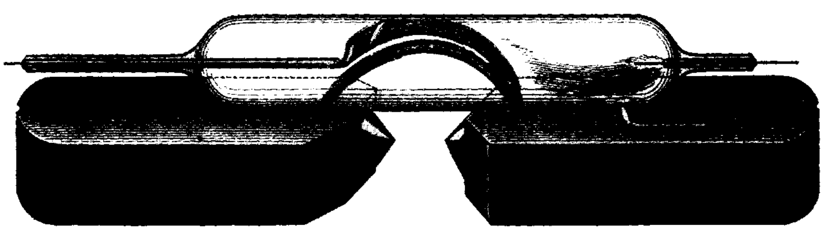

FIG. 7.6. The spiralling of the glow rays in a magnetic field (Hittorf 1869). A glass pipe acts as a collimator at the cathode end.

motion starting at the cathode. They would perhaps reveal the essence of the electric current and rid physics of its last imponderables.[29]

In his later works Hittorf abundantly quoted Faraday's views on charge, current, and polarization as the frame in which his thoughts on electric discharge had developed. However, he avoided further speculation, in conformity with his usual empiricism. His main purpose was to consolidate the fact of the asymmetry between the glow and the positive discharge. In 1879 he showed that the gas in the positive light was able to discharge very small potential differences between an additional pair of electrodes (Fig. 7.7). Further, quantitative study of the resistance of the residual gas in the various parts of the tube required the discharge to be continuous. Against received opinion, Hittorf showed that a continuous discharge could be produced by a battery of many cells, as long as the internal resistance of the battery was small enough. In 1883, he measured the potentials at various points of the continuous discharge thanks to secondary electrodes along the tube (Fig. 7.8). He found a sharp fall

[29] Hittorf 1869: esp. 222–3. Cf. Hiebert 1995: 119. The spiralling of the cathode rays in the magnetic field should not be confused with Plücker's illumination of the magnetic lines of force, which concerned the brighter part of the glow.

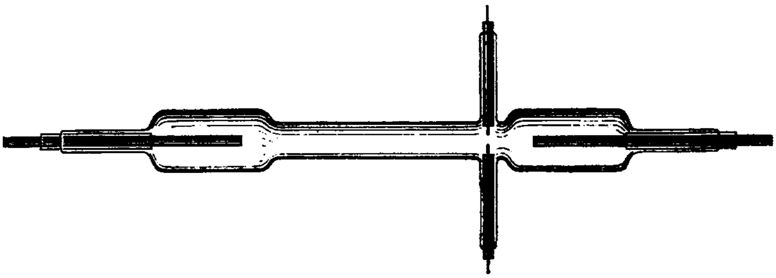

FIG. 7.7. Hittorf's tube for showing transverse conductivity (Hittorf 1879).

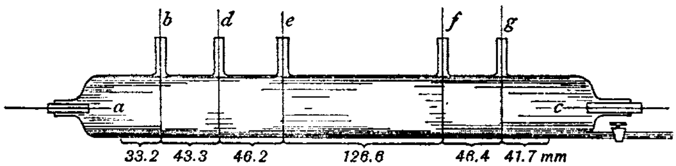

FIG. 7.8. Hittorf's tube for measuring potential falls (Hittorf 1883).

of potential in the negative glow, in conformity with his earlier resistance measurements. More surprisingly, he found that the potential fall in the positive discharge hardly depended on the current. In other words, the conductivity of the residual gas increased linearly with the current. This law beautifully illustrates Hittorf's extaordinary talent at extracting simple quantitative laws from complex phenomena.[30]

7.3.4 A corpuscular theory of discharge

Physicists were rather slow to recognize the importance of Hittorf's work on gas discharge, as they had been for his work on electrolysis. When, in 1871, Gustav Wiedemann and Richard Rühlmann proposed the first comprehensive theory of gas discharge, they paid no attention to Hittorf's experiments. They were concerned with discharge at moderate or high pressure, and with the well-known asymmetries between positive and negative discharge. In Weberian style, they assumed that particles of positive electricity or positively charged gas molecules were projected from the anode, and negative ones from the cathode. Thereupon the electric particles jumped from molecule to molecule, or the charged molecules exchanged their charge and their motion with other molecules during collisions. Light was emitted whenever the velocities of the charged molecules reached the threshold of thermal emission. The violence of the projection from the electrode depended on the affinity of each electricity for the metal of the electrode, and could therefore be made

[30] Hittorf 1879: 553–96 (continuity of battery discharge), 597–9 and 609–10 (on Faraday), 614–17 (tranverse conductivity); Hittorf 1883: 726, 729. Cf. Schuster 1911: 57–8.

asymmetrical. This stratagem explained the different tension thresholds for the positive and the negative discharge, as well as the shorter length of the negative discharge. The Faraday dark space corresponded to the neutralization of the two electricities. Nothing was said about Hittorf's new facts at lower pressures.[31]

7.3.5 Goldstein's divided discharge

In 1874 one of Helmholtz's students, Eugen Goldstein, took up the study of Hittorf's new rays, which he renamed 'cathode rays' (*Kathodenstrahlen*) or 'negative rays.' He observed that the rays emitted from a flat, extended cathode were able to cast distinct shadows. This indicated that the emission was normal to the surface of the cathode. Goldstein agreed with Hittorf that the rays were a special kind of current, as indicated by their magnetic deflection. But he rejected the idea that the positive discharge and the negative glow represented essentially different modes of conduction. In his opinion, the negative light and the successive layers of the positive light were all of the same kind. Each layer involved a bundle of rays starting at its front, and could be made to exhibit the same effects of magnetic deflection, shadow casting, and fluorescence as the glow light. The different appearance of the successive layers was only a matter of degree, and could be modified at will by playing with the shape and pressure of the discharge tube. Most strikingly, a narrowing of the section of the tube restored the appearance of the negative light, as if a new cathode were created in the strait (Fig. 7.9).[32]

In 1878 Goldstein sharpened his notion of multiple, similar discharges by a thought analysis of the path of the current in tubes of various convoluted shapes (Fig. 7.10). If, as he did not doubt, the negative rays represented an electric current, and if these rays did not travel in the direction of the anode, a return current was hard to imagine; and no effect of such a current had ever been observed. Goldstein drew a drastic conclusion: there was no return current at all, every layer of the discharge constituted an independent current system, and no current circulated at all in the dark spaces between the successive layers. This notion of open currents with no compensatory accumulation of charge contradicted both continental and Maxwellian theories of electricity. This did not worry Goldstein, who had more faith in his tubes than in established dogmas.[33]

[31] G. Wiedemann and Rühlmann 1872. Cf. Wiedemann 1882–1885, Vol. 4: 576–80. In the same year Cromwell Varley proposed that the negative glow was made of 'attenuated particles of matter projected from the negative pole' (Varley 1871: 239). His only arguments were the effect of the magnet on the glow, and the ability of the glow to repel a suspended silk fiber. He did not offer a comprehensive theory of the discharge.

[32] Goldstein 1876: 284–5 (normal emission of cathode rays), 286 ('Kathodenstrahlen'); 291 (contraction of the tube section, 'Aufeinanderfoge von Complexen negativen Lichtes'). Goldstein's most famous discovery is that of the 'canal rays,' which appear behing the cathode when a hole is pierced through it (Goldstein 1886). In England, Spottiswoode and Moulton 1880 and 1881 also emphasized the relative independence of the successive layers of the discharge, especially regarding the action of the magnet.

[33] Goldstein 1880a (dated 1878): 840–6 (problem of the return currents); 846–7 (independent current systems), 855 (open currents). Cf. Buchwald 1994: 135–7. For contemporary criticism, cf. G. Wiedemann 1882–1885, Vol. 4: 190. In the same paper (pp. 832–8), Goldstein showed that phosphoro-

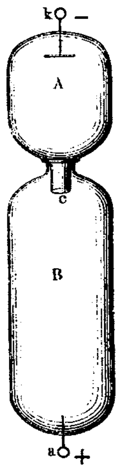

FIG. 7.9. Goldstein's funnel tube.

7.3.6 Crookes's fourth state of matter

In the same year an English experimenter, William Crookes, came to the field of gas discharge. Having worked on the radiometer for several years, he was a master at exploiting the vacuum pumping and glass blowing skills of his friend Charles Gimingham. Being converted to the kinetic-theoretical explanation of the radiometer, he sought to visualize the kinetic pressures by turning the mill of the radiometer into a cathode. This peculiar context of Crookes's discharge studies immediately explains why he operated with higher vacua than the Germans (reaching a millionth of an atmosphere!) and focused on molecular motions. With artfully designed discharge tubes and an excellent mercury pump, he rediscovered the rays of the negative light, their ability to induce fluorescence, their normal emission from the cathode, and their magnetic deflection.[34]

With George Stokes's help and support, Crookes interpreted the rays as a torrent of charged molecules projected from the cathode, as if in a gunshot. The emission of light depended on the collisions of the charged molecules with other molecules or with the glass of the tube. The first dark space then corresponded to the free flight of the charged molecules before their first collisions with other molecules. As further support to these views, Crookes adduced Stokes's electromagnetic explanation of the magnetic deflection, the sharp boundaries of shadows (absence of diffraction) (Fig. 7.11(a)), the mutual repulsion of two pencils of rays (Fig. 7.11(b)), and the motion of light objects under the impact of the rays (Fig. 7.11(c)). In his devotion

genic rays (light of very short wavelength) were produced at the end of the cathode rays. However, he observed these rays only inside the tube, and judged them unable to traverse solid films (p. 838).

[34] Crookes 1879a, 1879b. On British studies of gas discharge in this period, cf. Gordon 1880, Vol. 2: chs. 34–7.

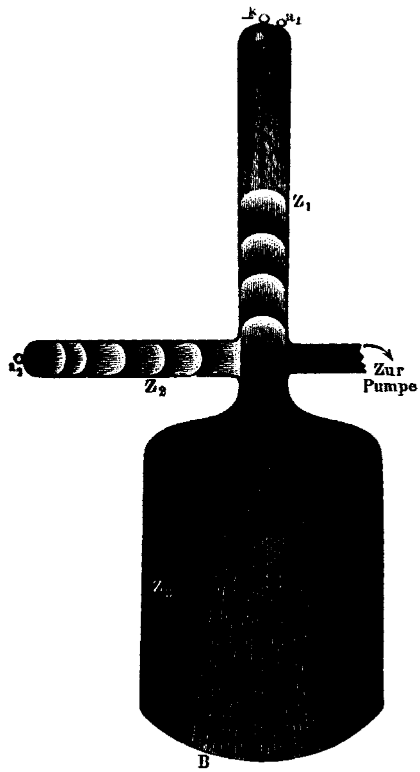

FIG. 7.10. Goldstein's tube for showing the mutual independence of the various layers of the discharge (Goldstein 1880a). The rays from the fifth layer travel straight to the fluorescent screen B, even though the anode a is in the perpendicular direction.

to Faraday, he announced his results as an 'illumination of the lines of molecular force.' By such lines he meant the straight trajectories of the molecules in a highly rarefied gas, for which intramolecular collisions became improbable. This 'ultra-gaseous' state of matter, as he called it, appealed to his taste for the occult, and allowed him to boast a major discovery.[35]

Crookes' publication irritated the German experts on gas discharge. Being familiar with most of the reported effects, they were shocked by the nearly complete lack of reference to Hittorf's and Goldstein's works. Moreover, they strongly opposed the interpretation of the cathode rays as a molecular torrent. Hittorf

[35] Crookes 1879a: 58 (cathode mill), 60–2 (proofs of molecular theory), 62 ('gun shots'), 64 (fourth state); Crookes 1879b: 142–4 (pressure and darkspace measures), 142 (explanation of dark space); Stokes [1876] for the magnetic deflection in terms of macroscopic current elements submitted to the electromagnetic force and to a mechanical tension (for a Weberian derivation, cf. Riecke 1881). Cf. Dahl 1997: 64–77. For the Stokes–Crookes exchange, see Stokes 1907, Vol. 2: 410–21; Wilson 1987: 191–201. On Crookes's biography and his spiritism, cf. Brock 1971.

(a)

(b)

(c)

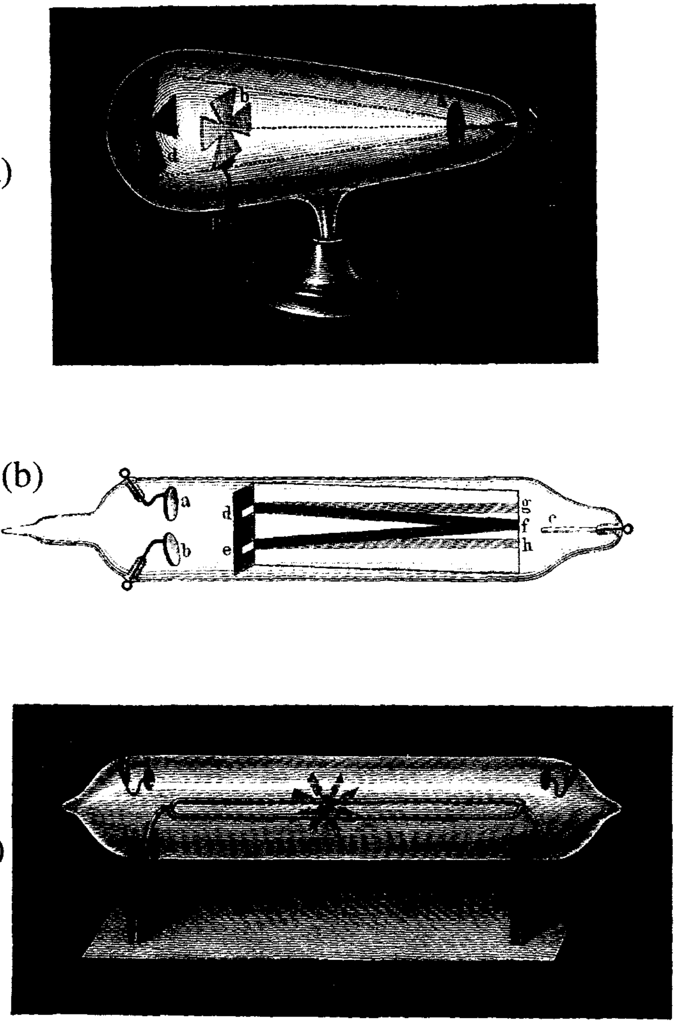

FIG. 7.11. Crookes's experiments supporting his projectile theory of the cathode rays (Crookes 1879a).

reassessed his agreement with Faraday's view that the dark space involved true disruptive discharge without electric convection. Goldstein noted that Crookes's theory failed to explain Hittorf's first layer of the glow light, the residual luminosity of the second layer (Crookes's dark space), the rectilinear propagation of the rays in the third layer, and their ability to travel well beyond the negative glow. He also found the dark space to be much thicker than the mean free path of the molecules, and much more sharply delimited than Crookes's explanation suggested.[36]

Not even Wiedemann, coauthor of the leading corpuscular theory of gas discharge, admitted Crookes's views. His son Eilhard had just persuaded him to give up any theory that made the discharge current a convection of charged molecules. The argument was based on W. von Zahn's recent finding that the spectral lines of the discharge exhibited no measurable Doppler effect. This implied that the velocity of the luminous molecules had to be less than 1 km/s, whereas the velocity of gas discharge was known to exceed 1000 km/s since Wheatstone's rotating mirror measurement of 1835. To this contradiction of the molecular theory of discharge, Eilhard Wiedemann added a specific objection to Crookes's interpretation of the cathode rays. If these rays were made of molecules, he argued, their velocity had to exceed 100 km/s in order to explain the heat produced during their impact on the tube walls. Again, such a high velocity contradicted the absence of Doppler effect in the luminous gas.[37]

7.3.7 German waves

Crookes's provocation prompted the Germans to publish alternative mechanisms of gas discharge. Eilhard Wiedemann provided the first comprehensive ether-based theory. Since the discharge current was not a convection of charged molecules, he reasoned, it could only be a polarization current involving both the ether and the molecules. Reasoning in loosely Maxwellian terms, he assumed that the electricity or 'free ether' accumulated at the surface of the electrodes induced a dielectric polarization of the gas, that is, a deformation of the 'ether envelopes' of the molecules. During the discharge, longitudinal waves of polarization started at the electrodes, and set the ether-envelopes of the molecules into light-generating vibrations. In this way Wiedemann explained the high velocity of the discharge, as well as his main experimental finding: that the luminosity of the gas under discharge was not a consequence of heating. He also suggested a reason for the dissymetry of the negative and the positive discharge: while the former mostly depended on the polarization wave, the second also involved a transfer of the free ether from molecule to molecule. In this scheme the cathode rays were a pure polarization wave, the first wave surface of which espoused the form of the cathode.[38]

Goldstein also had ether processes in mind, but of a different kind. As we have just seen, for Wiedemann the molecules played an essential part in the formation of the polarization that preceded the discharge current. In contrast, Goldstein assumed a tension in the free ether, and regarded the molecules as inhibitors of this tension. He justified this strange, anti-Maxwellian view by means of new measurements of

[36] Crookes 1879b: 163n for a discreet reference to Goldstein 1876 (which Schuster had translated for *PM*); Hittorf 1879: 607–8 (with reference to *FER* 1: #1551); Goldstein 1881a: 90–1.

[37] G. Wiedemann 1882–1885, Vol. 4: 580–1; E. Wiedemann 1880: 245–6 (no Doppler effect); 252 (heating of tube walls); Zahn 1879, also Tait 1880. E. Wiedemann's criticism was also directed against physicists like Johann Puluj and Wilhelm Gintl, who believed that the cathode rays were made of particles of the cathode's metal (cf. G. Wiedemann 1882–1885, Vol. 4: 586–7).

[38] E. Wiedemann 1880: 246–51 and 1879 (for the cold emission of light).

the resistance of the various part of the discharge. Placing a spark micrometer in parallel with the tubes, and varying the length of the various parts of the discharge, he found that when the pressure was sufficiently low, the only resistance left was a surface phenomenon on the cathode. This contradicted Hittorf's prior results, which indicated a strong resistance of the negative glow at low pressure. Goldstein inferred, against received opinion, that vacuum was a perfect conductor.[39]

Based on this concept, Goldstein provided a general theory of the discharge tube. First, an uneven tension of the ether had to build up in the discharge tube. If the pressure of the gas was not too high, the tension would exceed the rupture threshold on a series of surfaces, to be identified with the fronts of the various layers of the discharge. From the side of those surfaces opposite the cathode sprang the negative rays that induced luminous effects in the neighboring gas, with a variable aspect according to the original distribution of tension. The successive dark spaces indicated regions in which no tension, and therefore no ether motion, occurred (unless the pressure of the gas was low enough to allow the penetration of the rays from the previous layer-front). Goldstein's readers, both in Germany and in England, must have wondered how he could declare vacuum a perfect conductor and yet deny the existence of a current in the dark spaces. Evidently, Goldstein's concept of conduction was less luminous than his tubes.

7.3.8 *Divorcing the cathode rays from the discharge current*

Inflamed with quarrels over priority and interpretation, gas discharge was becoming a hot topic. In 1882 Helmholtz's star pupil, Heinrich Hertz, entered the field. His most important aim was to test whether the current in the negative discharge followed the path of the cathode rays, as Hittorf, Goldstein, Crookes, and the Wiedemanns had all assumed. Plausibly, his doubts came from Goldstein's visual proof (Fig. 7.10) that no return current corresponded to the cathode rays. In order to decide this issue, Hertz mapped with a suspended magnetic needle the magnetic force produced by a plane discharge. As long as the cathode rays did not have an effect *sui generis* (non-electromagnetic) on the magnet—which fact Hertz carefully checked—the lines of current could be computed from this map. Hertz found that they did not follow the cathode rays (Fig. 7.12). A possible explanation was that other currents of a different nature were superposed on the cathode ray currents, even in the negative glow. Hertz judged this implausible, and proposed that the rays were a side phenomenon, an ether disturbance that had little to do with the electric discharge proper. Helmholtz approved, and suggested a connection with the longitudinal waves that his electrodynamics permitted (for a non-zero k). Hertz added that the magnetic deflection of the rays could perhaps be explained in analogy with the Faraday effect: to a rotation of the polarization for a transverse optical wave could correspond a deflection of a longitudinal wave in the same medium.[40]

[39] Goldstein 1880b: 189; 1881b: 257–60, 266. Edlund 1882 proposed similar views in connection with his ether theory of 1872.

[40] Hertz 1883: part 2. Helmholtz to Hertz, 29 July 1883, in Koenigsberger 1902, Vol. 2: 305; Hertz to Helmholtz, reply to the former, *ibid.* Cf. Buchwald 1995, 1994: 150–7, 171–4, for a thorough discussion.

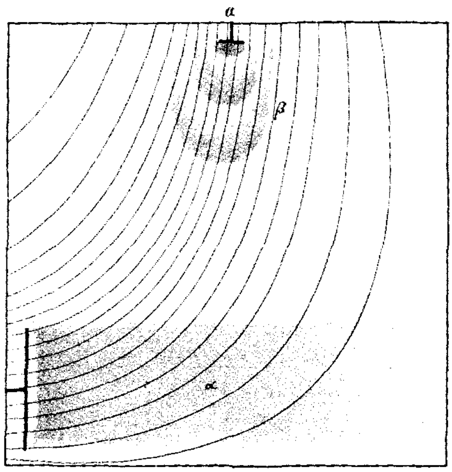

FIG. 7.12. Hertz's current map for the discharge in a layer of rarefied gas (Hertz 1883). a is the anode, β the striated positive light, α the negative glow.

In order to confirm his views, Hertz tested the absence of electrostatic effects of the rays by two different means. First, he tried to show that the rays, once purified through a gaze at the potential at the anode, failed to induce charges in a metallic cylinder connected to an electrometer (Fig. 7.13). Second, he passed the rays between two electrified plates and found no deflection. These experiments were questionable. In fact, Hertz observed an induced charge in the first experiment, but ascribed it to the incomplete purification of the rays. As for the deflection experiment, Hertz knew from Hittorf that the gas submitted to the discharge became a good conductor, but did not quite realize that this effect could prevent the establishment of the electric force between the deflecting plates. At least he admitted that these two electrostatic experiments were 'imperfect.' They carried little weight in later discussions of the cathode rays.[41]

Hertz's map of the discharge current had more impact. Eilhard Wiedemann immediately modified his theory, and stated that the cathode rays 'could not take any important part in the formation of the current and in the transfer of electricity.' Whereas the latter process was determined by longitudinal waves of dielectric polarization, the cathode rays were nothing but 'light rays of a very small oscillation period.' Wiedemann even claimed to have observed the reflection of this 'light' on the walls of the tube. Unlike Hertz, he meant true light, with transverse vibrations.

[41] Hertz 1883: part 3. Cf. Hon 1987 for a description of these experiments; Buchwald 1994: 158–163, 166–9 for Hertz's understanding of them; Lenard 1920: 80 for an *a posteriori* explanation of their failure. G. Wiedemann 1882–85, Vol. 4: 436–7 summarized Hertz's results. Yet they were usually ignored or dismissed. Goldstein had observed a deflection of the cathode rays when passing near another cathode (Goldstein 1876: 285; cf. Wiedemann 1882–85, Vol. 4: 425–9). He ascribed this effect to a variable refractive index of the medium near the cathode for the cathode waves (Goldstein 1880b).

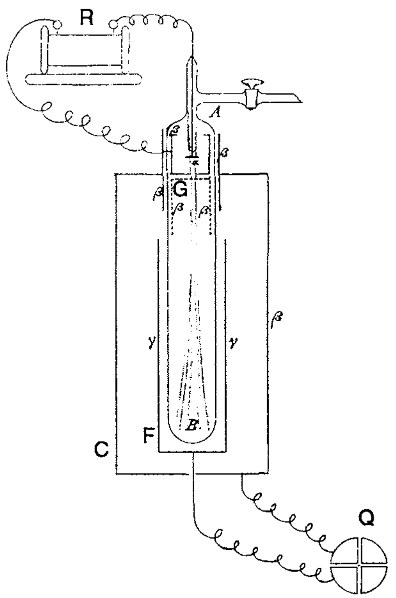

FIG. 7.13. Hertz's apparatus for detecting the electric charge of the cathode rays (Hertz 1883). R denotes a Ruhmkorff coil, G a filtering gaze, F the Faraday cylinder, C a Faraday cage, Q a quadrant electrometer, α the potential of the cathode, β that of the anode, and γ that of the Faraday cylinder.

He thus explained why the rays only 'interfered' with the longitudinal discharge waves when crossing them at right angle. The mutual repulsion of two beams, and their magnetic deflection were more problematic. Wiedemann tentatively referred the first effect to Maxwell's radiation pressure, and the second to Hertz's analogy with the Faraday effect.[42]

The publication of Goldstein's and Wiedemann's views signified a sharp contrast between German and British views on gas discharge. In England, Crookes' projections of charged molecules formed the basis of the few studies of the discharge. In Germany, ether processes were an essential part of the discharge, either in the form of dielectric polarization (Wiedemann) or in the form of vacuum conductivity (Goldstein). Paradoxically, the least Maxwellian or Faradayan view (Crookes') was British, and the most so (Wiedemann's) was German. It was a man trained in both traditions, Arthur Schuster, who brought an essentially new insight into this field.[43]

[42] E. Wiedemann 1883, 1884: 85, 86, 88–9. E. Wiedemann also exploited (*ibid.*: 85) his own finding that the screening of the cathode rays by a mica plate did not affect the global current in the tube (E. Wiedemann 1880).

[43] Translations of Goldstein's and E. Wiedemann's main papers appeared in *PM*. The main English studies of the discharge after Crookes were those of two gifted amateurs, William Spottiswoode and John Moulton: cf. J. J. Thomson 1893a: 129–31, 141.

7.4 Gaseous ions

7.4.1 Schuster's revolution

Born in Frankfurt in 1851, Arthur Schuster followed his family to England at age 19, attended Owens College in Manchester, and traveled to complete his education under Kirchhoff, Weber, Helmholtz, and Maxwell. Owens College offered him a chair of applied mathematics in 1880 and a chair of physics in 1887. He was equally familiar with Maxwell's *Treatise* and with Weber's and Helmholtz's electric theories. His British mentor, Henry Roscoe, suggested his first research topic: spectroscopy. This involved the frequent use of Geissler tubes as spectral sources. After a while, Schuster decided to study the discharge for itself. This happened in 1882, shortly after the stir created in Germany by Crookes's Bakerian lecture, and only a few months after Helmholtz's Faraday lecture. Schuster originally favored Crookes's theory, even though the mechanism for the projection of the molecules from the cathode remained obscure.[44]

In 1883 Schuster was 'struck' by the experiments performed by Lucien Blake in Helmholtz's laboratory: the vapor from mercury at a very high electric potential turned out to be completely unelectrified. This made the charging of molecules implausible in discharge tubes. Schuster then assumed that 'the passage of electricity from one molecule to the other [was] always accompanied by an interchange of the atoms composing the molecules.' Specifically, he imagined a breaking up of the molecules into charged parts at the cathode, followed by the electric repulsion of the negative particles, their free flight through the dark space, and light-generating collisions in the glow. Being aware of Eilhard Wiedemann's objection to Crookes (based on the Doppler effect), Schuster specified that the emission of light occurred only after the energy of the fast particles from the cathode had been isotropically redistributed among the particles of the glow.[45]

For the further explanation of the decomposition process, Schuster relied on a systematic analogy with Helmholtz's theory of electrolysis and on Hittorf's recent potential measurements. The potential fall in the negative glow, he argued, indicated the existence of a Helmholtzian double layer of electricity, with negative electricity on the cathode and an accumulation of positive particles in the glow. The strong electric field in this space, together with the chemical forces exerted by the positive particles, was responsible for the splitting of the molecules. One difference from electrolysis was the macroscopic size of the double layer, which Schuster explained by the lacunar structure of the gas. A more important difference was the absence of decomposition prior to the discharge. According to Schuster, an initial spark produced the initial supply of decomposed molecules that were necessary for the formation of the double layer. He also suggested that an electromotive force of contact

[44] Cf. Kargon 1975; Feffer 1989: 34–8; Dahl 1997: 92–9.

[45] Schuster 1884: 318 (quotes), 331 (no Doppler effect); Blake 1883. Cf. Feffer 1989: 40–1. Another source of Schuster's inspiration was the belief of his friend Norman Lockyer that complex spectra depended on molecular dissociation: cf. Brock 1969, and Feffer 1989: 40.

between the gas and the electrodes could explain why decomposition was predominant at the cathode. Lastly, he gave a foretaste of his future explanation of the striations of the positive light in terms of variable rates of decomposition and recomposition of the molecules along the gas.[46]

In support of his electrolytic conception Schuster adduced a wide range of properties of the discharge. Profiting from his earlier specialty, he provided spectroscopic evidence of the decomposition near the cathode. He also referred to Warren de la Rue and Hugo Müller's proof of a non-thermal expansion of the gas under discharge. He showed that in a reputedly monatomic gas, mercury vapor, the discharge was hard to pass and homogenous. Elaborating on Hittorf's old experiments about the effect of the anode on the glow, he managed to repel the glow with a positive electrode, in conformity with the accumulation of positive particles in this region. Of Hittorf, he could also explain the high transverse conductivity of the gas under discharge, and the fact that the conductivity of the positive column increased linearly with current intensity: conduction depended on the number of dissociated molecules, and this number naturally increased with the discharge current.[47]

For Schuster the best proof of the electrolytic theory would have been the demonstration that the particles of the decomposed molecules all carried the same charge: Helmholtz's 'atom of electricity.' He hoped to infer the value of e/M, the charge to mass ratio, from the magnetic deflection of the cathode rays and from their velocity. He did not succeed until 1890. Even then he could only give a wide bracket for the value. The main difficulty was the determination of the velocity of the particles. Schuster measured the potential difference V between the cathode and the diaphragm from which the magnetically curved beam originated. If, he reasoned, the particles traversed this potential difference without energy loss, then their velocity v would be given by $2Ve = Mv^2$, where M is the mass of the ion. This relation, together with the formula $Mv^2/R = evB$ for the curvature radius R in the magnetic field B, yields $e/M = 2V/B^2R^2$. Injecting his measurements of R and V, Schuster found a value 1000 times higher than that expected for the ions of the residual gas. He concluded that the ions were considerably slowed down by collisions before entering the diaphragm. Taking for v the thermal velocity of the gas, he recovered the electrolytic value of e/M.[48]

[46] Schuster 1884: 326–30 (double-layer), 329 (asymmetry), 336 (striations). Cf. Whittaker 1951: 355–56; Heilbron 1965: 62. Schuster did not yet call the charged atoms 'ions.' At the BA meeting of 1885, Schuster criticized Lodge's report on electrolysis (Lodge 1885b) for underestimating Hittorf's and Kohlrausch's evidence in favor of the independent migration of the ions (see above, pp. 269–70); he also explained Helmholtz's views and their bearing on gas discharge (Schuster 1885; cf. Feffer 1969: 46).

[47] Schuster 1884: 319 (Hg), 322 (spectrum), 323 (de la Rue), 326 (glow repelled), 335 (Hittorf's law). Cf. Feffer 1989: 41. Schuster 1887 modified Hittorf's experiments on transverse conductivity by placing the secondary electrodes far from the discharge (in order to exclude thermal effects). The discharge in mercury vapor has a large dark space, which however could not be seen in Schuster's narrow tubes (cf. Schuster 1911: 61–2). In 1893, Franz Stenger, of Dresden, speculated that the conduction in this monatomic vapor involved a 'dissociation of higher order' in which atoms were split into smaller parts (Stenger 1893: 379).

[48] Schuster 1884: 332; 1890: 545–7. Cf. Schuster 1911: 65–7. Heilbron 1964: 63; Feffer 1989: 41–2, 48–9. The pressure in Schuster's tube was 0.3 mm, for which the dark space is about 1 cm wide (in air; Schuster used nitrogen). Schuster's experimental difficulties seem to have depended on two conflicting

7.4.2 *Helmholtz versus Maxwell*

Schuster's researches were the subject of two Bakerian lectures, read in 1884 and 1890. In the meantime, he was pleased to observe that the electrolytic view of conduction in gases had made great progress in Germany. The initiator of this movement was a former assistant of Helmholtz, Wilhelm Giese. By careful electric measurements published in 1882, Giese established violations of Ohm's law for the conduction of hot gases, and he explained them in terms of the availability of 'ions.' The heating of the gas produced a limited supply of ions that were carried to the electrodes by the current and neutralized there. Giese was followed by the inseparable Hans Elster and Julius Geitel, who interpreted the properties of hot electrodes—what we now call the thermoionic effect—in similar terms. Helmholtz's son Robert also contributed to the new ionic physics by showing that the ability of hot gases to condense moisture (discovered by John Aitken) depended on the formation of ions.[49]

In his second Bakerian lecture, Schuster discussed all this work, and emphasized that all known cases of conduction in gases could be traced to the production of ions by various causes, including high heat, electric discharge, and UV light. He now systematically used 'ion' instead of 'charged atom,' as Giese had done in 1882. Thus was born a new physics of ions, no longer confined to gas discharge or electrolysis, and promising to unify a wide range of phenomena.[50]

Schuster declared himself satisfied with the reception of his theory, especially in Germany. He had received Helmholtz's personal support, *pace* Goldstein and Hertz. At home, he encountered objections from the Maxwellians. Anticipating this reaction, in his first Bakerian lecture he had emphasized that his theory avoided the most shocking heresies of continental theorists: the jumping of electricity from molecule to molecule (according to the Wiedemanns) and the conductivity of vacuum (according to Goldstein and Edlund). But enough remained to displease the Maxwellians. Following Helmholtz, Schuster had spoken of the two electricities as real substances with different attractions for different chemical elements. He had also introduced the crudely anti-Maxwellian notion of volume electricification in his explanation of the glow. And he had assumed the independent migration of ions, which Lodge refused to admit in his British Association report on electrolysis. All of that, he ironized in his 1890 lecture, runs 'against the so-called modern views of electricity.' For this reason he placed himself 'under the shelter of recognized authority' by quoting Helmholtz on atoms of electricity at the beginnning of his lecture.[51]

At the same time, Schuster had a conciliatory gesture:

requirements: he wanted to determine the velocity from the accelerating potential, and at the same time he wanted the rays to be visible (through the fluorescence of the residual gas). The more successful measurements by Wiechert and J. J. Thomson dropped both requirements and used much lower pressures.

[49] Giese 1882: 537–44; Elster and Geitel 1883, 1889; R. von Helmholtz 1887 (following a suggestion in Giese 1882: 538n).

[50] Schuster 1890a: 526–39.

[51] Schuster 1890a: 527 (pleased), 559 (quotes); 1884: 317. Cf. Feffer 1989: 49–50.

In all branches of physics, we are gradually forced by the advance of knowledge to abandon the assumption of homogeneousness, and if that is done, no further difficulty stands in the way of bodily electrifications; for we may take them to be really only surface electrifications between the atoms and the medium.

The notion of electrification as a discontinuity in dielectric strain could be maintained if only the strain occurred in the space between atoms. 'Even taking the extreme view [FitzGerald's and W. M. Hicks'] that electric stress is due to vortex filaments in the ether,' Schuster continued, 'we need only assume all these filaments to have the same intensity, and some to end at the surface of atoms, in order to rèconcile apparently antagonistic views.' As we will see in a moment, this is exactly what J. J. Thomson did in the following year.[52]

7.4.3 *J. J. Thomson on vortex rings*

In print, Schuster was not the first physicist to apply the electrolytic analogy to discharge in gases. J. J. Thomson did this in 1883, but in a quite different manner. Having also attended Owens College, Thomson shared Schuster's interest in questions bordering between physics and chemistry. His Cambridge education, however, directed him to the dynamical theories of William Thomson and Maxwell. One of his first publications, in 1881, was a Maxwellian theory of charge convection, motivated by Crookes' experiments. In 1882 he won the Adams prize for a highly mathematical work on the motion of vortex rings in an ideal incompressible fluid. Like William Thomson, he hoped to explain atoms and molecules in terms of combinations of such vortex rings.[53]

From difficult mathematical theorems on the interactions between two vortex rings, J. J. Thomson jumped to loose speculations on the kinetics and the chemistry of gases, for which the interactions between molecules were simplest. He imagined atoms as systems of mutually embracing rings, and identified their valence with the number of rings. In the simple case of two monovalent atoms, he predicted that the corresponding rings could combine their motions during soft collisions. However, the combination was unstable. In conformity with Clausius's theory of electrolysis, Thomson inferred that the molecules constantly exchanged their atoms during collisions, even though the proportion of free atoms in the gas remained in general very small.[54]

[52] Schuster 1890a: 558–9. Most likely, Schuster had in mind the vortex analogue of electrostatics proposed by William Hicks at the BA meeting of 1888 (Hicks 1888): bundles of vortex filaments corresponded to the electric lines of force; the abutting of the filaments on a solid to electric charge; their contraction to an electric current; and the linear motion of the fluid to a magnetic field. Cf. Whittaker 1951: 302–3.

[53] J. J. Thomson 1883b, 1881a, 1883a. Cf. Heilbron 1976; Falconer 1987: 252–3; Feffer 1989: 38–39; Davis and Falconer 1997: 1–17. On Thomson's interest in chemistry, cf. Chayut 1991. On his works in theoretical dynamics and his vortex models, cf. Topper 1971, 1980.

[54] Thomson 1883a: 114–5, 120, 124.

7.4.4 The vortex theory of gas discharge

If, as Thomson further assumed, an electric field was nothing but a heterogenous distribution of velocity of the primordial fluid, it had to affect the continual dissociations of the molecules. For example, a stronger velocity component along the electric lines of force would imply a higher dissociation rate, and therefore, according to the vortex theory, a smaller pressure along those lines. This could explain the Faraday stresses in the electric field. Most important, Thomson identified the current in a discharge tube with an ongoing decomposition of the gas molecules in the electric field. Here it is essential to note that for Thomson the split atoms did not carry any electric charge. From his Maxwellian point of view, electric charge was a macroscopic surface phenomenon, occurring at the limit between a conducting and a nonconducting medium. Conduction was not the convection of charged particles, but the dissipative decay of the energy of the field. Molecules were the instruments of this decay, not the conveyers of electric charge.[55]

Thomson's picture immediately explained why the electric strength of a gas depended on its chemical composition. It also suggested why rarefying the gas first eased the conduction, but impeded it when taken too far: a smaller pressure implied a higher tendency to dissociation (because the free atoms had less chances to meet partners), but decreased the amount of available molecules. The reasoning was dubious, and it did not explain the asymmetry and the main appearances of the discharge, as Schuster promptly noted.[56]

Thomson soon offered an improved explanation of gas discharge, again suggested by the vortex ring model: electric dissociation occurred when the molecule had traveled in the direction of the electric force for a sufficiently long time. In a dense gas, this condition was never met due to frequent collisions. In a rarefied gas, the dissociation was most probable near the cathode because the longest flights in the direction of the field ended there. Thomson regarded the decomposition next to the cathode as an explosion, during which the atoms or molecule parts were projected beyond the dark space, and recombined in the glow. The heat produced there induced new explosions, and so forth until the anode. This mechanism superficially resembled, and may have been partly inspired by, Schuster's theory. However, for Thomson the projected atoms carried no charge, they were not repelled by the cathode, and they owed their kinetic energy to the original explosion.[57]

In 1884 Thomson was elected to succeed Lord Rayleigh at the head of the Cavendish Laboratory. This responsibility of course involved experimental research. Thomson's early tries in the local tradition of precision measurement had been unimpressive, for he lacked the required patience and manual skills. He did not persist.

[55] Thomson 1883b: 427–9 (stresses), 431–2 (discharge). Cf. Feffer 1989: 38–9. However, in 1881 Thomson seems to have accepted Crookes' interpretation of the cathode rays, which was the declared incentive of his paper on electric convection (J. J. Thomson 1881a).

[56] J. J. Thomson 1883b: 432; Schuster 1884: 317. In 1890 Schuster offered his own explanation of why a higher pressure implies a more difficult discharge: the dissociating electric force at the cathode is diminished by the dielectric polarization of a surface layer of gas whose thickness increases with the pressure (Schuster 1890c: 197).

[57] J. J. Thomson 1884b: 237; 1886: 396–406.

In harmony with his recent theoretical interests, he decided to start a program for the experimental study of electric discharge in gases. For manipulations and apparatus, he depended on his friend Richard Threlfall and on a skilled glass-blower, D. S. Sinclair. His earliest experiments were of two kinds. In the first, he verified that the electric discharge was easier in spontaneously dissociated gases like iodine vapor, and measured the pressure variation during the discharge in various gases, as evidence of decomposition. In the second kind of experiment, he studied the discharge between two parallel metal plates, the simplest from the point of view of the vortex theory. The results were disappointedly complex, but gave Thomson an opportunity to publish his explosions-in-series theory.[58]

7.4.5 A Maxwellian's crisis

In 1888 Thomson's views underwent a major crisis. One cause was the confrontation with Schuster's results. According to Schuster, the electric current in a rarefied gas or in an electrolyte consisted in the motion of the available ions. Thus conductivity depended on the original supply of ions, not on the ease with which an electromotive force could break the molecules. In contrast, in Thomson's original theory conduction *was* decomposition. This concept conflicted with Hittorf's and Schuster's proofs of the high transverse conductivity of gases under discharges. Thomson had to admit that 'when the molecules are split up into constituents, a state of molecular structure is produced in which the discharge may be produced by rearrangement without further decomposition.'[59]

Thomson also came to worry about electrolysis. Being temporarily deprived of a glass-blower, he decided to measured the osmotic pressure of electrolytes for different values of the current. From the invariability of this pressure he concluded that the passage of the current did not at all increase the number of constituents, in conformity with the pre-dissociation theory of the Germans. Thomson still avoided Arrhenius's concept of quasi-complete dissociation. He maintained that the continual splitting up of the molecules (in the absence of electromotive force) was quickly followed by their recomposition, so that the fraction of dissociated molecules remained small. However, he now admitted that 'atoms in the nascent condition' were free to move under an external electromotive force. This meant a major departure from his earlier views, in which the separated atoms carried no electric charge. In Thomson's new conception, the vortex atom no longer appeared, and a molecule in the verge of decomposition behaved as a pair of charged atoms.[60]

Thomson's assimilation of Schuster's ions was strained and imperfect. He tried

[58] J. J. Thomson 1887, 1886. Cf. Heilbron 1976; Feffer 1989: 42–3; Davis and Falconer 1997: 45–9. On Thomson's break from the previous Cavendish tradition, cf. Falconer 1989. After 1887, J. J. Thomson crucially depended on his private assistant, Ebenezer Everett, who blew glass, built all sorts of apparatus, and helped in all manipulations: cf. Davis and Falconer 1997: 55–6.

[59] J. J. Thomson 1888: 292. Thomson frequently referred to Schuster's experiments, but never to his theoretical ideas, although I believe he found much inspiration in them.

[60] Thomson to Threlfall, 7 August and 4 September 1887, Cambridge University Library, quoted in Feffer 1989: 43–4, and J. J. Thomson 1888: 294 (electrolytic experiments); *ibid.*: 213 (against Arrhenius), 295 (charged atoms). Thomson calls the charged atoms 'ions' once, on 301; however, his systematic use of this word only started in 1896.

hard to blend the electrified atoms in a persistently Maxwellian view of the electric current. On the one hand, he still regarded the conduction current as 'a series of intermittent discharges caused by the rearrangement of the constituents of molecular systems.' On the other hand, he described this rearrangement as a separation of ions that temporarily discharged the electric field while forming local double layers, followed by recomposition of the positive ions of one double layer with the negative ions of the next. Thomson believed this picture to apply to every kind of conduction, including metallic conduction (with no visible effect of decomposition, because of the homogeneity of the metal). He could thus explain a typical Maxwellian anomaly, the transparency of electrolytic solutions and thin metallic sheets. The absorption of light did not occur in these conductors, because the oscillation period of the corresponding electric field was much smaller than the time required by the ions to discharge the field. Here were the seeds of a new, Maxwellian microphysics, in which ions contributed to the basic field processes from which charge and current derived.[61]

This major transition in Thomson's thought came without much publicity, and without explicit rejection of his earlier views. This attitude generated misunderstandings. In his second Bakerian lecture, Schuster attacked the notion that molecules were dissociated in gases before the passage of the discharge as being 'fatal' to Thomson's theory. Spontaneous dissociation, according to Schuster, would yield ions and permit conduction even for very small electromotive forces, by analogy with electrolytic conduction. In reality, Thomson admitted gas pre-dissociation only in his theory of 1883, for which the products of dissociation were electrically neutral. In 1888 he had the ions, but denied their existence in gases in normal conditions, even as 'nascent atoms.' In his reply to Schuster, Thomson could simply have explained the evolution of his views. Instead he claimed that pre-dissociation was not essential to his original views, perhaps to avoid self-denial. Schuster immediately countered that Thomson's old explanation of the relation between electric strength and pressure explicitly depended on pre-dissociation. He further reproached Thomson with having no clear, definite picture of the electric current: 'I do not know whether such general considerations [on the relation between current and decomposition] can be fitly described as a theory.'[62]

7.4.6 *The Grotthus chains*

Thomson withdrew from the quarrel. A few weeks after Schuster's second Bakerian lecture, he published an avowedly Helmholtzian discussion of ionic decomposition. The original electric field of the electrodes, he argued, was too weak to overcome the electric attraction between two ions charged with the electrolytic

[61] J. J. Thomson 1888: 397 (quote), 397–400 (general mechanism of conduction), 300–1 (transparency). Maxwell had suggested that the anomalous transparency of electrolytes had to do with the time needed to split the molecules (Maxwell 1873a: #799, #800).

[62] Schuster 1890a: 539; 1890b: 592; J. J. Thomson 1890a, 1890b. Cf. Feffer 1989: 44–5; Mulligan 1997.

quantum. Therefore, the separation of the ions needed to be aided by a special mechanism. Thomson considered two possibilities: there could be electric double layers at the electrodes that enhanced the decomposing field, as Helmholtz and Schuster assumed, or else the molecules of the gas could orient themselves in the external field and form chains in which chemical force could aid the molecular splitting.[63]

Naturally, Thomson preferred the second option, which meant a return to Faraday's old idea of Grotthus chains of decomposition. He soon found supporting evidence. He knew from Wheatstone, and verified with a 50-foot long discharge tube and a rotating mirror, that the velocity of the discharge was of the same order as the velocity of light. This fast propagation could not be due to a motion of the ions at the same velocity, if only because the corresponding kinetic energy would have exceeded the available electric energy. Thomson therefore imagined that the discharge traveled through a series of Grotthus chains, within which the decomposition propagated at nearly the velocity of light. He then attributed the striations of the positive discharge, and the relative independence of the currents in the successive layers (according to Goldstein), to the macroscopic length of the chains.[64]

In 1891 Thomson succeeded, after many unsuccessful tries, in obtaining the gas discharge without electrodes. He surrounded the vessel containing the rarefied gas with a coil fed by a high-frequency current, and observed a brightly luminous ring marking the path of the discharge. The required electric field was no more intense than that required in ordinary discharge tubes. Here Schuster's double layers could not help; only the Grotthus chains could enact the discharge. At least Thomson thought so. Yet he had adopted the central component of Schuster's view: the formation and convection of ions carrying the electrolytic quantum of charge. He confirmed the quantification of charge in the winter of 1891–1892 by repeating old experiments by Adolphe Perrot on the electrolysis of steam and showing that Faraday's law of electrochemical equivalents applied to it.[65]

7.4.7 The unit tubes of force

Thomson did not content himself with a rough admixture of Helmholtzian and Maxwellian ideas. In 1891 he seized Schuster's suggestion of vortex filaments with universal strength and made it the basis of a grand microphysical theory of the electromagnetic field. This theory was the core of his *Recent Researches*, pompously published in 1893 as a sequel to Maxwell's *Treatise*. The sources were Faraday's tubes of force, William Hicks's vortex filament theory of electrostatics, Poynting's idea of their motion and dissolution, Helmholtz's views on electrolysis—and Schuster's physics of ions, although Thomson kept minimizing reference to his

[63] J. J. Thomson 1890c: 360–1 (with a reference to Helmholtz's Faraday lecture on 360).
[64] J. J. Thomson 1890d: 132–140 (Grotthus chains); 1893a: 115–18 (new velocity measurement).
[65] J. J. Thomson 1891b and 1893a: 92–107 (electrodeless discharge); 1893a: 181–5, 559–70 and 1893b (steam). Cf. Feffer 1989: 45–6, 52–3; Davis and Falconer 1997: 83–89.

competitor. Where Helmholtz had atomized electricity, Thomson atomized Faraday's tubes of force.[66]

Thomson's basic entity was the 'unit tube of force,' a thin tube of electric displacement with a strength (flux) equal to the electrolytic unit of charge. Like Hicks's vortex filaments, the tubes could only be closed on themselves or terminate on matter. The surging of a tube from an atom meant a positive charge unit, and its ending on another atom a negative charge unit. Thomson further assumed that the interaction energy between an atom and a tube depended on the chemical element and on the direction of the tube. When two atoms were joined by a tube of length comparable to atomic dimensions, they formed a molecule. When the joining tube was much longer than that, the two atoms formed a pair of free ions. With atoms and unit tubes of force, Thomson explained everything Helmholtz had done in terms of the two electricities, their atoms, and their different attractions to different chemical atoms: chemical affinity, contact electricity, frictional electricity, etc.[67]

Thomson described his theory as 'a kind of molecular theory of electricity, the Faraday tubes taking the place of the molecules in the kinetic theory of gases.' All electric properties of matter and all electromagnetic phenomena were to be deduced from the statistical behavior of the unit tubes of force, as thermodynamics had been deduced from molecular statistics. Thomson imagined that a great number of unit tubes were scattered throughout space and thus imparted a fibrous structure to the ether. He defined the electric polarization $\mathbf{D}$ macroscopically, as giving the excess $\mathbf{D}\cdot d\mathbf{S}$ of the number of tubes passing from the back side to the front side of the surface element $d\mathbf{S}$ over the number of tubes proceeding the opposite way through the same element. In a dielectric, the tubes can neither be destroyed nor created, so that the variations of $\mathbf{D}$ are completely determined by the motion of the tubes.[68]

Suppose for a moment that the tubes all move with the same uniform velocity $\mathbf{v}$. Then the variation of $\mathbf{D}$ at a point moving with the velocity $\mathbf{v}$ is zero:

$$\frac{\partial \mathbf{D}}{\partial t} + (\mathbf{v}\cdot\nabla)\mathbf{D} = \mathbf{0}, \tag{7.1}$$

which is the same as

[66] Schuster 1890a: 559; J. J. Thomson 1891a, 1893a: Ch. 1. Cf. Buchwald 1985a: 49–53, who emphasizes the Maxwellian aspects of Thomson's picture; also Falconer 1987: 259–262. Although J. J. Thomson gave no reference for the analogy between unit tubes and vortex-filaments, he almost certainly borrowed it from Hicks (see note 52). This analogy should not be confused with Helmholtz's old analogy between vortex lines and electric current.

[67] J. J. Thomson 1891a, 1893a: 2–5, 64 (substitute to Helmholtz). In 1895 Thomson illustrated the interaction between a tube end and an atom by the interplay of a vortex filament with gyrostats spinning on the surface of the atom; the only purpose of this illustration was to show that Helmholtz's variable attractions of the two electricities for different chemical elements could be mimicked without electric fluids, as a dynamical coupling between two different motions (J. J. Thomson 1895b).

[68] J. J. Thomson 1893a: 4, 2, 6. The element $d\mathbf{S}$ must be large enough to be cut by a large number of tubes, but small compared with the distances over which the macroscopic fields vary appreciably.

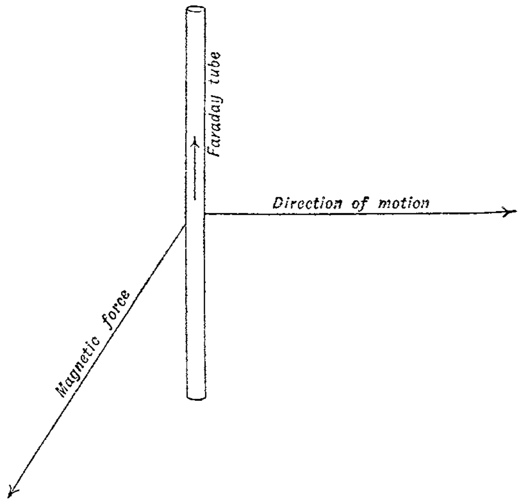

FIG. 7.14. J. J. Thomson's diagram of the magnetic effect of tube motion (J. J. Thomson 1893a: 12).

$$\nabla \times (\mathbf{v} \times \mathbf{D}) = \frac{\partial \mathbf{D}}{\partial t} + \mathbf{v}(\mathbf{v} \cdot \mathbf{D}). \tag{7.2}$$

In conformity with his old work on electric convection, Thomson interpreted the right-hand side of this equation as the total current, sum of the polarization and convection currents. Then $\mathbf{v} \times \mathbf{D}$ meant a magnetic force $\mathbf{H}$ produced by the mere motion of the tubes (Fig. 7.14).[69]

Thomson further ascribed a kinetic energy

$$T = \int \frac{1}{2} \mu H^2 \mathrm{d}\tau \tag{7.3}$$

to the motion of the tubes, and applied a touch of Cambridge-style dynamics. Deriving T with respect to $\mathbf{v}$ and $\mathbf{D}$ gave him, respectively, a momentum $\mathbf{D} \times \mathbf{B}$ for the tube motion, and the electromotive force $\mathbf{E} = \mathbf{B} \times \mathbf{v}$. The latter expression and the divergence-less character of $\mathbf{B}$ implied

$$\nabla \times \mathbf{E} = (\mathbf{v} \cdot \nabla)\mathbf{B} = -\frac{\partial \mathbf{B}}{\partial t}, \tag{7.4}$$

in agreement with Faraday's induction law. Thomson easily generalized these results to more complex motions of the tubes. For example, he regarded the field of a magnet

[69] J. J. Thomson 1891a, 1893a: 6–9 (without the restriction to uniform velocity).

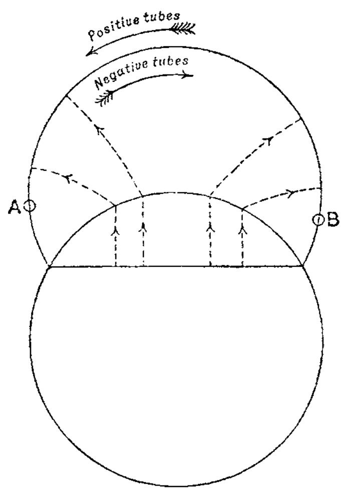

FIG. 7.15. Section of the field of a transversally magnetized cylinder (J. J. Thomson 1893a: 32). The dotted lines represent the magnetic lines of force. The tubes (e.g. A and B) are perpendicular to the drawing plane and parallel to the cylinder. They travel on magnetic equipotential surfaces, as indicated by the arrows.

as involving opposite motions of positive and negative tubes: the contributions of the two kinds of tubes to the polarization **D** cancelled each other, while their motions cooperated in building the magnetic field (Fig. 7.15).[70]

In sum, for Thomson an electric field meant a preferred orientation of the tubes of force, a magnetic field corresponded to a transverse motion of the tubes of electric force, and electromagnetic and electromotive forces derived from the kinetic energy of this motion. In Thomson's hands this ingenious picture led to impressively concise computations of difficult problems of the electrodynamics of moving bodies, such as Rowland's and Röntgen's rotating disk effects, or the convection of an electrified sphere. However, Thomson's reasoning lacked the rigor of Cantabrigian mixed mathematics. He did not care whether or not his dynamics of tubes of force could be given a complete mathematical expression. Quite a few of his quantitative deductions were erroneous, for instance that for the 'Lorentz force.' The true virtue of the unit tubes of force was the broad, unified intuition they gave of a very wide range of phenomena.[71]

[70] J. J. Thomson 1893a: 9–16, 28–32. Thomson was first to ascribe a momentum to the field. He regarded electromagnetic forces as resulting from the flux of this momentum.

[71] J. J. Thomson 1893a: 16–23 (moving sphere), 23–8 (rotating plates). *Ibid*: 36, Thomson gave $(1/3)\mathbf{v} \times \mathbf{B}$ for the Lorentz force, which aggravates the faulty derivation of J. J. Thomson 1881a. He ignored Heaviside's contrary result. Schuster corrected him in print in Schuster 1897.

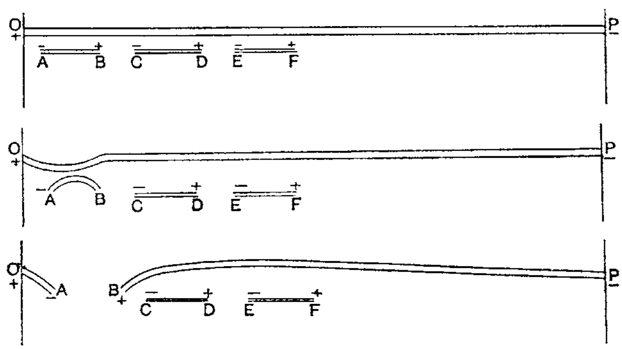

FIG. 7.16. Three stages of the shrinking of a tube of force, first version (Thomson 1893a: 45–46).

7.4.8 *Tube shrinking*

For electric conduction, Thomson borrowed Poynting's general idea of the dissolution of the tubes of force (without the magnetic tubes) and completed it with a microscopic mechanism. He treated the paradigmatic case of gas discharge as follows (Fig. 7.16). As a first step the molecules (AB, CD, EF, . . .) adjacent to a tube of force OP stretching between the electrodes take the orientation of this tube. Then the binding tube of the molecule nearest to the anode (AB) runs up into the tube OP and breaks it into a microscopic tube OA and a long tube BP, giving rise to a molecule OA and to a free ion B^+. The process continues until the tube OP has shrunk to molecular dimensions.[72]

This picture involved ions conceived as atoms connected to a unit tube. However, for Thomson the crumbling of tubes of force, not the motion of the ions, was the essence of the current. This was not the only difference from Schuster's conception. Thomson regarded the seriatim decomposition of the molecules AB, CD, EF as improbable. In conformity with his idea of Grotthus chains, he imagined trains of molecules for which decomposition occurred simultaneously (Fig. 7.17). Among the facts that indicated the existence of such aggregates, he recalled the high velocity of the discharge.[73]

While Thomson's qualitative understanding of the discharge involved tubes of force and molecular aggregates, his more quantitative considerations only required the numbers and velocities of the ions. In 1894 he sketched a kinetic theory of conduction in gases in the spirit of Maxwell's kinetic theory of gases. There he reckoned the electric current as the double flux of free ions under the accelerating effect of the electromotive force and the impeding effect of the collisions. After his discovery of X-ray ionization in 1896, he took into account the variation of the number

[72] J. J. Thomson 1891a, 1893a: 45–6. Cf. Buchwald 1985: 50–3. A similar theory is found in Poynting 1895.
[73] J. J. Thomson 1891a, 1893a: 46–7. Cf. Falconer 1987: 255–6.

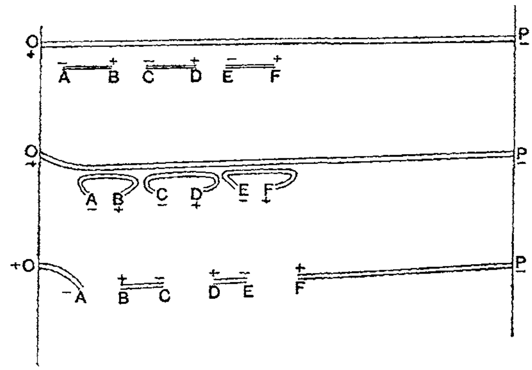

FIG. 7.17. The shrinking of a tube of force, second version (Thomson 1893a: 46).

of ions owing to the competition between molecular decomposition and recomposition during collisions. For example, he wrote

$$\frac{\mathrm{d}N}{\mathrm{d}t} = q - \alpha N^2 - \frac{I}{e} \qquad (7.5)$$

for the variation of the number N of ions when q ions were created, αN^2 recombined during collisions, and I/e were neutralized at the electrodes in unit time. Simple equations of that kind no longer referred to the tubes of force and their dissolution, and could be understood without commitment to a deeper view of electricity. They depended on a small number of parameters that bridged the properties of gases ionized by different causes. The later successes of Thomson and his collaborators in the study of ionized gases much depended on such simple, neutral models.[74]

7.5 The cathode ray controversy

In Schuster's conception of gas discharge, the negative glow played an essential role as the site of molecular dissociation. So did the cathode rays: the repulsion of negative ions by the cathode marked the beginning of the discharge. All other processes, including the positive light, were secondary. Moreover, Schuster regarded the magnetic deflection of the cathode rays as the best opportunity to test the concept of ions carrying the universal quantum of charge.[75]

Thomson agreed with Schuster that the cathode rays were ions projected from the cathode and that their magnetic deflection was an important fact. But he denied, like

[74] J. J. Thomson 1894a: 490–2; Thomson and Rutherford 1896: 395; J. J. Thomson 1898a: 36–9. This theory was a continuation of the kinetic theory of chemical dissociation given in J. J. Thomson 1884b in the context of the vortex model. For its later importance, cf. Lelong 1995: 45–8, 85–8.

[75] Cf. Falconer 1987: 247–8.

E. Wiedemann and Hertz, that the rays and the negative glow played any important role in gas discharge. In his view, the essence of the discharge current was the process of decomposition–recomposition of molecules, not the free flight of ions in the cathode rays. The main part of the discharge was the positive column, which took the shortest route between anode and cathode and occupied the whole space of an electrodeless discharge. In contrast, the negative glow and its rays were inseparable from the cathode, and their spread did not depend on the position of the anode. According to Thomson, Goldstein's observation of the rays in parts of the tube not in line with the cathode signaled the formation of secondary cathodes on the tube walls. Lastly, the magnetic deflection of the rays indicated, if they were negative ions, a velocity at least 1000 times smaller than that of the positive discharge. Naturally, Thomson judged the faster process to be the more important. He concluded: 'Strikingly beautiful as the phenomena connected with the "negative rays" are, it seems most probable that the rays are merely a local effect, and play but a little part in carrying the current through the gas.'[76]

7.5.1 The Lenard rays

New German studies of the cathode rays nevertheless caught Thomson's attention. In 1892 Hertz found that the cathode rays could traverse thin leaves of gold and other metals—which atoms or molecules of matter certainly could not do. The following year, his assistant Philipp Lenard exploited this property to get the rays out of the discharge tube. The trick was to close the end of the tube facing the cathode with a thin aluminum window (Fig. 7.18). Lenard verified that the emerging rays had the properties of the cathode rays, and measured their absorption and magnetic deflection in different gases at various pressures. The range of the rays was much larger than it would have been for British ions: a few centimeters in open air, to be compared with 10^{-5} cm for the mean free path of air molecules. Further, the rays could traverse an excellent vacuum, and their magnetic deflection was the same for every gas at any (reasonably small) pressure, in conformity with the view that they were a 'process in the ether.'[77]

Lenard had more to offer than a confirmation of the ether-wave view of the cathode rays. Having observed the turbidity of the air near the window (Fig. 7.19),

[76] J. J. Thomson 1893a: 113–5 (positive column), 122–4 (Goldstein reinterpreted), 137–8 (velocity of cathode rays); 128 (quote). Cf. Heilbron 1964: 62; Falconer 1987: 247. In his estimate of the velocity of cathode rays, Thomson used Hittorf's magnetic deflection experiments—not Schuster's—even though he was obviously very familiar with Schuster's second Bakerian lecture (cf. the polemic in *Nature* of 1890, and the references in J. J. Thomson 1893a: 108–10, 159). In 1894 Thomson still deplored the lack of quantitative experiments on the magnetic deflection of cathode rays (J. J. Thomson 1894b: 365). After receiving a letter of protest from Schuster, he publicly apologized in the *Philosophical Magazine* (*PM* 40 (1896): 151). Cf. Feffer 1989: 51.

[77] Hertz 1892a; Lenard 1894a (rays in open air, in a vacuum, etc.), 1894b (magnetic deflection). Cf. Lenard 1920: 16–24; Heilbron 1964: 65–6; Dahl 1997: 82–8. Lenard's first attempts to get the rays out of the tube antedated Hertz's discovery and depended on his belief that the rays were some kind of light (cf. Lenard 1920: 14–5). The possibility of the rays in a high vacuum could not be checked within the discharge tube, for the discharge required a minimal pressure.

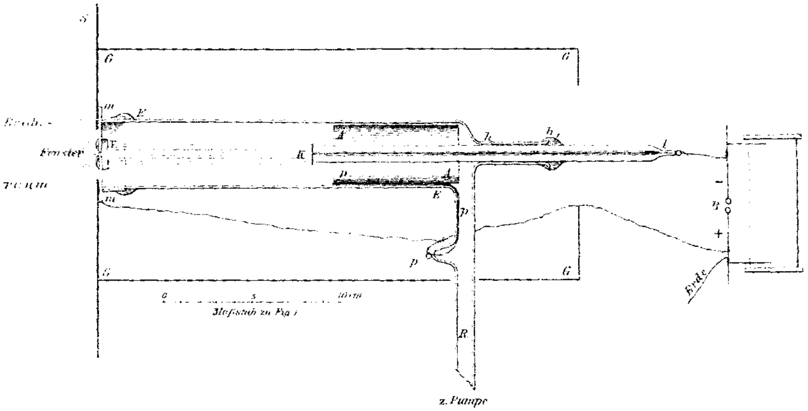

FIG. 7.18. Lenard's window tube (Lenard 1894a). B denotes a Ruhmkorff coil, G + S a grounded Faraday cage, K the cathode, A the anode, 'Beob. Fenster Raum' the space of observation of the rays behind the window. Lenard grounded the window so that it would not act as a secondary cathode.

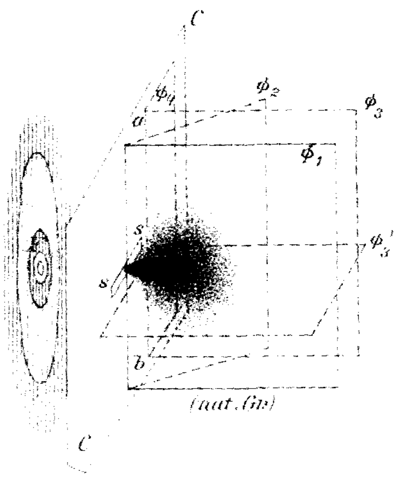

FIG. 7.19. Diffusion of the Lenard rays in open air (Lenard 1894a).

he carefully studied the scattering of the rays and found it to be quite similar to the scattering of light by a colloidal suspension. After confirming the analogy with experiments on diluted milk, he concluded that the wavelength of the cathode rays was so small that the granular structure of matter became apparent. Molecules scattered the cathode rays individually, as colloidal particles did to ordinary light. Lenard further studied the absorption of a given sort of rays (corresponding to fixed conditions in the discharge tube) in various gases at different pressure and in solid foils of variable thickness. The product of the range by the density of the absorbing body turned out roughly constant. For a given gas at different pressures, this result simply resulted from the colloid analogy. It was far more surprising in the case of different substances: together with the colloid analogy, it implied that the scattering by a single molecule was proportional to its mass! In print Lenard refrained from speculating on this strange property. According to his later account, however, he took the alchemical step and imagined that the same *Urstoff* (ultimate matter) was responsible for the scattering in all elements.[78]

Thomson easily accommodated Hertz's experiments on thin metal foils with his usual subterfuge: the formation of a secondary cathode. But he found Lenard's results harder to explain away. Rather than discussing them, he proposed a new proof of the non-ethereal nature of the cathode rays. By the old method of the rotating mirror he measured the velocity of the cathode rays in a hydrogen tube, and found 200 km/s for an accelerating potential of about 200 V. This result matched the value expected for hydrogen ions, and differed by three orders of magnitude from the normal velocity of ether processes (that of light). The supporters of the ether view did not trust this measurement. It may already be mentioned that the velocity of the cathode rays, more correctly measured three years later, turned out to be of the same order as that of light.[79]

7.5.2 X-rays

Thomson's interest in the cathode rays increased considerably in the following year, after the news of Röntgen's spectacular discovery had reached England. On an evening of November 1895, Wilhelm Röntgen was experimenting with a cathode ray tube surrounded with black cardboard, having probably in mind some extension of Lenard's experiments. A sheet of the kind of fluorescent paper used to detect cathode rays was lying at some distance. To Röntgen's surprise the paper glowed strongly even though the cathode rays could not possibly have traveled through the glass wall of the tube, the cardboard, and the large air space. Effects of the gas discharge far from the tube had been noted by several other observers, including Lenard and J. J. Thomson. Röntgen was the only one, however, to suspect a new kind of ray and to investigate their nature more thoroughly. He determined the following properties: the rays were produced during the impact of the cathode rays on the tube walls, they were not deflected by a magnet, they propagated in straight lines, they were able to penetrate thick opaque bodies, and their absorption only depended on the density of the traversed matter. Of the three last properties Röntgen

[78] Lenard 1894a: 235–6 (turbidity), 237 and 259–60 (milk), 250 (absorption law); 266–7 (wavelength); Lenard 1895 (absorption law). On *Urstoff* and *feinere Bestandteile*, cf. Lenard 1920: 24, 47.

[79] J. J. Thomson 1894b. Cf. Heilbron 1964: 67. Thomson later explained the gross inexactitude of this measurement (he should have found a velocity close to that of light) by the delay in the fluorescence of the tube walls: this delay could depend on the intensity of the rays and therefore on the distance from the cathode (J. J. Thomson 1897c: 315).

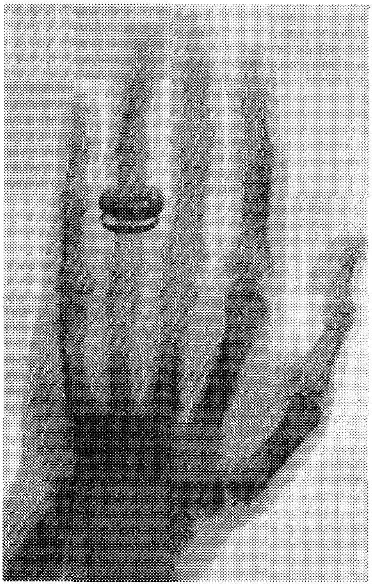

FIG. 7.20. Hand of the anatomy *Geheimrath* at Würzburg (Röntgen 1896a: 13).

offered a stunning demonstration: a photograph of the bones of a human hand (Fig. 7.20).[80]

X-rays surely were the most sensational discovery of nineteenth century physics. Among the numerous physicists who took up their study, J. J. Thomson was prominent. He quickly established that the passage of the rays through a gas turned it temporarily into an excellent conductor. In conformity with his and Schuster's ideas on conduction in gases, he traced this effect to the formation of ions, for which he coined the verb 'to ionize.' He immediately understood that he had in hand a way to produce ions in perfectly controlable conditions. With John McClelland he established the existence of a saturation current, which he originally interpreted in terms of an alignment of his dear Grotthus chains. A few weeks later, he adopted Giese's explanation of the similar effect in heated gases as a balance between the supply and the removal of ions. In conformity with this view, Thomson's new collaborator Ernest Rutherford observed that the passage of a current diminished the conductivity of the ionized gas. These researches opened a new, fruitful field in which Thomson's kinetic theory of the ionic current was developed and tested.[81]

[80] Röntgen 1895, 1896a, 1896b. Cf. Glasser 1959: 1–23, for a carefully documented history of X-rays; Whittaker 1951: 357–8; Heilbron 1964: 68–72, who also discusses anticipations, and explains the timing of Röntgen's discovery (e.g. Lenard's window-tube focused the experimenter's attention on processes outside the discharge tube).

[81] J. J. Thomson 1896a; Thomson and McClelland 1896; Rutherford and Thomson 1896. Cf. Whittaker 1951: 359–60; Glasser 1959: 264–7; Heilbron 1964: 71; Falconer 1987: 255–9; Davis and Falconer 1997: 114–21; Dahl 1997: 115–21.

Naturally, Thomson was also interested in the nature of X-rays. Röntgen originally believed that he had found the long-sought longitudinal vibrations of the optical ether. A month later, Schuster propounded that the new rays were light of extremely high frequency, ultra-ultraviolet light produced during the collisions of the cathode ray ions with the walls of the tube. J. J. Thomson endorsed this interpretation, which soon gained general acceptance. On the occasion, he explained why the X-rays did not share some of the characteristic properties of light. For example, they were not refracted because, according to Helmholtz's ionic theory of dispersion (to be discussed later), the refractive index of any substance went to *one* for ultra-high frequencies. Most interesting was Thomson's suggestion to explain why the absorption of a given sort of X-rays depended on the density of the absorbing matter only: he imagined a Rayleigh scattering of the rays on 'primordial atoms' that made up all matter, something like Prout's protyle. This inspiration probably came from Lenard's cloud analogy for cathode ray absorption. As we saw, Lenard kept his more alchemical thoughts for himself. Thomson, who had once tried to build up chemical atoms with vortex rings, had no such inhibition.[82]

7.5.3 A dilemma

The cathode rays became, in Thomson's words, 'the parents of the Röntgen rays.' As such they were worthy of increased attention. In his presidential address for the British Association meeting of September 1896, Thomson reviewed the evidence in favor of the corpuscular view, to which he added Jean Perrin's electrostatic proof of the electric charge of the rays. He also returned to Lenard's window experiments, and offered a reason why the magnetic deflection of the rays did not depend on the nature of the gas. The impact of the cathode rays on the window produced X-rays that traversed the window and ionized the gas beyond it; at each pulse of the induction coil feeding the discharge tube, an electric impulse was communicated to the window, and this impulse imparted a definite *momentum* p to the negative ions on the external surface of the window; then the magnetic deflection, eB/p, did not depend on the ion mass.[83]

This explanation was somewhat contrived. Moreover, the large penetration of the Lenard rays remained incompatible with the corpuscular view. Lenard, who attended the BA meeting, had an opportunity to convince Thomson of the force of his arguments. Subsequently, Thomson measured the magnetic deflection of a beam of cathode rays *within* the tube, and found it to be independent of the residual gas (Fig.

[82] Röntgen 1895: 11; Schuster 1896: 268 (with the comment: 'If Röntgen's rays contain waves of very small length, the vibrations in the molecules which respond to them would seem to be of a different order of magnitude from those so far known. Possibly we have here the vibration of the electron within the molecule, instead of that of the molecule carrying with it that of the electron'); J. J. Thomson 1896a: 582; 1896b; 1896c: 304–5. Cf. Whittaker 1951: 358; Glasser 1959: 261–4; Falconer 1987: 265 (primordial atoms).

[83] J. J. Thomson 1896c: 302 (parents); 1896b: 701–2 (on Perrin), 702–3 (impulse); 1897b (impulse and magnetic deflection); Perrin 1895. FitzGerald and Stokes proposed explanations of the Lenard rays that were similar to Thomson's: cf. Falconer 1987: 247–50.

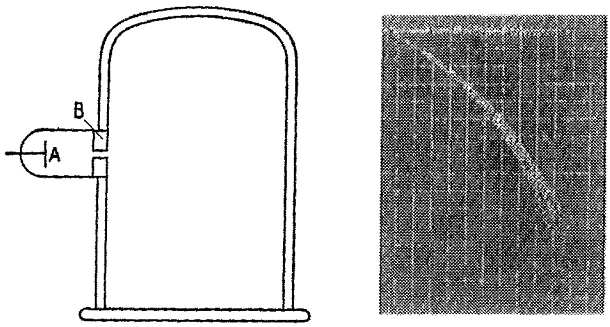

FIG. 7.21. J. J. Thomson's device for showing the magnetic deflection of cathode rays (J. J. Thomson 1898a: 152, 154). A denotes the cathode, B the anode. The magnet is not represented.

7.21). Here the trick of a definite momentum could not do. Yet Thomson remained convinced that the magnetic deflection indicated a flux of electrified particles. He eliminated any doubt on this point by improving Perrin's experiment. In the latter experiment, a Faraday cylinder connected to an electrometer was placed in the axis of the cathode (Fig. 7.22(a)). As Thomson realized, the experiment would have been worthless to a Hertz or a Wiedemann, because the electric current in the tube could account for the charge accumulated in the cylinder, even if the cathode rays did not participate in this current. Thomson therefore used a tube (Fig. 7. 22(b)) in which the beam of cathode rays could not reach the Faraday cylinder, unless it was curved by approaching a magnet. He thus isolated the impact of the cathode rays as the cause of the deflection of the electrometer.[84]

The dilemma had now reached its full force: the rays could only be electrified particles, and yet their magnetic deflection was independent of the residual gas. Thomson's first inspiration was to exploit the molecular agglomerates he had abundantly used in his explanation of the discharge current. The cathode rays could be made of such agglomerates, set to a constant charge-to-mass ratio. At first glance, the assumption also explained the macroscopic penetration of the cathode rays in open air, because a massive particle would be little disturbed by the impact of the air molecules. Thomson soon dismissed this possibility, however, for it made the absorption a function of the viscosity of the gas (which hardly depends on pressure).[85]

7.5.4 The corpuscle

The only left-over possibility was 'a somewhat startling assumption': the particles of the cathode rays were much smaller than atoms. In order to explain Lenard's

[84] J. J. Thomson 1897a (8 February). For comparisons of Hertz's, Perrin's, and Thomson's experiments on the electric charge of the rays, cf. Lenard 1920: 79–84; Buchwald 1994: 158–66.

[85] J. J. Thomson [1896d]; 1898a: 197. Cf. Falconer 1987: 265. Thomson held the agglomerate conception of the cathode rays even before his magnetic deflection experiments.

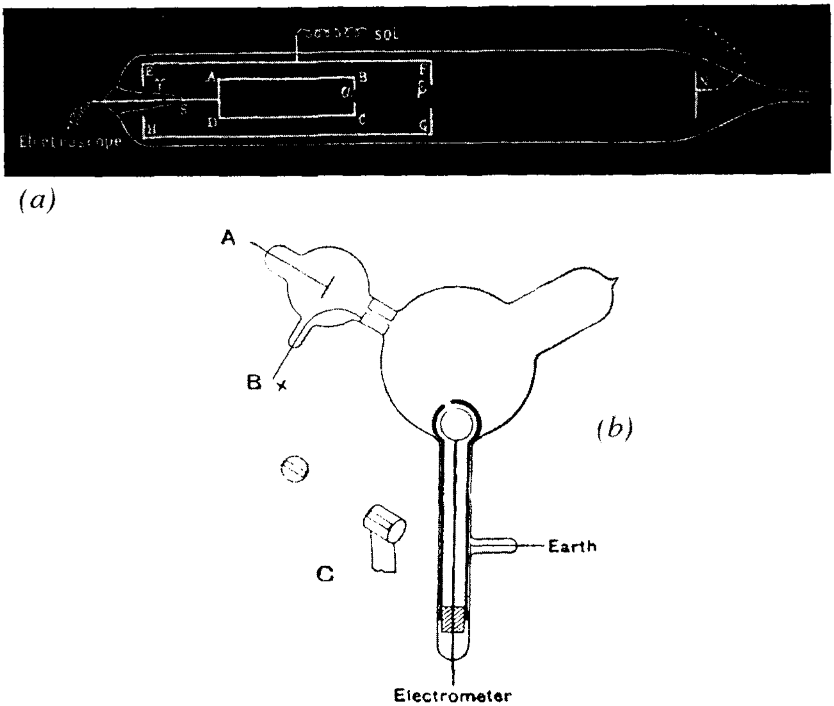

FIG. 7.22. Perrin's (a) and J. J. Thomson's (b) tubes for detecting the electric charge of cathode rays (Perrin 1895; J. J. Thomson 1897c).

absorption law, Thomson further assumed that all atoms were clouds of such particles, all identical to each other: then the absorption only depended on the total number of particles per unit volume of the gas, which was proportional to the density. Here Thomson was transposing his previous Lenardian explanation of the similar law for X-rays. The main difference was that the absorbed rays now were the very constituents of the absorbing atoms, projected from the cathode after some sort of atomic explosion. Thomson called 'corpuscle' the new, subatomic particle. He already dreamt of atoms made of regular arrangements of corpuscles. Perhaps chemical valence depended on the stability of such configurations, as he illustrated with a system of floating magnets.[86]

[86] J. J. Thomson 1897b: 430–1; 1897c (floating magnets). Cf. Whittaker 1951: 360–1; Heilbron 1964: 81–82, who first saw the crucial importance of Lenard's absorption results; Falconer 1987: 267–71 for excluding reconstructions of Thomson's corpuscle that give weight to Larmor's theory or to the Zeeman effect; Heilbron 1976: 367 and Kragh 1997 for Alfred Mayer's floating magnets and Thomson's use of them in the contexts of vortex atoms and corpuscles. Thomson also referred to Norman Lockyer's spectroscopic arguments in favor of subatoms (J. J. Thomson 1897b: 431): cf. Heilbron 1964: 19–20; Falconer 1987: 267–8; Kragh 1997. By accepting Lenard's absorption law, Thomson implicitly admitted the transparency of his window to the cathode rays; the impulse theory of the Lenard rays thus became purposeless.

In order to consolidate his corpuscle, Thomson returned to the magnetic deflection of the cathode rays. He knew that the deflection was the same in every residual gas, and he could precisely measure it. In order to extract the charge-to-mass ratio from this measurement, the velocity of the rays needed to be known. As every expert agreed, a determination from the accelerating potential of the rays was not reliable, because part of the kinetic energy thus gained could be absorbed by the residual gas, and also because this potential varied in time, especially when the tube was fed by an induction coil. Neither could Thomson rely on his own previous determination with the rotating mirror, which led to a charge-to-mass ratio of the same order as that of ions. He now admitted that a delay in the phosphorescence of the tube wall could have spoiled this measurement.[87]

In the early spring of 1897, Thomson tried a new method based on measuring the heat and electric charge produced during the impact of the rays on a Faraday cylinder. These two measurements, combined with the magnetic deflection of the same rays gave him a charge-to-mass ratio some 1600 times larger than that of the hydrogen ion. This result was not in itself a sufficient proof of a small mass: it could alternatively be explained by a large charge, and it depended on a delicate calorimetric measurement. However, Thomson judged that 'in conjunction with Lenard's results' the numbers favored a particle of much smaller mass than the hydrogen ion. He also noted that the effect of a strong magnetic field on the sodium lines, recently discovered by Pieter Zeeman, indicated a charge-to-mass ratio of the same order. A few months later he greatly improved his determination of the charge-to-mass ratio by combining electrostatic (Fig. 7.23) and magnetic deflection.[88]

Thomson first announced the discovery of the 'corpuscle' at an evening meeting of the Royal Institution on 30 April 1897, and in a more complete form in the October issue of the *Philosophical Magazine*. He hoped to settle the controversy about the nature of cathode rays, and, more ambitiously, to start a new physics based on a new fundamental building block. At that time, he believed the corpuscle to be a material particle whose mass was only partly electromagnetic. The mass (e^2/a) and the charge-to-mass ratio (a/e) of a purely electromagnetic particle, he argued, would have been essentially variable, for the radius a was arbitrary. Thomson also rejected the connection between the charge of the corpuscle and the electrolytic quantum of electricity. He could not ascribe the charge of an ion to the included corpuscles, since all corpuscles carried a negative electrification. Moreover, he believed that the charge of the corpuscle had to be a large multiple of the electrolytic quantum in order to explain the additivity of the dielectric permittivity of chemically combined gases.[89]

[87] See note 78 above for Thomson's denial of his previous velocity measurement.

[88] J. J. Thomson 1897b: 431–2 (heat and charge), 432 (quote, Zeeman); 1897c (electric and magnetic deflections). Cf. e.g. Whittaker 1951: 362–3; Anderson 1964: 42–6; Davis and Falconer 1997: 123–9; Dahl 1997: 158–74. Some 15 years earlier, Eilhard Wiedemann had already measured the heat produced by the impact of cathode rays, and had used the result against Crookes: see above, p. 284.

[89] J. J. Thomson 1897b, 1897c: 311 (material mass), 312–3 (charge). Cf. Heilbron 1964: 83; Feffer 1989: 59. If, Thomson reasoned, the electric moment of a molecule were simply determined by the separation of its ions, the additivity law could not be understood; there had to be large contributions from the constitutive atoms, which seemed to require a high charge of the corpuscles.

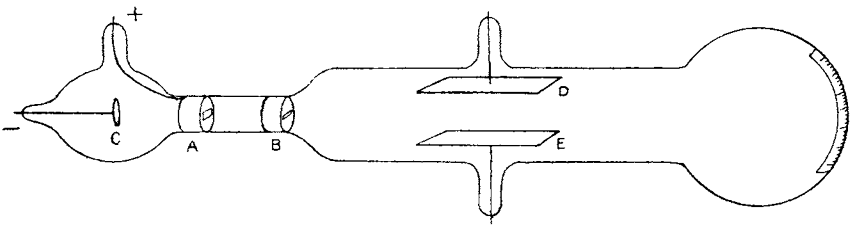

FIG. 7.23. J. J. Thomson's tube for the electrostatic deflection of cathode rays (J. J. Thomson 1897c). The deflecting potential is applied to the plates D and E.

7.5.5 *Corpuscle versus electron*

Thomson's invention of the 'corpuscle' was not an isolated phenomenon. In the next chapter, we will see that Emil Wiechert had already found in the cathode ray a new particle that had more in common with our modern electron than Thomson's corpuscle. We will also see that FitzGerald and Larmor had their own concepts of 'the electron.' These physicists agreed that a negatively charged particle, with a mass much smaller than the hydrogen ion, had been found in the cathode rays. Within a year or so, the ether wave theories of the cathode rays died away. Even Lenard, who had been the staunchest supporter of the ether view, declared the cathode rays to be 'special, undetected ether particles which are individually moveable, which possess mass (inertial), and which also appear as charge carriers.' As this quote already suggests, there were important disagreements on the wider significance of the new particle.[90]

The electron of Larmor, Wiechert, and FitzGerald was a singularity in the ether with a purely electromagnetic mass and carrying the electrolytic quantum of charge; any variation of electric charge meant a transfer of electrons. For Larmor and Wiechert, electrons were also the basic constituents of matter, as Thomson's corpuscles were. For most other physicists, including FitzGerald, speculations on the inner constitution of atoms were premature and bordered on the alchemical; the emission of an electron by an atom did not imply a dissociation; it only meant a change of electrification. J. J. Thomson rejected this view of the 'disembodied electron' for at least three reasons. His concept of electric charge involved the abutting of tubes of force on matter, and thus excluded disembodied electricity. His concept of the corpuscle depended on Lenard's absorption law, which indicated the existence of a universal building block of atoms. And he regarded the explanation of chemical atoms as the highest aim of physics.[91]

[90] Lenard 1898: 279–80. Cf. Heilbron 1964: 84; Falconer 1989: 271–3. On the extended, collective, and constructive character of the 'discovery of the electron,' cf. Arabatzis 1996; Lelong 1997; Darrigol 1998.

[91] Cf. Falconer 1987: 273–4; Feffer 1989: 59–60; Lelong 1997: 105–10. In France, Paul Villard maintained until 1905 the anti-atomist interpretation of the cathode rays as charged particles of hydrogen: cf. Lelong 1997.

During the last years of the century, Thomson and his collaborators determined by cloud chamber measurements that the charge of the corpuscle was identical to the electrolytic charge quantum and that the same corpuscle was produced in many other circumstances, including the photo-electric effect, the thermionic effect, and radioactivity. Thomson also admitted that the mass of the corpuscle could after all be purely electromagnetic. This made the corpuscle more similar to the electron of other physicists. Yet Thomson retained the name 'corpuscle' for most of his career. Perhaps he wished to indicate that beyond the narrow issue of the electron, his style of physics differed from that of other leaders of the growing microphysics.[92]

7.5.6 *The new Cavendish style*

Thomson's main achievement was not so much the discovery of a new particle, for which no single contributor or date can be given. Rather, Thomson's corpuscle consecrated the birth of a new Maxwellian microphysics. This meant, on the theoretical side, the marriage of Helmholtz's and Schuster's ions with Faraday's tubes of force, and the development of simple models with few measurable parameters for the evolution of ions in various circumstances. While emphasizing the pedagogical virtue of his tubes and models, Thomson denounced the traditional 'loads of learning' of the Cambridge system, which could 'crush the enthusiasm of students' before they had a chance to perform experiments. By the latter activity Thomson did not mean the rigorous discipline of precision measurement championed by his predecessor. He reoriented the Cavendish Laboratory toward the exploration of the new physics of ions and radiations. In this context, visual displays and orders of magnitude counted more than accuracy. Although the older techniques of electric measurement, thermometry, etc. were still useful, they were now subordinated to newer techniques of glass blowing, high vacuum, cloud chambers, etc. With contagious enthusiasm, Thomson taught this new physics to a number of visitors attracted by the revealed mysteries of matter—and by a new system of fellowships. The new Cavendish style quickly diffused through the world of physics.[93]

7.6 Conclusions

The notion of electrically charged atoms or molecule parts first emerged in the field of electrolysis. Against the dominant chemical ideas, Hittorf and Kohlrausch gave quantitative proofs of the independent migration of the two ions of a solute (in the macroscopic sense). One aspect of this independence was that electrolytic conduction had nothing to do with chemical affinity and satisfied Ohm's law even for

[92] J. J. Thomson 1898b, 1899. Cf. Heilbron 1964: 84–5; 1976: 367; Feffer 1989: 60–1. On charge measurement, cf. Robotti 1995; Davis and Falconer: 129–34; Dahl 1997: 175–89. Thomson 1898b: 541 referred to Lorentz's indication that the charge of optical ions equalled the electrolytic quantum, based on dispersion and the Zeeman effect (see Chapter 8, p. 331).

[93] Cf. Falconer 1989 for the reorientation of the Cavendish; Lelong 1995: Ch. 2, and Kim 1995 for the diffusion of the new style.

very small electromotive forces. Clausius interpreted this fact in terms of a pre-dissociation of the molecules. By analogy with the kinetic theory of gases, he imagined a thermal motion of the molecules of the solute and collisions that split them into electrically charged parts. The role of the external electromotive force was to superpose a drift of these charged parts over the thermal motion. For a long time Clausius and his followers only admitted a partial pre-dissociation of the molecules. When, in the late 1880s, Arrhenius argued for a nearly complete dissociation in dilute solutions, he still met considerable opposition. Nonetheless, the electrified atoms and molecule parts—now called ions—had become the central concept of German electrochemistry.

Helmholtz gave to the atomistic concept of ion its most precise form. In his 'fully comprehensible nature' of 1847, all chemical and electrochemical phenomena were reduced to variable attractions of the two electricities for different chemical elements. On the one hand, this view justified the validity of thermodynamic principles through which chemical heats and voltaic forces could be related. On the other hand, Helmholtz combined it with Faraday's law of electrochemical equivalents to introduce the notion of molecule parts carrying a fixed quantum of charge, or integral multiples of it. During his electrochemical investigations of the 1870s, he no longer believed in the literal existence of electric fluids. However, he still argued that 'atoms of electricity' bound to chemical atoms or molecule parts with specific energies provided the central picture of electrochemistry. With this clear, definite notion, he explained the processes occurring at the interface between two different conductors in terms of 'double layers' of electric atoms or ions. He thus resolved to his satisfaction the old conflict between the chemical and the contact theory of the galvanic cell, and explained a wide range of newer electrochemical phenomena.

Maxwell and his followers criticized the notions of free ions, atoms of electricity, and electric double layers as unphilosophical regressions to the old electric fluid or fluids. They questioned the independent migration of ions and the pre-dissociation theory, despite Maxwell's sympathy for Clausius's kinetic theoretical views. And they fought Helmholtz's form of the contact theory of galvanic cells, which William Thomson supported. Having no alternative molecular theory of galvanism to offer, they lost the lead in electrochemistry to the Germans. They hoped, however, that the future would bring a more philosophical view of the electrolytic current and thereby shed light on the general process of electric conduction.

Faraday had long before expressed the same hope, both for electrolysis and for electric discharge through gases. In the 1860s and 1870s, the most successful explorers of the latter field were his German admirers Plücker and Hittorf. In gases rarefied with Geissler's pump, they observed the magnetic and electric properties of the discharge. Plücker established that the discharge behaved like ordinary currents with respect to magnetic deflection. Hittorf found the similarity to extend to the conduction process near the positive electrode. However, in the vicinity of the cathode he observed a surprising ray propagation of the discharge, which Goldstein later called 'cathode rays.' By patient electric measurements, Hittorf also demonstrated several ways in which the gas discharge violated Ohm's law (besides the long known electromotive threshold), for instance a sharp potential drop near the cathode, and a very low resistance of the gas to the passage of a *second* discharge.

Although they repeatedly referred to Faraday's hope that discharge in rarefied gases would reveal the essence of dielectric disruption, Plücker and Hittorf avoided speculation on the mechanism of gas discharge. The first attempts of this kind ignored their works and flatly contradicted Faraday's intuition. In the early 1870s, G. Wiedemann and Rühlmann imagined that charged molecules as well as Weber's particles of electricity were projected from the electrodes. In 1879, Crookes and Stokes similarly proposed that the cathode rays were torrents of negatively charged molecules projected from the cathode. In the early 1880s Goldstein and E. Wiedemann rejected these corpuscular views because they were incompatible with the very high velocity of the discharge and seemed impotent to explain its complex visual appearance. They proposed alternative theories in which the discharge current was a longitudinal ether wave. For E. Wiedemann, as for Faraday, the polarization of material particles was essential to the formation of the current. In contrast, for Goldstein vacuum was a perfect conductor, and matter could only impede the formation of the current.

Ether theorizing was not a German forte. Goldstein's views were too fantastic to be taken seriously by any major theorist, and Wiedemann's too loose and vague to have more than a descriptive value. However, their polemic with Crookes attracted the attention of two first-class British physicists. One was the German-born Schuster, who in 1884 transferred Helmholtz's electrolytic concepts of ions and double layers to gas discharge. In his ingenious theory, conduction in gases involved the dissociation of gas molecules into ions and the convection of the latter in the electric field; the obvious differences from electrolytic conduction, including the violations of Ohm's law, resulted from the great difficulty of producing ions in attenuated matter. Schuster also brought direct experimental proofs of ionic dissociation. In Germany, Helmholtz's disciples provided further evidence that conduction in gases always involved the production and convection of ions. These works marked the beginning of a new microphysics in which the electrodynamic properties of matter were reduced to the interactions of ions.

The other British star physicist who took up the study of gas discharge was J. J. Thomson, a wrangler and a devoted Maxwellian. Like Schuster, Thomson developed an analogy between electrolysis and gas discharge, and regarded chemical dissociation as essential to both kinds of current. However, to him the products of molecular dissociation were neutral atoms or molecular parts, and the electric current was the dissipation of the electric ether strain, not the convection of molecule parts. Thomson developed this Maxwellian view on the basis of the vortex ring model of atoms and molecules. In this model, on which he had written a prize-winning essay, there was no place for the notion of electrified atom.

Around 1888 Thomson became aware of the empirical superiority of Schuster's ionic theory of discharge. He was then willing to give up the vortex atoms. But he could not simply admit Helmholtz's ions: this would have contradicted the basic Maxwellian tenet of the primacy of ether processes. He solved the crisis in 1891 by redefining an ion as the abutting of a unit tube of electric force on an atom or a group of atoms. This picture allowed him to transfer the successes of Helmholtz's matter-bound atoms of electricity without betraying the Maxwellian reduction of electric charge and conduction to ether processes. Charge was the ending or surging of ether tubes, conduction their shrinking. Thomson also replaced Schuster's double layers with Faraday's old notion of Grotthus chains, which various properties of the discharge seemed to favor.

Meanwhile, the disagreements on the nature of cathode rays persisted. Goldstein regarded them as current-carrying ether waves, E. Wiedemann as ultra-ultraviolet light, Hertz as longitudinal waves in the optical ether, Schuster and Thomson as negative ions projected from the cathode. Each expert had excellent reasons to maintain his position. No one studied the rays for themselves, except Hertz, who suspected an entirely new entity, neither ordinary light nor electric current. In 1894 his assistant Lenard succeeded in getting the cathode rays out of the discharge tube, and found them to behave quite differently from Schuster's ions. While repeating this experiment (presumably), Röntgen made his spectacular discovery of X-rays. Thereafter physicists payed more attention to their 'parents,' the cathode rays.

Thomson consolidated the proofs of the corpuscular nature of the cathode rays. But he could not make sense of the high penetrability of these rays demonstrated by Lenard, until he suspected that the involved 'corpuscles' were much smaller than ions. In 1897, he and Wiechert provided proofs that the charge-to-mass ratio of the cathode ray particles was about 2000 times larger than that of the hydrogen ion. Physicists soon agreed about the corpuscular nature of the cathode rays and about the existence of an electrically charged particle of subatomic size. However, they disagreed on the role of this particle in the general economy of physics. For Thomson, the new particle was the fundamental building block of all matter. For other theorists, it was a materialization of the quantum of electric charge.

This split exploitation of the new particle reflected the existence of two varieties of the raising physics of ions. The one founded by Schuster and Thomson focused on electric conduction in electrolytes and gases, and on the structure of matter. That of Lorentz, Larmor, and Wiechert—to which the next chapter is devoted—sought to improve Maxwell's synthesis of optics and electromagnetism. Both led to compromises between continental and British electrodynamics. In the last years of the century they gradually converged around a unified concept of the electron. The special importance of Schuster's and Thomson's contributions was their anchoring of the new micro-electrodynamics in firm empirical ground. As the head of the Cavendish Laboratory, Thomson distanced himself from the previous discipline of precision measurement and developed the necessary techniques to study conduction in rarefied gases and the properties of the new radiations. German physicists like Lenard, Kaufmann, and Stark also excelled in this new physics. However, Thomson and the Cavendish Laboratory were most efficient in training newcomers and diffusing a coherent set of experimental and theoretical techniques.

8

The electron theories

8.1 Introduction

For many of Maxwell's readers, his most impressive success was the unification of optics and electromagnetism. Yet Maxwell had left this topic in a very incomplete state. For optical refraction, dispersion, magneto-optical effects, and the optical behavior of moving bodies he had no fully electromagnetic explanation. He judged that these phenomena required a more detailed consideration of the molecular structure of matter, but performed little work of that kind. As we saw in the previous chapter, some of Maxwell's heirs, especially Lodge and J. J. Thomson, did introduce atomistics in Maxwell's theory. But they were mostly concerned with the mechanism of electric conduction. In optics and magneto-optics, the Maxwellians favored the macroscopic approach that was dominant in the *Treatise*. The physicists who first developed atomistic considerations in this domain either lived on the continent (Lorentz, Helmholtz, and Wiechert), or disagreed with important aspects of Maxwell's theory (Larmor). Toward the end of the century, their efforts converged toward a new, microphysical approach of electromagnetism which Larmor called 'the theory of electrons.' This chapter recounts the multiple formation of this theory.[1]

8.2 Some optics of moving bodies

One major concern of the early electron theorists was the optics of moving bodies. A proper history of this topic, from James Bradley's discovery of stellar aberration to the Michelson–Morley experiments, should take into account the astronomical context as well as the debates on the nature of light. Here we will only retain the aspects of this history that were relevant to the evolution of electrodynamics.[2]

[1] See Chapter 5 for Maxwellian optics and magneto-optics. 'Theory of electrons' belongs to the title of Larmor 1895a.

[2] On the history of the optics of moving bodies, cf. Ketteler 1873; Mascart 1893, Vol. 3, Ch. 15; Larmor 1900a, Ch. 2; Schaffner 1972; Hirosige 1976; Buchwald 1988; and the authoritative survey by Janssen and Stachel [1999].

8.2.1 Fresnel versus Stokes

The oldest known effect of motion in optics is the aberration of stars: the apparent position of a star in the sky is obtained by combining the velocity of the light from this star with the velocity of the Earth with respect to the fixed stars. In his undulatory theory of the 1820s, Augustin Fresnel regarded this effect as a consequence of the motion of the Earth through a stationary ether. More exactly, he assumed that the ether remained at rest in a vacuum, but that in a moving material body the ether excess (needed to explain refraction) was carried along by the body. Accordingly, dilute moving matter, such as the Earth's atmosphere, left the course of light unchanged; but denser matter moving in the direction of the light waves conveyed a fraction $1 - 1/n^2$ of its velocity to the waves, if n is the optical index of the body. This partial dragging explained Arago's earlier observation that the refraction of light was unaffected by the motion of the Earth through the ether (see Appendix 11).[3]

Fresnel's theory did not remain unchallenged. George Stokes found it hard to imagine that the huge mass of the Earth would be permeable to the ether. In 1845, he assumed the absence of any ether wind near the surface of the Earth. Then the Earth's motion had no effect at all on optical experiments with terrestrial sources. However, Fresnel's simple explanation of stellar aberration no longer applied. Stokes required the velocity of the ether (with respect of the fixed stars) to be irrotational, so that the path of luminous rays was the same as if the ether were stationary. Stokes's theory was most popular in Britain. A report on optical theories written in 1885 for the British Association still played down 'Fresnel's somewhat violent assumptions on the relation between the ether within and without a transparent body.' By that time some British physicists may have realized that Maxwell's electromagnetic theory of light implicitly supported Stokes's complete ether drag, for it idealized ether and matter as a single medium with variable macroscopic properties.[4]

In 1851 Hippolyte Fizeau had announced a confirmation of the Fresnel drag, based on interposing moving water in a double-beam interference device (Fig. 8.1). Stokes's theory could be modified to include this result, but at the price of becoming more complex than Fresnel's. Maxwell himself admitted that Fizeau's result, if it were true, favored Fresnel's theory of the ether. However, the confidence in this delicate experiment was not so high beyond the Channel. In 1878 Maxwell still judged Stokes's theory 'very probable,' though with proper reserve: 'The whole question of the state of the luminiferous medium near the Earth,

[3] Bradley 1728 (aberration); Fresnel 1818. Cf. Whittaker 1951: 94–5, 109–10; Hirosige 1976: 6–9; Mayrargue 1991.

[4] Stokes 1845b, 1846a, 1846b; Glazebrook 1885 (BASS report). Cf. Wilson 1972; Hirosige 1976: 10–12; Whittaker 1951: 386–7; Buchwald 1988: 56–7. Glazebrook did not comment on the implications of the electromagnetic theory of light for the optics of moving bodies. Heaviside (1889a: 519–521), Lodge, and FitzGerald (letters of April 1891, commented in Hunt 1991a: 203) agreed that Maxwell's theory implied a complete drag, against J. J. Thomson 1880 who found a dragging coefficient of half (in good agreement with Fresnel's value for water) by a naïve combination of the equations of Maxwell's *Treatise*.

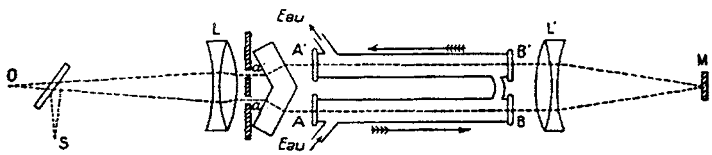

FIG. 8.1. Fizeau's experiment of 1851 to measure the dragging of light waves by moving water (from Mascart 1893, Vol. 3: 101). After reflection by a glass plate, the beam from the source S crosses the lens L, the double-hole diaphragm $\alpha\alpha'$, and the V-shaped double blade to result in two parallel pencils of light. One pencil travels along ABMB′A′, in the direction of the water flow; the other along A′B′MBA, against the water flow. They meet again and interfere in O.

and of its connexion with gross matter, is very far as yet from being settled by experiment.'[5]

On the continent, the Fresnel drag was usually regarded as an established fact. In the early 1870s, Wilhelm Veltmann and Eleuthère Mascart proved that the Fresnel drag, together with Huygens' and Doppler's principles, implied the insensitivity of terrestrial optics to the Earth's motion to first order in u/c, where u is the velocity of the Earth with respect to the ether and c the velocity of light. Mascart accompanied his demonstration with many careful experiments that denied previous claims to the contrary. His general conclusion read:

The translational motion of the earth has no appreciable influence on optical phenomena produced by a terrestrial source, or light from the sun, [so] these phenomena do not provide us with a means of determining the *absolute* motion of a body, and *relative* motions are the only ones that we are able to determine.

Mascart further noted that Fresnel's and other's explanations of the Fresnel drag in terms of the excess ether in transparent bodies were no longer possible. For example, the invariance of double refraction experiments required two different dragging coefficients for the two refracted beams. An adequate ether-based theory of the Fresnel drag had yet to be given.[6]

8.2.2 *The Michelson–Morley experiments*

The situation grew more complex in the 1880s, when the American physicist Albert Michelson performed experiments to settle the issue of ether motion. In the first trial,

[5] Fizeau 1851; Maxwell to Huggins, 10 June 1867, *MSLP* 2: 311; Maxwell 1878: 769, 770. In 1864 Maxwell erroneously believed that the Fresnel–Fizeau drag implied an effect of the Earth's motion on the deviation of light by a prism, and performed an experiment to test this effect (Arago's prism experiment, with a terrestrial source), with a negative result; Stokes soon corrected his calculation: cf. Harman 1995a: 9–10.

[6] Mascart 1872, 1874: 420; Veltmann 1870a, 1870b, 1873. Cf. Newburg 1974; Pietrocola Pinto de Oliveira 1992; Hirosige 1976: 18–22. Maxwellian physicists do not seem to have been aware of the difficulty with Fresnel's explanation of the drag, as is seen in Glazebrook 1885: 182, and in Lodge 1893.

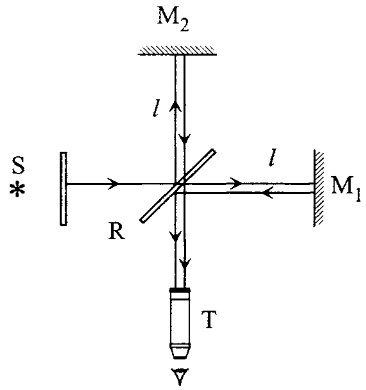

FIG. 8.2. Michelson's interferometer schematized.

performed in Potsdam in 1881, Michelson used an interferometer of his own (Fig. 8.2) to test a second-order effect of the Earth's motion with respect to the ether. In this device, the light from the source S is divided into two beams by the semi-reflecting plate R. After reflection on the mirrors M_1 and M_2, the two beams meet again at R and the resulting interference pattern is observed through the telescope T. Suppose that one arm of the interferometer is parallel to the velocity **u** of the Earth with respect to the ether. According to Michelson's analysis of 1881, the effect of this motion through the stationary ether is to increase the duration of the round trip of the light in this arm by a factor $[l/(c - u) + l/(c + u)]/(2l/c)$, which is equal to $(1 - u^2/c^2)^{-1}$. Michelson's device was sensitive enough to detect half of the corresponding fringe-shift. From the null result, he concluded that Fresnel's theory of aberration had to be abandoned.[7]

A year later, Michelson learned from Alfred Potier that he had overlooked the effect of the motion of the other arm of the interferometer: the round trip of the light in this arm is increased by a factor $(1 - u^2/c^2)^{-1/2}$. This effect reduces the expected fringe-shift by one half, and the accuracy of the Potsdam measurement becomes insufficient to exclude Fresnel's theory. Michelson subsequently decided to repeat Fizeau's experiment, following advice from William Thomson and Lord Rayleigh. With his powerful interferometric technique and the help of Edward Morley, he accurately confirmed the Fresnel dragging coefficient in 1886.[8]

A few months later, the Dutch physicist Hendrik Lorentz published what Michelson called 'a very searching analysis' of the problem of ether motion. Lorentz first argued that Stokes's theory was kinematically impossible: the flow of an incompressible fluid around a moving solid sphere cannot be both irrotational and adhering to the sphere. Stokes's theory could only be saved by admitting a finite slip of

[7] Michelson 1881. Cf. Swenson 1972: 54–73; Howbold and Pyenson 1988.
[8] Michelson 1882: 522n (Potier's remark); Michelson and Morley 1886. Cf. Swenson 1972: 73, 74–87.

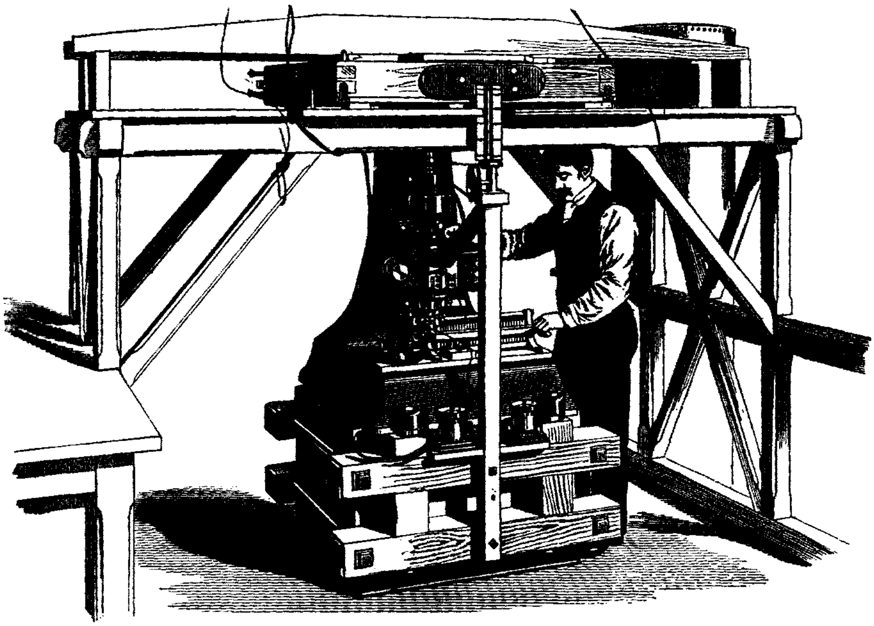

FIG. 8.3. Lodge's ether-whirling machine (Lodge 1893: 767). The operator faces the motor that drives the rotating disk pair. The disks are inclosed in the wooden drum above the head of the operator. The drum holds the interferometric device. It is supported by the long, outer girders, so that the vibrations of the central altar do not perturb the optical measurements.

the ether at the surface of the Earth and by adopting the Fresnel drag in transparent bodies, so that the slip did not affect the laws of refraction on Earth. This modified theory allowed for a null result of Michelson's Potsdam experiment, because there could be places on Earth in which the ether slip was two small to be detected. However, Lorentz far preferred Fresnel's theory for its higher simplicity and for other reasons to be discussed later. Like Potier, he noted the mistake in Michelson's theory of his experiment, and he called for a repetition.[9]

Michelson and Morley fulfilled this wish the following year, 1887. With an improved interferometer set on a marble slab floating in a mercury bath, they found that the fringe shift was at least 20 times smaller than that expected for the stationary ether. This perplexing result caught the attention of the two leading Maxwellians, FitzGerald and Lodge. The latter took up the problem of ether motion as his next experimental enquiry. He constructed an impressive 'whirling machine' in which two heavy coaxial steel disks rotated close to each other at high speed (Fig. 8.3). An

[9] Lorentz 1886; Michelson and Morley 1887: 335. Cf. Hirosige 1976: 26–28; Buchwald 1988: 59–61. Unknown to Lorentz, Stokes had long been aware of the kinematic difficulty. However, he believed that his gel-like ether could radiate away the rotational component of the motion impressed by the Earth: cf. Wilson 1972.

eventual motion of the ether between the disks was tested by Michelson-style interferometry. The negative result, published in 1893, confirmed Fresnel's stationary ether. This made the Michelson–Morley experiment of 1887 look more paradoxical than ever.[10]

In 1889 FitzGerald had already mentioned to Lodge a possible way out of the paradox. From Heaviside, he knew that the electric field of an electrified sphere moving at the velocity v was compressed toward the meridian plane (see p. 201), in the proportion $(1 - v^2/c^2)^{-1/2}$. Impressed by the coincidence with the light-time ratios of the Michelson experiment, he offered the following explanation of its null result:[11]

> The length of material bodies changes, according as they are moving through the ether or across it, by an amount depending on the square of the ratio of their velocities to that of light. We know that electric forces are affected by the motion of electrified bodies relative to the ether, and it seems not an improbable assumption that the molecular forces are affected by the motion and that the size of the bodies alters consequently.

The hypothesis initially attracted little attention: electrodynamicists were busy with Hertz's new waves. When in 1890–1892 Hertz and Heaviside proposed a complete, Maxwellian electrodynamics of moving bodies, they deplored that their fully dragged ether was incompatible with the optics of moving bodies. Yet they postponed further study of the relation between matter and ether. So did other German writers on Maxwell's theory. They usually concentrated on assimilating the core of Maxwell's system, and neglected the side-issues of electromagnetic optics. One exception was the champion of ionic physics, Hermann von Helmholtz.[12]

8.3 Helmholtz's ionic optics

8.3.1 Migrating centers of force

Since his crucial experiments of the mid-1870s, Helmholtz was convinced that Maxwell's theory was the only viable one. What he meant at that time by 'Maxwell's theory' was his own reinterpretation in terms of a highly polarizable vacuum. As we saw in the previous chapter, he judged the electric fluid or fluids to be convenient in the discussion of electrolysis. In his Faraday lecture of 1881 he introduced the provocative notion of 'atoms of electricity.' Yet he was not ontologically committed to the view of electricity as a substance: 'It is not at all necessary,' he declared to his British listeners, 'to accept any definite opinion about the ultimate nature of electricity.'[13]

[10] Michelson and Morley 1887; Lodge 1893. Cf. Hirosige 1976: 29–30; Swenson 1972: 87–97; Hunt 1986 (on Lodge).

[11] FitzGerald 1889b. Cf. Brush 1967; Hunt 1988, 1991a: 189–97.

[12] Hertz 1890b: 285. Heaviside 1891–2: 524. Heaviside to Hertz, 14 August 1889, in O'Hara and Pricha 1987; Hertz to Heaviside, 3 September 1889, *ibid.* Cf. Darrigol 1993b: 319, 324–5, 337.

[13] Helmholtz 1881a: 60, 59. See Chapter 7, pp. 273–4.

In the 1890s, Helmholtz adopted Hertz's equations and his idea that electric charge was only a 'name' for a conserved property of the field. This view was perfectly compatible with the concept of ions, which required only the permanence of the electric charge. But it was harder to reconcile with the transfer of atoms of electricity between ions and electrodes. On this point Helmholtz had the following to say:

The only requirement of the electrochemical theory that is not contained in Maxwell's equations is that these centers of electric force [in the ions] can migrate, with much expense of work, from ion to ion during chemical transformations, as if they were attached to a substantial carrier that would be attracted with different force by the valence sites of various kinds of ions.

In other words, Maxwell's equations remained sufficient if all transfers of electricity were reduced to microscopic charge convection. In the case of electrolysis, this view implied the existence of submolecular electrified particles carrying the atom of electricity. As we will see, the theories of Lorentz, Larmor, and Wiechert assumed so much.[14]

8.3.2 Dispersion

At the close of his life, Helmholtz devoted most of his efforts to subsuming physics under the principle of least action. In the case of electrodynamics, he managed to find an action for the Maxwell–Hertz equations, including the case of moving bodies (see Appendix 9). The mathematical difficulty of this work must have deterred most of his contemporaries. Yet in 1893 Helmholtz obtained a highly influential theory of optical dispersion by combining the variational method with the picture of elastically bound ions.[15]

Physicists had long believed that the dependence of refraction on wavelength was caused by the molecular structure of the medium. In Cauchy's optical theory, ether and matter molecules moved together, so that the refraction index was a decreasing function of the wavelength. In the 1870s, however, it became known that selectively absorbing bodies like iodine vapor or dyes exhibited 'anomalous dispersion,' for which red light is more refracted than violet light. Franz Neumann's student Wolfgang von Sellmeier soon showed by an energetic argument that if the molecules were themselves oscillators coupled to the ether motion, the dispersion was reversed when the optical frequency went through the molecular resonance frequency.[16]

In 1875 Helmholtz improved this theory by adding a damping of the molecular oscillations and re-expressing it in terms of two coupled dynamical equations: one for the ether, one for the molecular vibrations. His theory of 1893 can be seen as an

[14] Helmholtz 1893a: 506–7. On his adopting Hertz's formulation, see Chapter 6, p. 258.

[15] Helmholtz 1893a. On Helmholtz and least action, see Chapter 6, p. 258.

[16] Sellmeier 1872. Cf. Heilbron 1964: 21–5; Buchwald 1985a: 233–4; Carazza and Robotti 1996: 589–90.

electromagnetic translation of this system. The electric field acts on the elastically bound ions, while the motion of the ions counts as a field source. Dispersion corresponds to the dependence of this interplay on the field frequency. Technically, Helmholtz obtained the equations of motion by injecting terms depending on the *average* ionic polarization **P** into the Hamiltonian action. This gives

$$m\frac{\partial^2\mathbf{P}}{\partial t^2}+\alpha\frac{\partial\mathbf{P}}{\partial t}+\frac{\mathbf{P}}{\theta}=-\frac{\partial\mathbf{A}}{\partial t} \tag{8.1}$$

for the motion of the polarization in the electric field $-\partial\mathbf{A}/\partial t$; and

$$\nabla\times(\nabla\times\mathbf{A})=\mu_0\frac{\partial}{\partial t}\left(\mathbf{P}-\varepsilon_0\frac{\partial\mathbf{A}}{\partial t}\right) \tag{8.2}$$

for the relation between the vector potential **A** and the ionic convection current $\partial\mathbf{P}/\partial t$. The plane wave monochromatic solutions of this system lead to the dispersion formula

$$n^2-1=\frac{\varepsilon_0^{-1}}{\theta^{-1}+\alpha i\omega-m\omega^2}, \tag{8.3}$$

where ω is the pulsation of the waves and n the optical index of the medium. Helmholtz had no interest in the microscopic details of the interaction, or he judged them too complex to be worth further analysis. His step into microphysics was a cautious one, in conformity with his principle that 'one should not specialize theoretical assumptions further than is necessary for the subject matter.'[17]

Yet the idea that light and matter interacted through ions was novel, even heretical, for Maxwellian physicists. Whenever atomic structure played a role, Maxwell and his Cambridge followers tended to return to the elastic solid theory of light. In a question for the Mathematical Tripos of 1868, Maxwell anticipated Sellmeier's type of dispersion theory, based on an elastic solid ether. His disciple Glazebrook proceeded similarly, adding an illustration with a system of strings. Before Schuster and J. J. Thomson's works on gas discharge, the Maxwellians usually avoided molecular extensions of the macroscopic concepts of charge and currents. Helmholtz had no such inhibition.[18]

[17] Helmholtz 1875, 1893a, 1880: 910. Cf. Buchwald 1985b: 234–9; Carazza and Robotti 1996: 598–602. Owing to the confusion between microscopic and macroscopic fields, Helmholtz's expression of the action is not quite correct. Although it does not affect the form of the dispersion formula, the mistake must be corrected before the generalization to a moving body, as is done in Reiff 1893 (and in eqns. (8.1) and (8.2)).

[18] Maxwell [1868b], 1869, [1873b]; Glazebrook 1893. Cf. Whittaker 1951: 261–5; Heilbron 1964: 24–5. For a notable exception to the Maxwellian attitude, see Chapter 7, p. 265, note 2.

8.3.3 Moving bodies

Helmholtz devoted his last works to another shortcoming of Maxwell's electromagnetic theory of light: the optics of moving bodies. In a preliminary step, he discussed the motion of the ether around a moving solid. He examined 'whether pure ether could be devoid of any inertia and satisfy Maxwell's equations, and which motion it would perform in this case.' Like Hertz, Helmholtz was reluctant to ascribe momentum to the ether. He tried to balance the Hertz force $\partial(\mathbf{D} \times \mathbf{B})/\partial t$ on the ether by a pressure gradient. According to the Maxwell–Hertz equations, this could not be done without allowing a slip of the ether at the surface of the moving solid. Helmholtz died before completing this investigation. At the turn of the century, Wilhelm Wien and Gustav Mie tried to revive his project. They met little success: by that time the problem of ether motion had become largely obsolete.[19]

Had he been less scrupulous with the dynamics of ether motion, Helmholtz could have ignored the motion of the ether between ions and generalized the equations of his dispersion paper to the case of a moving transparent body. Richard Reiff did this for him in 1893. Reiff's intricate calculations amounted to including the effect of the global convection of the ions in the polarization current. The result was exactly Fresnel's partial drag. Helmholtz's contribution and Reiff's announcement gave a decisive impetus to ionic theory in Germany. They were not unprecedented, however. Unknown to Reiff, Hendrik Lorentz had derived Fresnel's coefficient a year before. Moreover, Lorentz had obtained an ionic theory of dispersion equivalent to Helmholtz's some 15 years earlier![20]

8.4 Lorentz's synthesis

Lorentz's isolated breakthrough had much to do with his Dutch background. As befitted his country's openness, he read indiscriminately from German, English, and French sources. His main inspirations, Helmholtz, Maxwell, and Fresnel, belonged to very distinct, sometimes conflicting, traditions. While in an average mind the eclecticism could have created confusion, Lorentz profited from it. He selected elements from each system and made his own syntheses. His philosophy rejected the *a priori* necessity of any theoretical principle, and denounced the dangers of exclusively pursuing one direction of research. Still, he had personal preferences. Like his countryman Johannes van der Waals, he asserted the superiority of 'the principle of atomism.' Much of his research concerned the kinetic molecular theory. His most influential works brought atomism to bear on optics and electrodynamics.[21]

[19] Helmholtz 1893b; Wien 1898: II; Wien 1901; Mie 1899, 1901a, 1901b.
[20] Reiff 1893.
[21] On Lorentz's biography, cf. McCormmach 1974; For his philosophy, see Lorentz 1878a.

8.4.1 A Helmholtzian thesis

Lorentz's first major work, his dissertation of 1875, was not about the molecular hypothesis but about a footnote found in Helmholtz's fundamental memoir of 1870 on the motion of electricity. After noting Maxwell's analogy between electric motions in a dielectric and vibrations in the optical ether, Helmholtz had written:[22]

> This analogy is also relevant in another, very important respect, which Maxwell has not touched. So far the mechanical state of the luminiferous ether in transparent media has been identified with that of solid elastic bodies. However, at the limit between two transparent media, this assumption gives boundary conditions which are not the ones needed to explain the reflection and refraction of light, so that there remains an unsolved contradiction in theoretical optics. In contrast, the theory of electric oscillations [in dielectrics with very large polarizability] gives the laws of wave propagation, reflection, and refraction that are known to apply to light [. . .].

Lorentz's dissertation was an explication of this concise footnote. He first confirmed the difficulties of the elastic solid theories of light, then applied 'Maxwell's theory'—he meant Helmholtz's reinterpretation of it—to the problem. It must be recalled that Maxwell had shrunk from this task, presumably because he did not trust his equations for quickly variable fields in matter. Lorentz performed all calculations with the full set of Helmholtz's equations, and took the limit of infinite dielectric polarizability only at the end. He obtained the boundary conditions between two media by requiring that no observable quantity should become infinite on the separating surface. The resulting formulas for the direction and intensity of reflected and refracted rays fitted Fresnel's in the isotropic case and Neumann's in the anisotropic case.[23]

Lorentz justified his preference for Helmholtz's system: 'I shall start with instantaneous action at a distance: thus we will be able to found the theory on the most direct interpretation of observed facts.' He meant that the most basic experiments of electricity, Coulomb's for instance, dealt with distance forces. And he found it 'difficult not to conceive a current as the motion of a certain substance which is contained by all good conductors of electricity.' However, in conformity with his pluralist philosophy he did not make action at a distance a rigid dogma. The true starting point of the theory, he stated, was the initial differential equations, not action at a distance.[24]

The use of Helmholtz's equations instead of Maxwell's implied a much higher analytical complexity than in the modern treatment of the same problem. Lorentz easily found his way in the mathematical thicket. He expounded his results with great clarity and elegance, and explored every possible contact with experiment.

[22] Lorentz 1875; Helmholtz 1870b: 558–9. Cf. Hirosige 1969: 160–7.
[23] Lorentz 1875. Cf. Hirosige 1969: 160–7. On the difficulties of the elastic solid theory and the Maxwellian solution, see Chapter 5, pp. 190–1. Lorentz was not aware of MacCullagh's work.
[24] Lorentz 1875: 224.

These qualities were promptly recognized by the Dutch authorities, who offered him the Leyden chair of theoretical physics in 1878.[25]

Through his dissertation work, Lorentz became convinced of the superiority of 'Maxwell's theory' to the 'old wave theory.' At the same time he emphasized the limitations of the new theory and sketched a molecular program for overcoming them:

Let us think about the dispersion phenomenon, the rotation of the plane of polarization, and the manner in which these phenomena are related to the molecular structure, then about the mechanical forces which perhaps play a role in certain light phenomena. Then let us think how external forces and the motion of the medium influence light; and let us think about the emission and absorption phenomena and the radiant heat [. . .]. Finally, the theory of light should reveal the link between [molecular] electric motions and the physical and chemical state of matter, a link that lies at the basis of spectral analysis, with its wealth of surprising results.

This program strikingly defines Lorentz's work of the next 30 years, except for the last topic, which other physicists took on.[26]

8.4.2 *Dispersion*

Most of Lorentz's dissertation treated dielectrics macroscopically, with a simple linear relation $\mathbf{P} = \kappa\mathbf{E}$ between polarization and electromotive force. However, it also contained a suggestion for a more detailed, microscopic picture:[27]

If one wishes to give an absolutely complete description of electric motion [in material dielectrics], one will have to take into account the ether first, then the imbedded molecules. The distance, the size, and the shape of the molecules then come into play, which very probably entails the possibility of explaining dispersion and the rotation of the polarization plane. Here I will leave these questions aside. I shall only remark that in gases, for which the influence of molecules is very small, this influence can very simply be taken into account in a first approximation. For this purpose, *we shall suppose that the ether has absolutely the same properties in gases as in a vacuum.*

The latter assumption was the simplest Lorentz could make. He only had to superpose the polarization of the molecules over that of a vacuum, according to

$$\mathbf{P} = \kappa_0\mathbf{E} + N\kappa_1\mathbf{E}, \tag{8.4}$$

where κ_0 is the polarizability of a vacuum, κ_1 that of a molecule, and N the number of molecules per unit volume. Consequently, the propagation velocity for transverse waves is proportional to $(\kappa_0 + N\kappa_1)^{-1/2}$. The corresponding optical index, $(1 + N\kappa_1/\kappa_0)^{1/2}$, varies with the density of the gas in conformity with Arago and Biot's empirical law.[28]

[25] Cf. McCormmach 1974. [26] Lorentz 1875: 382–383. Cf. Hirosige 1969: 173.
[27] Lorentz 1875: 279 (my emphasis). [28] Lorentz 1875: 280. Cf. Hirosige 1969: 171–2.

This simple reasoning owed much to the Helmholtzian context, in which polarization was reduced to microscopic shifts of electric charge. It contradicted the spirit of Maxwell's *Treatise*. For Maxwellian physicists well into the 1880s, polarization was a macroscopic property of the ether, not to be extended at the atomic scale. The role of the molecules was to modify the elasticity of the ether in the intervening space by an unknown non-electrical mechanism. In contrast, Lorentz assumed polarizable molecules imbedded in an incorruptible ether.[29]

In 1878, Lorentz published a theory of dispersion based on the same basic picture. He identified molecular polarization with the electric moment $q\mathbf{r}$ of 'particles provided with free electricity within molecules' (q denotes the charge, $\mathbf{r}$ its shift). These hypothetical particles of mass m moved under the combined effects of the elastic force $g\mathbf{r}$ and the local electric field. Consequently, their polarizability depended on the pulsation ω of the light according to $\kappa_1 = q^2/(g - m\omega^2)$. For the relation between this polarizability and the optical index n, Lorentz no longer relied on the macroscopic polarization $\mathbf{P}$. Instead he discussed the electromagnetic emission of individual molecules by means of the retarded potentials for Helmholtz's equations. The highly intricate calculations yielded our Lorenz–Lorentz law, according to which $(n^2 - 1)/(n^2 - 2)$ is proportional to $N\kappa_1$. The resulting dispersion formula exhibited the same resonance as Helmholtz's and Sellmeier's earlier formulas. It therefore explained anomalous dispersion.[30]

This work of Lorentz anticipated essential features of the future electron theory: the separation of ether from matter, the idea of an electromagnetic coupling between them, and the focus on microscopic processes. Although it was the first electromagnetic theory of dispersion, it remained mostly unnoticed until the mid-1890s. One plausible reason for this neglect is that Lorentz published in Dutch and lacked personal connections abroad. Another is that before the 1890s there were few potential sympathizers, Helmholtz excepted.

8.4.3 The Maxwell–Weber synthesis

In the lack of feedback, Lorentz discontinued his molecular electromagnetic program for more than 10 years. In the 1880s he mostly worked on thermodynamics and kinetic theory. His memoir of 1884 on the theory of magneto-optical effects adopted the Maxwellian strategy of macroscopically modifying the field equations. However, his 1886 discussion of the optics of moving bodies revealed the persistence of his faith in the molecular program. To the simplicity argument in favor of Fresnel's stationary ether, he added the following justication: 'It may be that what we call an atom can perfectly well occupy the same place as a portion of the ether, that for example an atom is nothing but a modification of the state of this medium; then one could understand that an atom can move without dragging the ether around it.'[31]

[29] Cf. Buchwald 1988: 62–3.

[30] Lorentz 1878b: 80. Cf. Hirosige 1969: 173–8; Buchwald 1985a: 198, 294–8; Carazza and Robotti 1996: 590–5. On Ludvig Lorenz's optics, cf. Kragh 1991.

[31] Lorentz 1884, 1886: 203.

Lorentz resumed his program in 1890, after Hertz's discovery of electromagnetic waves and the resulting enthusiasm for Maxwell's theory. In 1875 he had preferred Helmholtz's rendering of Maxwell's theory, for being 'founded on the most direct interpretation of observed facts.' After Hertz's new facts, he judged Helmholtz's theory to be 'artificial.' He promptly recommended to 'bring together old and new theory, at least with regard to form.' The old theory was Weber's. In place of Weber's particles of electricity, he assumed the existence of 'small charged particles' interacting through Maxwell's ether.[32]

Lorentz developed this synthesis in a major French memoir published in 1892. The first part was devoted to Maxwell's original macroscopic theory. Like Helmholtz and Poincaré, Lorentz praised Maxwell's use of the Lagrangian method, which provided a mechanical foundation without explicit mechanism. He generalized Maxwell's original considerations to include three-dimensional currents and moving bodies (see Appendix 9). However, Lorentz also had sympathy with Hertz's approach, which started directly with the field equations and avoided the unobservable potentials. In later writings he rarely felt the need to provide a dynamical foundation for his theory.[33]

Regarding charge and current, Lorentz assimilated more of Maxwell's concepts than Hertz had done. He called 'electricity' that of which the total current is a flow. In a dielectric he assumed the flow to be elastically resisted, so that the displacement **D** of electricity was proportional to the electromotive force **E**. He regarded the electric charge of an insulated body as the excess of electricity measured by the flux of **D** across an embracing surface. This made electrification depend on a previous conduction process that brought the necessary electricity. In sum, Lorentz took seriously Maxwell's metaphor of the incompressible fluid. But he ignored the more basic concept of polarization and the derived definitions of charge and current, which British Maxwellians judged most fundamental. Like Hertz, he must have had trouble reconciling Maxwell's and Helmholtz's polarization concepts. Fortunately, the notion of a charged body was all he needed for his own microscopic theory.[34]

In the second part of his memoir, Lorentz developed 'the theory of a system of charged particles which move across the ether without dragging this medium.' The idea was to apply the Maxwell equations to this microscopic system and to reduce all electromagnetic phenomena to interactions of the charged particles via the stationary ether. The only way matter could affect the ether was through the electromagnetic effects of its charged particles. By analogy with Weber's theory, conduction became a flow of the charged particles; charge, their accumulation; polarization in material dielectics, their elastically resisted shift; and magnetism, their microscopic cyclic motions. All of this was utterly un-Maxwellian: gone were the analogy between material dielectics and the ether, the concept of conduction as a decay of displacement, and the prejudice against applying electromagnetic concepts

[32] Lorentz 1875: 224; Lorentz 1891: 99. Cf. Hirosige 1969: 183–6.

[33] Lorentz 1892a: 173–88 (bodies at rest), 206–27 (moving bodies). Cf. Hirosige 1969: 193–6; Buchwald 1985a: 195–7; Darrigol 1994a: 275–8.

[34] Lorentz 1892a: 189–202. Cf. Darrigol 1994a: 278–9.

at the molecular scale. Lorentz knew no dogma, and saw very clearly the benefits of the new picture.[35]

For the microscopic fields **d** and **b**, Lorentz's equations read, in Hertz's units,

$$\nabla\cdot\mathbf{d}=\rho_{\mathrm{m}},\quad \nabla\cdot\mathbf{b}=0,$$
$$\nabla\times\mathbf{b}=\frac{1}{c}\left(\rho_{\mathrm{m}}\mathbf{v}+\frac{\partial\mathbf{d}}{\partial t}\right),\quad \nabla\times\mathbf{d}=-\frac{1}{c}\frac{\partial\mathbf{b}}{\partial t},\tag{8.5}$$

where $\rho_m\mathbf{v}$ represents the convection current of the charged particles. For the force acting at a point of a particle with the charge density ρ_m, Lorentz gave

$$\mathbf{f}=\rho_m\left(\mathbf{d}+\frac{1}{c}\mathbf{v}\times\mathbf{b}\right).\tag{8.6}$$

These equations were, up to irrelevant terms, those given by Hertz and Heaviside in the case of electric convection through an immovable ether. Lorentz further justified the convection term $\rho_m\mathbf{v}$ in the current by appealing to electrolysis and to Rowland's experiment. He derived the induction law and the force law by Lagrange's method, taking

$$T=\frac{1}{2}\int b^2\mathrm{d}\tau,\quad U=\frac{1}{2}\int d^2\mathrm{d}\tau\tag{8.7}$$

for the kinetic and potential energies of the ether (see Appendix 9).[36]

Starting with these fundamental equations, Lorentz easily retrieved standard electrostatic and electrodynamic effects by averaging over the charged particles. His main endeavor, however, was to extend the electromagnetic optics he had earlier inaugurated in the Helmholtzian framework. He first determined the effect of a single elastically bound particle on an incoming electromagnetic wave, and then summed over all the charged particles belonging to a given volume element of the dielectric. This cumbersome method brought insight into the microscopic processes, as well as some interesting by-products: the electromagnetic mass of a charged particle, and the radiative damping force $(e^2/4\pi c)\ddot{\mathbf{v}}$ (Lorentz overlooked a factor 2/3). Most spectacularly, Lorentz succeeded in deriving Fresnel's mysterious dragging coefficient.[37]

Some tricks used in the latter calculation had a bright future. From the wave equations with respect to the ether, Lorentz switched to those with respect to the moving

[35] Lorentz 1892a: 228–7. Cf. Buchwald 1988: 62–3.

[36] Lorentz 1892a: 230–8. Cf. Hirosige 1969: 200–1; Darrigol 1994a: 280–3. Lorentz used electromagnetic units until 1904.

[37] Lorentz 1892a: 250–67 (electrostatics and electrodynamics), 268–292 (dielectrics at rest), 292–320 (dielectrics in motion), 319 (Fresnel's coefficient), 281 (radiation damping). Cf. Hirosige 1969: 202–23; Buchwald 1988: 63–5.

transparent body. If the velocity **u** of this body with respect of the ether is parallel to the x-axis, this change amounts to the substitution

$$\frac{\partial^2}{\partial x^2}-\frac{1}{c^2}\frac{\partial^2}{\partial t^2}\rightarrow\frac{\partial^2}{\partial x^2}-\frac{1}{c^2}\left(\frac{\partial}{\partial t}-u\frac{\partial}{\partial x}\right)^2 \tag{8.8}$$

in the wave operator. Using a standard procedure for solving differential equations, Lorentz sought new variables x' and t' that would bring back the wave operator to a more familiar form. Thus he discovered that the transformation

$$x' = \gamma x, \quad t' = \gamma^{-1}t - \gamma ux/c^2, \tag{8.9}$$

where

$$\gamma = \frac{1}{\sqrt{1-u^2/c^2}}, \tag{8.10}$$

restored the original wave operator.[38]

Implicitly, Lorentz had discovered what Poincaré later called the Lorentz invariance of the wave operator: the substitution

$$x' = \gamma(x-ut), \quad t' = \gamma(t-ux/c^2) \tag{8.11}$$

obtained by combining the transformation (8.9) with the Galilean transformation $x \rightarrow x - ut$, leaves the operator $\partial^2/\partial x^2 - \partial^2/c^2\partial t^2$ unchanged. However, at that stage Lorentz did not reason in terms of invariant properties, either physical or mathematical. His only concern was to ease the calculation of the retarded potentials from a moving source.

8.4.4 Macroscopic field equations

Within a few months Lorentz introduced a first significant simplification of his optics of moving bodies. Through a proper averaging procedure, he derived equations for the macroscopic fields in a homogenous transparent body moving at the velocity **u** with respect to the ether. He used a system of axes bound to the transparent body, and defined the material polarization **P** as the average electric moment $\langle\rho_m\mathbf{r}\rangle$ over the charged particles belonging to the same volume element. Then the vector $\langle\mathbf{d}\rangle + \mathbf{P}$ is easily seen to play the same role as Maxwell's **D**. Averaging the effect of the Lorentz force on the charged particles, Lorentz further showed that the polarization **P** was approximately proportional to the vector $\mathbf{E} = \langle\mathbf{d} + (\mathbf{u}/c) \times \mathbf{h}\rangle$.[39]

To first order in u/c the macroscopic fields obey the system

[38] Lorentz 1892a: 297. [39] Lorentz 1892b, 1895: 35, 63–4, 75. Cf. Hirosige 1969: 206.

$$\nabla \cdot \mathbf{D} = 0, \quad \nabla \cdot \mathbf{H} = 0,$$

$$\nabla \times \mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{H}}{\partial t}, \quad \nabla \times \left(\mathbf{H} - \frac{1}{c}\mathbf{u} \times \mathbf{E}\right) = \frac{1}{c}\frac{\partial \mathbf{D}}{\partial t}, \tag{8.12}$$

$$\mathbf{D} = \varepsilon \mathbf{E} - \frac{1}{c}\mathbf{u} \times \mathbf{H}.$$

Accordingly, the velocity of a plane wave traveling in the direction parallel to the motion of the transparent body is $c\varepsilon^{-1/2} - u\varepsilon^{-1}$ (with respect to this body). The case $u = 0$ gives $n = \varepsilon^{1/2}$ for the refraction index. Consequently, the transparent body partially drags the waves with the coefficient $1 - 1/n^2$, in conformity with Fresnel's hypothesis (see also Appendix 11).[40]

In turn, Fresnel's hypothesis implies that optical experiments performed on Earth do not depend on the Earth's motion to first order in u/c. In 1895 Lorentz derived this result directly from his field equations. To this end he used the first-order version of the transformation (8.9), which amounts to the substitution of the 'local time' $t' = t - ux/c^2$ for the time t, together with the field transformations

$$\mathbf{D}' = \mathbf{D} + \frac{1}{c}\mathbf{u} \times \mathbf{H}, \quad \mathbf{H}' = \mathbf{H} - \frac{1}{c}\mathbf{u} \times \mathbf{E}. \tag{8.13}$$

To first order in u/c, the resulting field equations have the same form as the Maxwell equations in a system at rest with respect to the ether. From which result Lorentz deduced the 'theorem of corresponding states':

> If, for a given system of bodies at rest, a state of motion is known for which **D**, **E**, and **H** are certain functions of x, y, z, and t, then in the same system drifting with the velocity **u**, there exists a state of motion for which **D′**, **E**, and **H′** are the same functions of x, y, z, and t'.

Since, according to the relations (8.13), **D** and **H** vanish together if and only if **D′** and **H′** do so, the surface delimiting a light beam is the same in two corresponding states. Consequently, the laws of reflection and refraction, which control the shape of light beams, are the same in the drifting system as in a system at rest. A similar invariance holds for interference experiments, since the positions of dark fringes are the same for corresponding states.[41]

This kind of reasoning became typical of Lorentz's theory. In order to prove the absence of effects of the Earth's motion, Lorentz first wrote the field equations with respect to the Earth, then introduced a change of variable that retrieved the form of the equations with respect to the ether, next used the reverse transformation to generate solutions for the moving system from solutions for the system at rest, and finally

[40] Lorentz 1892b: 216; 1895: 75.

[41] Lorentz 1895: 84. Cf. Hirosige 1969: 207. Lorentz also used the theorem to derive the Fresnel coefficient by simply transforming the phase of a plane wave (Lorentz 1895: 95–7): cf. Darrigol 1994a: 289.

examined whether the two kinds of solution had the same observable consequences. In this procedure, only the original fields and coordinates had physical meaning. The transformed field and coordinates were mere computational aids.[42]

8.4.5 The Lorentz contraction

Lorentz also applied the correspondence technique to the electrostatics of drifting bodies. In this case the field equations in the moving system of axes are brought back to their form in a system at rest by the transformation $x' = \gamma x$, with the coefficient γ given in eqn. (8.10). Consequently, the qualitative laws of electrostatics, for example the impossibility of an internal charge of conductors, are unaffected by the global motion. The quantitative expression of forces and surface charges is only altered to second order in u/c. Lorentz concluded that the motion of the Earth had no observable effect on the equilibrium of a system of conductors.[43]

However, Lorentz imagined a connection between this problem and the Michelson–Morley experiment of 1887. Like FitzGerald, whose suggestion he was unaware of, he noted that a contraction by the amount γ^{-1} of the arm of the interferometer parallel to the Earth's motion would annihilate the theoretical fringe shift. Then he argued that his theory *implied* the contraction, if only the molecular forces responsible for the cohesion of solids behaved like electrostatic forces in regard to the Earth's motion through the ether. Suppose indeed that the molecular forces completely determine the dimensions of a solid for a given molecular arrangement. Then the fictitious solid corresponding to the actual moving solid through the dilation $x' = \gamma x$ will have the dimensions of the actual solid brought to rest. Consequently, the moving solid must contract in the proportion γ^{-1} in the direction of motion. No more than FitzGerald's was Lorentz's contraction a purely *ad hoc* hypothesis.[44]

8.4.6 German success

Lorentz obtained the preceding results regarding macroscopic field equations, corresponding states, and the contraction of lengths in 1892. Three years later, a systematic account of his theory appeared in German. In this *Versuch* (attempt), Lorentz introduced the charged particles—which he now called ions in reference to electrolytic and gaseous conduction—as the exclusive mediators between matter and the ether. He posited the microscopic field equations and the Lorentz force, and focused on the consequences for macroscopic bodies on Earth. He thus covered standard electrodynamic phenomena, optical dispersion, crystal optics, magnetism, magneto-optics, and the optics of moving bodies. During the next five years, his theory grew more and more popular, especially in Germany. There were internal reasons for this

[42] Cf. Nersessian 1986: 218–21; Janssen 1995: 157–79.

[43] Lorentz 1892c, 1895: 35–9, 119–24. Cf. Darrigol 1994a: 289–92. Heaviside had used the same transformation in one of his derivations of the field of a moving charge (Heaviside 1888–9: part 4).

[44] Lorentz 1892c: 221. Cf. Hirosige 1969: 204–5; Nersessian 1986, 1988; Janssen 1995: 180–98.

success: the extreme clarity and simplicity of the basic assumptions, and the extent of the empirical ground covered.[45]

However, two experimental discoveries hastened Lorentz's ascension. One was the effect of a strong magnetic field on the D-line of sodium observed in late 1896 by his student Pieter Zeeman with a Rowland grating. From the measured broadening, Lorentz inferred that the emitting 'ion' had a mass-to-charge ratio (m/q) about 2000 times smaller than that of the hydrogen ion. The reasoning assumed a simple harmonic vibration of the ion, modified by the magnetic force $q(\mathbf{v}/c) \times \mathbf{B}$. Ionic vibrations parallel to the magnetic force were unmodified, whereas the frequency of circular vibrations in the perpendicular plane was altered by $\pm qB/4\pi mc$. From this remark, Lorentz inferred that observation in the parallel and antiparallel directions should give a doublet of circularly polarized lines, while observation in the perpendicular direction should give a linearly polarized triplet. Zeeman's verification of this prediction lent much credibility to Lorentz's simple reasoning, and to the general idea that ions were responsible for the electromagnetic properties of matter.[46]

Also important were Wiechert's and Thomson's cathode ray studies. Since the discovery of X-rays, Lorentz suspected that the cathode rays were particles much smaller than atoms. Accordingly, he must have welcomed the experimental proofs of a large charge-to-mass ratio of these particles. In 1898, he was first to remove the ambiguity on the electric charge of the optical ions. From dispersion measurements he drew q^2/m; from the Zeeman effect, q/m. The resulting value of q was close to the electrolytic quantum. The following year Lorentz began to call the charged particles of his theory 'electrons,' in conformity with George Johnstone Stoney's name for the electrolytic quantum (and in harmony with Larmor's usage).[47]

The public consecration of Lorentz's theory occurred in 1898, at the Düsseldorf meeting of the *Naturforscherversammlung*. The problem of ether motion was the main theme, and Lorentz the guest of honor. In his introductory report, Wilhelm Wien listed the relevant experiments, including those of Michelson and Morley, and discussed the various theoretical conceptions, especially those of Helmholtz and Lorentz. He clearly favored Lorentz's stationary ether and the ionic theory. The attending elite, Voldemar Voigt, Max Planck, Paul Drude, and Gustav Mie, were champions of a more phenomenological physics, in the lineage of Franz Neumann. They nevertheless appreciated the strength of Lorentz's argumentation. Two of them, Drude and Planck, soon adopted the electron theory.[48]

Drude's conversion is especially striking. Whereas his *Physik des Aethers* and his earlier magneto-optics had a purely macroscopic outlook, his highly influential *Lehrbuch der Optik* of 1900 included ionic theories of magneto-optics, dispersion,

[45] Lorentz 1895.

[46] Zeeman 1896, 1897a (includes Lorentz's reasoning), 1897b. Cf. Heilbron 1964: 100–2; Arabatzis 1992; Kox 1997.

[47] Lorentz 1896: 165 (suggestion about cathode rays); Lorentz 1898a (*e* determined), 1899 ('electrons'). Cf. Carazza and Robotti 1996: 606–7.

[48] Lorentz 1898b; Wien 1898. Cf. Hirosige 1976: 33–6.

and the optics of moving bodies. In the same year he developed a full-fledged 'electron theory of metals' that yielded the Wiedemann–Franz relation between thermal and electrical conductivity, as well as thermoelectrical laws. The proposed mechanism of electric conduction resembled Weber's old idea of electric particles jumping from atom to atom. In fact, some central assumptions of Drude's theory derived from an earlier ionic theory of conduction by Weber's disciple Eduard Riecke. By 1900 the self-denying enthusiasm of early German Maxwellians had lived. Lorentz's synthesis of British and continental electrodynamics now looked most promising.[49]

8.5 Larmor's reform

In the period 1894–1897, the Irish-born physicist Joseph Larmor devised an electrodynamic theory which in its final form shared essential features of Lorentz's: the electrons, the stationary ether, and the derived optics of moving bodies. However, a look at his memoirs on this subject suffices to reveal important differences. Even in the eyes of British contemporaries, they were written in a difficult, at times obscure, style. Larmor meant to present an evolving theoretical complex, not a definitive synthesis. He multiplied historical and philosophical digressions. Essential elements of his theory, even in its final stage, were only expressed in words and pictures. He usually confined precise mathematization to the phenomenological level. Larmor's physics was freer and broader than conceptual rigor and practical efficiency commanded.[50]

8.5.1 Between Thomson and Maxwell

Larmor's interests and methods—not his obscure style—owed much to his Cambridge training and his Irish roots. He revered Hamilton's principle of least action 'as the fundamental formulation in dynamics and physics,' and praised it for conveying 'a clearer and more compact mode of representation [. . .] and an easier grasp of mathematical relations as a whole, than any other.' However, no more than William Thomson did he think that the principle freed one from the duty of illustrating physical theories. As he later explained:[51]

> The problem of the correlation of the physical forces [. . .] is divisible into two parts, (i) the determination of the analytical function which represents the distribution of energy [more exactly, the Lagrangian] in the primordial medium which is assumed to be the ultimate seat of all phenomena, and (ii) the discussion of what properties may be most conveniently and simply assigned to that medium, in order to describe the play of energy in it most vividly, in

[49] Drude 1900a, 1900b, 1900c; Riecke 1898. Cf. Seeliger 1922; Whittaker 1951: 418–20; Kaiser 1987; Eckert *et al.* 1992: 27–30. An earlier theory by Giese combined ionic convection and charge transfers between colliding ions and atoms (Giese 1889); in 1895 Lorentz proposed to retain only the first process (Lorentz 1895: 7). Suggestions for a convective theory of metallic conduction are also found in J. J. Thomson 1900.

[50] Cf. Buchwald 1985a: 141–2.

[51] Larmor 1884b: 55–6; 1893b: 389–90. Cf. Buchwald 1985a: 135–6; Hunt 1991a: 212.

terms of the stock of motions which we have derived from the observation of the interaction of natural forces that presents itself directly to our senses, and is formulated under the name of natural law.

Before 1893 Larmor's works on electromagnetic and optical theory were based on ether models in William Thomson's style. For example, in 1890 Larmor generalized Thomson's explanation of the Faraday effect in terms of flywheels placed in small spherical cavities within an elastic solid; the rotation of the flywheels stood for the magnetized matter, the solid for the ether. More important, Larmor venerated Thomson's vortex ring theory of matter. So did his fellow wrangler J. J. Thomson. There were, however, significant differences in the two men's theoretical views, as clearly appears in their attitudes toward Maxwell's theory.[52]

J. J. Thomson never tried to explicate the mechanism underlying Maxwell's electromagnetic field. Like Maxwell, he satisfied himself with a Lagrangian foundation of the field equations or of the motion of tubes of force. In contrast, in 1893 Larmor deplored that 'the nature of electric displacement, of electric and magnetic forces on matter, of what Maxwell calls the electrostatic and magnetic stress in the medium, of electrochemical phenomena, are all left obscure.' He required 'an ultimate dynamical theory,' as intelligible as the older elastic solid theory of the optical ether. Like William Thomson, he believed the electromagnetic theory of light to be a regression, and he would rather 'explain electric actions on the basis of a mechanical theory of radiation, instead of radiation on the basis of electric actions.'[53]

On magneto-optical phenomena, J. J. Thomson was again faithful to the Maxwellian spirit. Like FitzGerald, he relied on a modification of the macroscopic Lagrangian of the electromagnetic field. Instead Larmor used Thomson's gyrostatically loaded ether as we saw, and expressed the need of a sharp separation between ether and matter:[54]

The hypothesis of gyrostatic cells interspersed throughout the medium, though at first sight artificial, is a correct realization of the current views of the influence of ponderable matter on the undulations of the aether. Any exhaustive optical investigation must take cognizance of the mutual influence of the two interpenetrating media, the aether and the ordinary matter.

On electrolysis, J. J. Thomson originally avoided the anti-Maxwellian concept of charged molecule parts, and identified the electrolytic current with the decomposition–recomposition series of the intervening molecules. In contrast, Larmor accepted Helmholtz's ions and double layers, and applied them in 1885 to a new determination of the size of molecules. In 1891, he even flirted with Helmholtz's general theory of the motion of electricity (1870), which 'offered a more general view of the nature of dielectric polarization' and presented 'a concrete illustration of the general

[52] Larmor 1890, 1891a; Larmor 1893a: 390 (vortex rings). On Thomson's flywheel model for the Faraday effect, cf. Smith and Wise 1989: 439, 473–74.

[53] Larmor 1891a: 253; 1893a: 397. Cf. Darrigol 1994a: 302–3. On J. J. Thomson, see Chapter 7, pp. 295–300.

[54] J. J. Thomson 1888: ##41–3; 1893a: ##408–14; Larmor 1891a: 248.

statements of Maxwell with respect to electric displacement.' But he soon found (mistakenly) that Helmholtz's theory disagreed with the known value for the electrostatic pressure at the interface of two dielectrics. By 1893 he agreed with Maxwell's concept of current: 'The electric current,' he wrote, 'is in a dielectric the rate of change of the electric displacement, which is of an elastic character; in a conducting medium part of the current is due to the continual damping of electric displacement in frictional modes.'[55]

To summarize, in the early 1890s Larmor accepted some central notions of Maxwell's electrodynamics, but he was dissatisfied with several components of this theory: the dynamical foundation, the notion of displacement, the conflation of ether and matter, and the treatment of electrochemistry. For a better theory, he sought inspiration in William Thomson's vortex atom, in various mechanical models of the ether, and even in Helmholtz's ions.

8.5.2 MacCullagh resurrected

In 1893 Larmor discovered MacCullagh's rotational ether while reviewing the theories of magneto-optics. As we saw in Chapter 5, MacCullagh's medium, unlike the ordinary elastic solid, led to the boundary conditions required by Fresnel's laws of reflection and refraction. In his theory of the Kerr and Faraday effects, FitzGerald had given an electromagnetic interpretation of this medium: the equation of motion,

$$\mu \frac{\partial^2 \boldsymbol{\xi}}{\partial t^2} = -\nabla \times \frac{\nabla \times \boldsymbol{\xi}}{\varepsilon} \tag{8.14}$$

is identical to Maxwell's equation

$$\frac{\partial \mathbf{B}}{\partial t} = -\nabla \times \mathbf{E} \tag{8.15}$$

if $\mathbf{B}$ is the momentum $\mu\partial\boldsymbol{\xi}/\partial t$, and $\mathbf{E}$ the local torque causing the twist $\mathbf{D} = \varepsilon\mathbf{E} = \nabla \times \boldsymbol{\xi}$. Larmor soon became convinced that MacCullagh's ether was the ultimate medium he was looking for.[56]

MacCullagh's ether was a dynamical medium in the two manners that Larmor demanded: its equations of motion were based on the principle of least action, and its elasticity could be illustrated mechanically. To prove the latter point, Larmor resorted to one of William Thomson's numerous ether models, proposed in 1890. By a clever assemblage of gyrostats, Thomson had obtained resistance of the elements of the medium to rotation without resistance to translation, as required for the rotational ether. This illustration disposed of Stokes's old objection to MacCullagh's

[55] Larmor 1885; 1891b: 233; 1892: 284; 1893a: 339. On Larmor's error, cf. Buchwald 1985a: 139–40, 320.

[56] Larmor 1893a: 340–43. Cf. Hunt 1991a: 212–3.

theory: a torque could act on an element of the medium without reaction from the adjacent parts of the medium, Larmor explained, because the gyrostatic effect established 'a kind of connection with absolute immovable space.' In addition, the model pointed to 'the ultimate conceivable simplification' of physics in which every energy would be of kinetic origin.[57]

Yet Larmor did not believe that Thomson's model could represent the actual structure of the ether. He knew that under a constant strain the rotational elasticity of the system of gyrostats slowly dimished. Hence Thomson's assemblage could only be an illustration of the basic dynamical properties of the ether for a moderate length of time. Larmor did not ask more. He shared the Maxwellian creed that the ultimate medium could not resemble any of the bodies accessible to our immediate experience:[58]

> The main problem of transcendental physics is to assign the nature of the ultimate medium or scheme of relations which combines physical phenomena into a unity, in whose relations dynamical notions have their scope: and it is only the prejudice of education that would keep, in this wide field, too close to the ideal of mechanical transmission in a homogeneous elastic solid.

8.5.3 Electromagnetic vortices

Larmor appreciated not only the dynamical virtues of MacCullagh's ether, but also the opportunity it offered for exploring the relation between matter and ether. This medium had the rigidity required for the propagation of optical waves, and yet was fluid enough to permit the free motion of matter. Most decisively, Larmor managed to build permanent vortices into the medium. Within a few months he could sketch an ambitious 'dynamical theory of the electromagnetic and luminiferous medium' that combined MacCullagh's optics, Maxwell's electromagnetism, and William Thomson's vortex atoms.[59]

At first glance, permanent vortices seem impossible in the rotational ether: the circulation of the velocity $\mathbf{B}/\mu$ of the medium around a vortex does not vanish; this implies, in electric terms, a displacement current in every section of the vortex, so that the state of the medium cannot be stationary. Larmor avoided this difficulty by assuming a 'fault' of the medium along the axis of the vortex. The core of the vortex was either hollow or deprived of elasticity. This property being allowed, Larmor spread vortex atoms in the rotational ether, and proceeded to discuss the effect of matter on its optical properties.[60]

[57] Thomson 1890; Larmor 1893a: 354; 1893b: 408, 390; 1897c: 15–7 (Thomson improved). Larmor was of course aware of FitzGerald's vortex sponge, but he questioned its stability (Larmor 1893a: 354). On Thomson's model, cf. Smith and Wise 1989: 486–7.

[58] Larmor 1897c: 17 (gyrostatic elasticity not strictly permanent); 1897b: 629–30 (quote). On the Maxwellian medium, cf. Stein 1981.

[59] Larmor 1893b, 1894. Cf. Buchwald 1985a: 141–53; Hunt 1991a: 212–7; Darrigol 1994a: 305–310.

[60] Larmor 1893b: 400, 406.

Larmor's explanation of inductive capacity and optical refraction in material bodies was typical of the Maxwellian use of molecular considerations: 'The presence of vortex atoms, forming faults, so to speak in the aether will clearly diminish its effective rotational elasticity; thus it is to be expected that the specific inductive capacity of material dielectrics should be greater than the inductive capacity of a vacuum.' No detailed picture of the effect of individual atoms was given, and no electric property was ascribed to the individual atoms. The reference to the microcosm only served to justify a modification of macroscopic parameters or equations. For example, Larmor added higher derivatives of $\boldsymbol{\xi}$ to the equation of motion of the medium in order to represent dispersion. For anomalous dispersion, he recognized the superiority of theories of Sellmeir's type but made no attempt to emulate them.[61]

Larmor also discussed the optics of moving bodies. He found two reasons to support Fresnel's stationary ether. First, the rotational elasticity of MacCullagh's medium did not impede the translation of vortex rings. Second, an ether wind with the velocity $\partial\boldsymbol{\xi}/\partial t$ would have implied a magnetic field $\mathbf{B} = \mu\partial\boldsymbol{\xi}/\partial t$. Since such a field had never been detected, the ether had to be stationary, unless the inertia μ of the ether was small. A small inertia, however, would have implied an unacceptably large deviation of light rays in a magnetic field. Lodge soon verified with the interferometer of his whirling experiment that even a strong magnetic field had no measurable effect on the velocity of light. Hence Larmor's ether had to be at least as dense as ordinary matter. Larmor found the hypothesis 'somewhat startling,' but admissible for an 'intangible medium.'[62]

For the Fresnel drag, Larmor admitted the incapacity of his reasonings: 'The nature of the further slight alteration [. . .] of elasticity produced by a motion of the matter as a whole, there appears to be no means of exactly determining.' As a substitute, he offered a dubious thermodynamic reasoning: a drag different from Fresnel's, or else a positive result of the Michelson–Morley experiment, would allow one to use the ether wind to power machines.[63]

Thanks to FitzGerald's electromagnetic interpretation of MacCullagh's medium, Larmor could also treat electric and magnetic phenomena. There was, however, a major obstacle: if the shift $\boldsymbol{\xi}$ of the rotational ether is uniquely defined at every point of a dielectric, then electric charge is impossible, because by Stokes's theorem the flux of the electric displacement $\nabla \times \boldsymbol{\xi}$ across a dielectric surface surrounding a conductor is necessarily zero. Larmor offered a Maxwellian solution to this difficulty: 'The legitimate inference is that the electric displacement in the medium which corresponds to an actual charge cannot be set without some kind of discontinuity or slip in the linear displacement of the medium; nor can it lose a charge without a similar rupture.' In other words, a body could not be charged without a con-

[61] Larmor 1893b: 406–7; 1894: 438–43.

[62] Larmor 1893b: 391 and 1894: 476–8 (stationary ether); Larmor 1893b: 413 and 1894: 483–4 (Lodge's test); Larmor 1894: 483 ('startling' and 'intangible'). Cf. Hunt 1986: 124–32 and 1991a: 214–5.

[63] Larmor 1894: 476 (quote), 479–80, 482. On the thermodynamic reasoning, cf. Warwick 1991: 37–40.

ducting connection to another charged body; and the connection implied a breakdown of the elastic property of the medium, in accordance with Maxwell's concept of conduction.[64]

Vortices were the natural counterpart of electric currents in the rotational ether. A macroscopic vortex implied closed lines of flow, to be identified with the closed lines of magnetic force around a current. As long as this circular flow was resisted by the rotational elasticity of the medium, the vortex corresponded to a displacement current. When the core of the vortex lost its elasticity, a conduction current was held to occur. To complete this picture, Larmor knew from Helmholtz that the kinetic energy of a system of linear vortices had the same form as the energy of a system of currents. He deduced Neumann's electrodynamic laws from this analogy.[65]

Another energetic argument gave the Coulomb forces between two electrified bodies: these forces were necessary to compensate the variation of the elastic energy of the medium when the distance between the two bodies varied. The intuitive reason for these forces was harder to find. Larmor's theory had no room for the Faraday–Maxwell stresses, because in the rotational medium stresses are a linear function of the electric displacement. Quite imaginatively, Larmor proposed that the motion of an electrified body implied an encroachment of the surrounding medium causing wavelets of elastic rearrangement. He interpreted the electrostatic force as the dynamical reaction of the body to this emission of wavelets. This was neither distance action nor contact action; it was dynamically propagated action.[66]

8.5.4 From vortices to electrons

Very soon Larmor encountered grave difficulties in his scheme. Partly as a result of FitzGerald's private criticism, he became aware that his vortices in the rotational ether were quite problematic. First, there were the usual difficulties of the vortex ring atom, for example the arbitrariness in the size of the vortices. Knowing this from the beginning, Larmor imagined compensatory mechanisms. A more serious problem was the representation of ions. Larmor admitted ions for electrolysis, for the emission of light, and for chemical forces. The forces between two atoms could not be reduced to the hydrodynamic interaction between the corresponding vortex rings, because this would have made all bodies ferromagnetic (a vortex ring being also an electric current loop in the rotational ether). Larmor therefore admitted an electric charge of the vortex atoms. Yet by December 1893 he feared that such a mixed state of motion and strain would not be stable.[67]

[64] Larmor 1893b: 398–399. Cf. Buchwald 1985a: 143–50; Darrigol 1994a: 308. In mathematical terms, the shift ξ around a charged body is necessarily multivalued.

[65] Larmor 1893b: 399 and 1894: 454–5 (nature of current); Larmor 1893b: 400 and 1894: 457–65 (electrodynamics).

[66] Larmor 1894: 451–3. Other Maxwellians could not make sense of Larmor's dissolution picture: cf. Hunt 1991a: 217–218.

[67] Larmor 1893b: 406–7 (size problem), 401 (chemical forces); [406] (instability). I use square brackets to indicate additions Larmor made to the proofs. FitzGerald's role is documented in Buchwald 1885a: 161–7, and Hunt 1991a: 217–20.

The vortex representation of electric currents was also problematic: according to Helmholtz's famous hydrodynamic theorem, the strength of the vortices had to be invariable. This befitted Amperean currents in magnets, but excluded electromagnetic induction in macroscopic conductors as well as the Weberian explanation of diamagnetism. For the latter phenomenon, Larmor referred to an alternative model by William Thomson. For induction, he proposed the following escape: 'Ordinary currents must [. . .] be held to flow in incomplete circuits, and to be completed either by conduction across an electrolyte or by electric displacement or discharge across the interval between molecules.' In this case the medium's stress acted in the breaches of the circuit and hopefully yielded the observed electromotive force.[68]

Lord Kelvin (as William Thomson had become) struck the most damaging blow. He reminded Larmor that the analogy between currents and vortices was imperfect, that it gave the wrong sign for electrodynamic forces. Larmor's several attempts to overcome this difficulty turned his theory into a baroque monster. Among other intricacies, he imagined fluctuations in the orientation of Amperean currents that re-established the right sign for the forces between magnets and explained the electromagnetic induction in a conductor moving near a magnet at rest. As Buchwald puts it, he 'replaced contradictions with mysteries.'[69]

Between June and August 1894 Larmor gave up the vortices and introduced instead a new subatomic particle. In June, he judged that his introduction of breaches in electric circuits was the 'most unnatural feature of the present scheme,' and proposed to regard conduction currents, whenever possible, as ionic convection. This was a growing trend even in England, as we saw while discussing Schuster's and J. J. Thomson's gas discharge studies. The only obstacle was the passage of electricity between electrolytes and electrodes. Larmor had earlier proposed special paths of disruptive discharge between two atoms. He now offered 'a more fundamental view': the charge or discharge of an ion was to be understood as the transfer of a 'monad.' Like Helmholtz's 'migrating centers of force' of 1892, the monads were point singularities in the ether carrying the electrolytic charge quantum, positive or negative. Like William Thomson's and J. J. Thomson's vortex rings, they were meant to be the universal constituents of matter.[70]

Larmor also replaced the Amperean vortices with rotating chains of monads. He thus re-established the correct sign of magnetic interactions, and explained electromagnetic induction in the field of a stationary magnet without the baroque fluctuation mechanism. Also, he no longer needed a separate explanation for diamagnetism. By August 1894, he had completely eliminated vortex atoms and vortex currents from his theory. He renamed the monads 'electrons' after Stoney's name for the electrolytic quantum. Again, FitzGerald played a crucial role in forcing Larmor toward this solution.[71]

[68] Larmor 1894: 468, 477–8; Larmor 1893b: [400–1] (quote).
[69] Larmor 1894: [504], [506–8]. Cf. Buchwald 1985a: 155–9; Hunt 1991a: 217–9.
[70] Larmor 1894: [475] (unnatural feature); 1893b: 406 (electrodes); 1894: [475] (monads).
[71] Larmor 1894: [468n], [515]. In 1881 Stoney had introduced and evaluated 'the quantity of electricity needed for the rupture of one chemical bond,' and had combined it with the velocity of light and the gravitation constant to define natural units of space, time, and mass (Stoney 1881). In 1891 he gave

The electrons were centers of radial twist in the medium. They could be mentally constructed by the following ether surgery: remove a filament of the medium between two points M and M′; grab the walls of the resulting cylindrical cavity and rotate them to a given angle; refill the cavity with the stuff of the medium; and release the whole. The end result is a self-locked strain representing two electrons of opposite signs at M and M′. The inertia of such electrons is entirely electromagnetic, more exactly: ethereal. Larmor's matter was nothing but swarms of singularities in the ether.[72]

At that point Larmor had the basic ingredients for elaborating a theory similar to Lorentz's: a stationary ether, and the reduction of all effects of matter to the action of electrified particles. However, he was still tributary of the Maxwellian emphasis on macroscopic Lagrangians and equations. For the electrodynamics of macroscopic currents, he summed the contributions of all relevant electrons to the kinetic energy of the medium, and retrieved Maxwell's macroscopic energy formula. Then he wrote the corresponding Lagrange equations, which led to the forces known to Maxwell (with a difference to be later discussed).[73]

For optics, Larmor mostly transposed the reasonings of his earlier theory, the electrons now playing the role of the vortex rings in modifying the elastic properties of the medium. However, he was now able to derive Fresnel's dragging coefficient. His method was to distinguish, somewhat like Reiff, two different contributions to the macroscopic displacement or rotational strain: one 'belong[ing] to the waves and provid[ing] the stress by which they are propagated,' the other belonging 'to the orientation of the molecules.' In the wave equation for the displacement, he isolated the time derivative of the second contribution, and replaced it with a convective derivative. This immediately gave the Fresnel coefficient. The reasoning was much simpler than Lorentz's of 1892—of which Larmor was not aware—but lacked rigorous foundation.[74]

8.5.5 *Learning from Lorentz*

In early 1895, Larmor read Lorentz's *Versuch*. In a subsequent 'Theory of electrons,' he turned his rough and partially misconceived scheme into a precise deductive theory that much resembled Lorentz's. Despite his claim to the contrary, this dramatic improvement owed much to Lorentz's insights. For example, he gave up the

the name 'electron' to this quantity, and explained the D doublet of sodium in terms of a precessing elliptic motion of an electron within the atom. Here he meant a mobile center of electrification in the atom's matter, not a separable subatomic particle (Stoney 1891. Cf. Heilbron 1964: 93–94; O'Hara 1975). At FitzGerald's request, Stoney sent his 1891 paper to Larmor in July 1894 (cf. Robotti and Pastorino 1998: 170).

[72] Larmor 1894: [516], [520].

[73] Larmor 1894: [521–3], [528–9]. Cf. Buchwald 1985a: 168–71; Darrigol 1994a: 313–5.

[74] Larmor 1894: [530–3]. Cf. Darrigol 1994a: 315–6. As Larmor knew from Glazebrook 1885: 216, Boussinesq's derivation of Fresnel's coefficient used the substitution of a convective derivative for the ordinary time derivative, but in a different manner, dictated by Fresnel's idea of the convected excess ether.

idea of a macroscopic Lagrangian and adopted Lorentz's procedure of averaging microscopic dynamic equations. A related change was the inclusion of the convection current of the electrons in the microscopic form of the Ampère law. Previously, Larmor had only written the field equations for the ether between the electrons, because his electrons were faults in the ether. He now used the convection current as a 'kinematic fiction' easing the passage to the macroscopic current. This view differed from Lorentz's, which put the convection current on the same footing as the displacement current. But it served the same formal purpose: to complete the first circuital equation in a consistent manner.[75]

With this subterfuge, Larmor could easily derive the macroscopic form of the first circuital equation in matter moving at the global velocity **u**:

$$\nabla \times \mathbf{H} = \frac{1}{c^2}\frac{\partial \mathbf{E}}{\partial t} + (\rho - \nabla \cdot \mathbf{P})\mathbf{u} + \mathbf{j} + \frac{\mathrm{D}\mathbf{P}}{\mathrm{D}t} \tag{8.16}$$

in rationalized electromagnetic units. The first term represents the average displacement current; the second the convection of the average charge $\rho - \nabla \cdot \mathbf{P}$; the third the conduction current (corresponding to the relative motion of the electrons with respect to matter); and the last the convective derivative of the polarization **P**. Note that the average charge includes two terms: ρ corresponds to an excess of free or little-bound electrons as may occur in conductors, $-\nabla \cdot \mathbf{P}$ to an heterogeneity in the shift of bound electrons. In the absence of magnetic polarization, the other macroscopic circuital equation simply reads[76]

$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t}. \tag{8.17}$$

Larmor completed the macroscopic system of equations with a relation between the polarization **P** and the average electric force **E**. He first used a macroscopic guess similar to Helmholtz's and Reiff's. However, for the Fresnel drag he also examined the microscopic polarization mechanism in Lorentz's manner, and obtained

$$\mathbf{P} = (\varepsilon - 1)(\mathbf{E} + \mathbf{u} \times \mathbf{B}). \tag{8.18}$$

by averaging the Lorentz force over electrons moving nearly at the global velocity **u**. The wave solutions of eqns. (8.16), (8.17), and (8.18) yield Fresnel's dragging coefficient. The only difference from Lorentz's second derivation of this coefficient is that Larmor wrote the macroscopic equations with respect to the ether, while Lorentz wrote them with respect to the moving body.[77]

[75] Larmor 1895b: 556, 577–8.

[76] Larmor 1895b: 573. For details, cf. Darrigol 1894a: 317–9. Larmor defined **E** as the average of the microscopic electric field, whereas Lorentz (see p. 328 above) took the average of the force acting on a unit charge bound to the moving body ($\mathbf{E} + \mathbf{u} \times \mathbf{B}$ in Larmor's notation).

[77] Larmor 1895b: 557–8, 575–7.

Larmor also dealt with the general insensitivity of terrestrial optics to the Earth's motion, including the Michelson–Morley experiment of 1887. From Lorentz he borrowed the local time, the contraction of lengths, and the technique of corresponding states. The only difference concerned the justification of the Lorentz contraction. Whereas Lorentz needed to assume that molecular forces transformed like electromagnetic ones, Larmor could point to his reduction of matter to singularities in the ether. He soon reversed the argument, making the Michelson–Morley experiment of 1887 a proof of his ultimate medium.[78]

A related difference appeared in Lorentz's and Larmor's later extensions of the technique of corresponding states to the motion of electrons. In conformity with the dualistic nature of his scheme, Lorentz's reasonings included transformations of the velocity and acceleration of the electrons. Instead Larmor assumed the 'completeness of the aethereal scheme,' meaning that 'the electron *taken by itself* must be in any conceivable theory a simple singularity of the aether whose movements [. . .] are traceable through the differential equations of the surrounding free aether alone.' He thus reached simpler invariance proofs, based on the tranformations of the fields only.[79]

No more than Lorentz did Larmor expect a general invariance of electromagnetic phenomena. He predicted a first-order effect of the Earth's motion on electromagnetic forces, and a second-order effect on electric conductivity. That no experimental evidence yet existed for such effects did not worry him. His and Lorentz's conviction of the physical existence of the ether was so strong that they naturally expected effects of the ether wind.[80]

8.5.6 Transcendental physics

Facing or anticipating criticism from his British colleagues, Larmor struggled to make his theory appear necessary. As we saw, his true reason for switching to a 'theory of electrons' was the difficulty of combining MacCullagh's ether with Maxwell's concept of conduction and William Thomson's theory of matter. In his own historical reconstruction, Larmor instead evoked an intrinsic defect of Maxwell's theory. He believed to have proved that Maxwell's dynamical reasonings, if properly conducted, should have led to an additional force $(\mathbf{j}\cdot\nabla)\mathbf{A}$ besides the empirically known force $\mathbf{j}\times\mathbf{B}$ acting on the current $\mathbf{j}$ (see Appendix 9). Soon after, he learned from Lodge and FitzGerald that this new force did not exist. But he maintained that it was a consequence of Maxwell's theory. In subsequent writings, he declared that this failure of Maxwell's macroscopic field dynamics compelled one to adopt the electron theory. The electric current had to be analyzed into the motion of electrons, and dynamical reasoning confined to the electronic scale.[81]

[78] Larmor 1895b: 566. Cf. Warwick 1991: 63.

[79] Larmor 1900a: 165, 171–2; see also Larmor 1897c: 41. Cf. Darrigol 1994a: 327–31.

[80] Cf. Darrigol 1994a: 331–2.

[81] Larmor 1894: [529]; 1897b: 627. On Lodge's and FitzGerald's input, cf. Buchwald 1985a: 170–1; Hunt 1991a: 223–6.

Maxwellian physicists suspected a mistake in Larmor's destructive criticism. They knew that Heaviside had confirmed Maxwell's electrodynamic force formula without recourse to Lagrangian dynamics. However, they were not able to locate the fault in Larmor's reasonings. While varying the kinetic energy of the field during a deformation of the condutors, Larmor miscalculated the effect of the deformation on the current density $\mathbf{j}$ (see Appendix 9). He assumed that this effect was such that the product $\mathbf{j}d\tau$ was invariant, whereas it is $\mathbf{j}\cdot d\mathbf{S}$ that is invariant, owing to the physical meaning of the current as a flux. Larmor's dismissal of Maxwell was all too quick.[82]

A similar comment can be made about another of Larmor's disproofs of Maxwell's theory. He argued that the Rowland experiment with a rotating, uniformly electrified disk eluded Maxwell's theory, whereas the new electron theory offered a satisfactory explanation. Again, this objection came *a posteriori*: in 1894 Larmor believed that Rowland's effect had only been established for disks with radial cuts. Moreover, Larmor's judgment that no current could exist in the Maxwellian analysis of the rotating, uniformly electrified disk was incorrect. In his Maxwellian electrodynamics of moving bodies, Hertz had shown that the motion of the ether near the surface of the disk implied a displacement current of the same intensity as the convection currents of the fluid theory.[83]

Besides his pseudo-refutations of Maxwell's theory, Larmor argued for the philosophical necessity of the basic concepts of his theory. 'The idea of an aethereal medium,' he declared, 'supplies so overwhelmingly natural and powerful an analogy as for purposes of practical reason to demonstrate the existence of the aether.' Atomism was equally necessary: 'As soon as we [. . .] cease to regard space as mere empty geometrical continuity, the atomic constitution of matter [. . .] is raised to a natural and necessary consequence of the new standpoint.' He regarded himself as the founder of a new transcendental aesthetics: 'It might be held that this conception of discrete atoms and continuous aether really stands, like those of space and time, in intimate relation with our modes of mental apprehension, into which any consistent pictures of the external world must of necessity be fitted.'[84]

8.5.7 Maxwellian antipathies

Larmor's rhetoric of unavoidability failed to impress his competitors. None of Maxwell's disciples accepted Larmor's theory as a whole, although they were ready to introduce atoms and ions in electrodynamics. Heaviside was most antagonistic. He accused Larmor of hiding the failure of the rotational ether—which he had predicted in 1891—behind pedantic Lagrangians and convoluted verbiage: 'What I *should* like to see,' he told FitzGerald in 1895, 'would be a little more candour about the illsuccess of the rotational ether to satisfy electrical requirements.' He also

[82] On the Maxwellians' skepticism, cf. Hunt 1991a: 226–7. On Larmor's error, cf. Darrigol 1993b: 346–8.

[83] Larmor 1895b: 583; 1894: 466–7; Hertz 1890b: 274–5. Cf. Darrigol 1994a: 323; 1993b: 321.

[84] Larmor 1897c: 13–4; 1900a: 76; 1904: 278; 1900b: 202. Cf. Darrigol 1994a: 326–7.

criticized the complete elimination of true conduction and the concept of electrons as singularities in the ether. In his view, electrons could only be bits of electrified matter, in conformity with Maxwell's concept of electric charge. They were no more than a 'special hypothesis,' to which he preferred his familiar register of electrotechnical analogies.[85]

J. J. Thomson's certainly saw more than a special hypothesis in his 'corpuscle.' Yet he found it 'exceedingly difficult to arrive at any definite conclusions as to the merit of [Larmor's theory].' He shared Heaviside's criticism of the electron *qua* singularity, and accordingly avoided the term 'electron.' His and Poynting's picture of the dissolution of unit tubes of forces in conductors preserved a Maxwellian intuition of the electric current, unlike Larmor and Lorentz's electronic flow. Friendlier to Larmor were FitzGerald and Lodge: they both accepted the need of 'a theory of electromagnetic actions depending entirely on the action of electrons' and they approved the project of reducing electrons to singularities in a dynamical ether. However, Larmor's gyrostatic illustration of the rotational ether did not meet their idea of a dynamical explanation. With the vortex sponge they still hoped to reduce ether and matter to a perfect fluid 'squirming internally with the velocity of light.'[86]

Larmor's true superiority, in the British context, was his treatment of the optics of moving bodies. He won the Adams Prize of 1898 with an essay on this topic, which grew into his influential *Aether and matter*, published in 1900. The subsequent rise of his approach depended on two factors: his activity in the Cambridge Mathematical Tripos, and the disengagement of Maxwell's heirs. FitzGerald died prematurely in 1901. Heaviside drifted out of the scientific community. Lodge devoted himself to his principalship at the new University of Birmingham. The only dangerous competitor, J. J. Thomson, focused on the new radiations and atomic structure. At any rate, the Cambridge institutions prevented destructive interference: Larmor controlled the Mathematical Tripos, while Thomson controlled the Natural Science Tripos. Larmor had a free hand to train a few British theorists in the subtleties of the ultimate medium.[87]

8.6 Wiechert's world-ether

Lorentz and Larmor were not the only physicists who thought of reducing electrodynamics to the motion of charged particles through a stationary ether. The idea independently occurred in the mind of the Königsberg physicist Emil Wiechert, better known for his later direction of Göttingen's Geological Institute. Despite his training in the Neumann school of mathematical phenomenology, Wiechert was an

[85] Hertz to FitzGerald, 22 July 1897, quoted in Hunt 1991a: 233; Heaviside 1893–1912, Vol. 3: 58. Cf. Hunt 1991a: 229–38.

[86] J. J. Thomson, review of Larmor 1897c, quoted in Buchwald 1985a: 172; FitzGerald 1896: 353; Lodge 1909: 97. Cf. Buchwald 1885a: 172–3; Hunt 1991a: 128–9.

[87] Larmor 1900a. On the Maxwellians' decline, cf. Hunt 1991a: 241–3. On Larmor, J. J. Thomson, and the Cambridge Tripos, cf. Warwick 1992, 1993a.

enthusiastic supporter of atomic theories. Like Helmholtz, but unlike other German converts to Maxwell's theory, he insisted on the failure of the macroscopic field approach to explain electrolysis, optical dispersion and absorption, and ferromagnetism. He recalled the merits of Weber's molecular theory, and offered a psychological explanation for his colleagues' Maxwellian excesses: 'Following human nature, we went too far in the introduction of the new ideas. In the thrill of new progresses, we applied the new scheme to everything, without exception, and we left aside whatever resisted the integration.'[88]

8.6.1 Electric atoms

Nonetheless, Wiechert had his own fascination for one aspect of Maxwell's theory: the unification of optics and electromagnetism. In this achievement he saw a sign that the ether was the 'actual carrier of the sensory world.' Lecturing on 'the meaning of the world-ether' in March 1894, he suggested to reduce matter to a myriad of 'centers of excitation' in the ether, endowed with a mass of purely electromagnetic origin. This view explained the aberration of stars, since mere centers of excitation, like William Thomson's vortex rings, traveled freely through the ether: 'We are reminded of the observation that the waves on the ocean travel with great velocity and nevertheless leave their carrier, water, behind them.' Wiechert further speculated that there was only one kind of center of excitation: Helmholtz's 'atom of electricity,' materialized, and made the 'building block' (*Baustein*) of all matter.[89]

Wiechert had the basic ingredients to build a theory comparable to Lorentz's—of which he was not aware. However, he judged that his views were too hypothetical to be fully published. Before 1898 he only gave outlines, with few mathematical developments. He showed that Ampère's electrodynamics and Faraday's law were compatible with the interpretation of the conduction current as a flow of electric atoms if Hertz's equations applied to the stationary ether and the Lorentz force acted on the electric atoms. He interpreted the polarization of material dielectrics as an elastically resisted shift of the electric atoms. He also sketched a dispersion theory based on Helmholtz's dynamic relation between ionic polarization and electromotive force. Unlike Lorentz, he said little on the optics of moving bodies, except for the aberration phenomenon; and he did not analyse the microscopic action of the electric atoms on the field.[90]

8.6.2 Cathode rays

The most original aspect of Wiechert's approach was his focus on the experimental grounding of his fundamental hypotheses. These were four:

[88] Wiechert 1894, 1896b: 2. Cf. Angenheister 1928; Heilbron 1964: 95–6; Hirosige 1966: 18–20; Jungnickel and McCormmach 1986, Vol. 2: 236.

[89] Wiechert 1894: 10; 1896b: 18 (*Bausteine*).

[90] Wiechert 1896a, 1896b, 1897.

1. Processes in free ether are ruled by Maxwell's equations.
2. Electricity is made of atoms.
3. Ether is stationary.
4. There is no direct influence of matter on the ether; all interactions proceed through the electric atoms.

The two first hypotheses were solidly anchored in Hertz's experiments and in the laws of electrolysis. The third was 'powerfully supported by aberration.' For the last, Röntgen's discovery provided the desired confirmation. As Wiechert explained in 1896, the lack of reflection and refraction of X-rays excluded a direct influence of matter on the ether, for such action—unlike the action via elastically bound ions—would not disappear at high frequency.[91]

The latter argument supposed the interpretation of X-rays as light of extraordinarily high frequency. Wiechert, like Schuster and J. J. Thomson, regarded X-rays as electromagnetic disturbances caused by the impact of cathode ray particles against a solid target. He judged Hertz's disproofs of the corpuscular view of cathode rays inconclusive, owing to the ionization of the residual gas. In his opinon, the cathode rays were ions or 'perhaps even electric atoms.' In order to decide this point, he set himself to determine the charge-to-mass ratio of these rays. Combining measurements of the magnetic deflection and the potential fall at the cathode, he found a much larger ratio than that for the hydrogen ion. This result surprised him, for it implied a velocity of the rays far above the 200 km/s measured by J. J. Thomson. A possible escape was to admit that only a small fraction of the potential fall at the cathode (about one volt instead of several hundred volts) served to accelerate the rays. Wiechert found this implausible, and came to doubt J. J. Thomson's velocity measurement: the latter could be flawed by delays in the fluorescence of the tube walls.[92]

In the fall of 1896, Wiechert became aware of Theodor Des Coudres's proof that the velocity of cathode rays was larger than found by J. J. Thomson. Des Coudres astutely submitted the cathode rays of a tube fed by a Tesla current to the magnetic action of a wire (abcd) fed by the same current (Fig. 8.4). In this device the rays are produced intermittently, at the frequency of the Tesla current ($10^6\,s^{-1}$), and the phase of the deflecting field is the same for each pulse. If the velocity of the rays were as small as J. J. Thomson thought, this phase would depend on the position of the wire abcd along the tube: the sign of the deflection would be reversed for a 10 cm shift. From the lack of such reversal, Des Coudres determined that the velocity had to be at least 10 times larger than was assumed by Thomson.[93]

Wiechert proceeded to improve this result. As the frequency of the tube current

[91] Wiechert 1896b: 44–7.

[92] Wiechert 1896b: 45 (with the surprising remark that the corpuscular view of cathode rays was most common in Germany at that time). For the history of his cathode ray experiments, cf. Wiechert 1898a: 260. Larmor also speculated that cathode rays could be free electrons (Larmor 1894: [524n]).

[93] Des Coudres 1895, 1896. Cf. Wiechert 1897: 13–15. J. J. Thomson had already used Tesla oscillations to produce his electrodeless discharges (J. J. Thomson 1891b) and to feed the tube on which he measured the velocity of cathode rays (J. J. Thomson 1894b).

could not be much increased, he ingeniously combined the deflections produced by two wire pieces abcd and efgh placed at different distances from the cathode and fed by a current of much higher frequency than the tube current (Fig. 8.5). Before the end of 1896, he demonstrated that the velocity of his rays was at least equal to 3×10^7 m/s, and probably not much higher than that. In combination with the magnetic curvature of the same rays, this gave a charge-to-mass ratio at least 2000 times, and probably not much larger than that of the hydrogen ion. Wiechert announced these results and displayed his apparatus on 7 January 1897, at the mathematico-

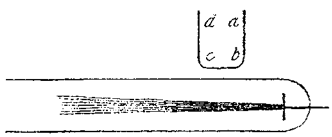

FIG. 8.4. Des Coudres's device for measuring the velocity of cathode rays (Wiechert 1897: 14).

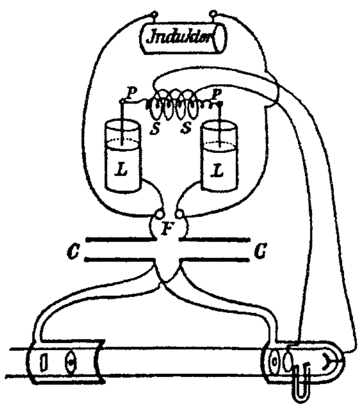

FIG. 8.5. Wiechert's device for measuring the velocity of cathode rays (Wiechert 1898a). The induction coil ('Induktor') and the spark gap F drive two oscillating circuits. The first, made of the Leyden jars L and the spiral PP of a Tesla transformer, induces in the secondary SS the tension for producing the cathode rays in the tube (each positive half-period of this tension produces a train of rays). The second, of a much higher frequency, consists in two plane condensers C and two wire loops. The combined action of one loop, the horseshoe magnet, and the first diaphragm (behind the anode) is used to modulate the cathode ray beam; the other loop acts on the beam further in the tube, in front of a second diaphragm followed by a fluorescent target. The luminosity of this target depends on the phase shift of the modulation during the travel of the rays through the tube. The velocity of the rays can be deduced from the variations of this luminosity when the traveling length and the modulation frequency vary.

physical society of Königsberg. The cathode ray particles, he concluded, were nothing but the electric atoms.[94]

So far, Wiechert's new electrodynamics had remained mostly a Königsberg affair. The discovery of the electric atoms in the cathode rays changed his attitude. In the following years he publicized his contribution to the electron theory, consolidated his cathode ray studies, and stimulated those of his new Göttingen colleagues. For the electric atom he adopted Stoney and Larmor's term: electron. However, he acknowledged Lorentz's priority in founding the electron theory of electrodynamics, and adopted the Dutch reticence toward further speculation on the nature of ether: 'Matter and ether,' he declared in 1901, 'are only pictures that we see in nature from our human standpoint; to decide what corresponds to these pictures in reality is left to the future progress of science.'[95]

8.7 Conclusions

Maxwell and his British followers were convinced that the molecular structure of matter should be taken into account in the electromagnetic theory of light. However, being reluctant to apply electric concepts at the molecular scale, they lacked means to specify the mechanism by which matter modified the properties of the ether. They were satistified with modifications of the macroscopic field equations, or they confined molecular considerations to the older elastic solid theory of light. As for the German Maxwellians of the early 1890s, they ignored every phenomenon that did not fit Maxwell's original field phenomenology. The physicists who first transcended this limitation had a more relaxed attitude toward Maxwell's theory.

Helmholtz, who originally grasped Maxwell through the electric shift concept of polarization, had no qualms about interpreting material dielectric polarization as a shift of ions. Lorentz, who started from Helmholtz's reinterpretation of Maxwell, proceeded similarly. Wiechert reified Helmholtz's 'atoms of electricity,' and tried to save the aspects of Weber's theory that had proved superior to Maxwell's. In Britain, no major theorist sought inspiration in the older German views of electricity. However, Larmor admitted Helmholtz's electrolytic ions, and he was temporarily interested in his polarization theory. More important, he agreed with William Thomson that Maxwell's theory lacked an ultimate dynamical medium, as intelligible as an elastic solid or a perfect fluid was. His valiant attempt at constructing atoms, the ether, and ions by combining MacCullagh's medium and the vortex theory of matter led to a crisis, at the end of which he transgressed essential parts of the Maxwellian dogma.

The pioneering electron theorists all agreed that the ether was stationary and that the interaction between ether and matter proceeded entirely through charged particles. However, they had different ways to justify this picture. Lorentz and Helmholtz

[94] Wiechert 1897: 15–6. Cf. Heilbron 1964: 96.

[95] Wiechert 1898b, 1899, 1901: 668. Wiechert refers to Larmor in 1896 (Wiechert 1896b: 48), to Lorentz only in 1898 (Wiechert 1898b: 92).

advocated simplicity, experimental adequacy, and abstract dynamics. They did not try to explicate the nature of their 'ions' and they did not attempt a general theory of matter. In contrast, Larmor and Wiechert interpreted their 'electrons' or 'electric atoms' as singularities in the ether. They wanted to eliminate the basic ether/matter duality and to build all matter from positive and negative electrons. While Wiechert was not too keen to pursue this speculation, Larmor explained how to construct electrons by perforating and refilling MacCullagh's medium, and imagined atoms made of rotating electrons.

The various founders of electron theory also differed in their physico-mathematical techniques. In conformity with the German preference for mathematical phenomenology, Helmholtz and Wiechert favored macroscopic methods: they referred to the microscopic picture only to justify modifications of macroscopic equations.[96] In contrast, Lorentz first scrutinized the interaction between electromagnetic waves and individual ions, and proceeded only at a later stage to the average macroscopic effects. He strove to develop an intuition of electromagnetic processes at the microscopic scale. Larmor was originally closer to Helmholtz's macroscopic methods, though for a different reason: he still depended on the Maxwellian tendency to treat the effect of matter on the ether as a modification of macroscopic field dynamics. Only after reading Lorentz did he evolve toward a more genuinely microphysical approach.

Lorentz's most innovative results concerned the optics of moving bodies. Granted that the ether was stationary, every terrestrial device was submitted to an ether wind corresponding to the fast motion of the Earth. Yet every attempt at optically detecting effects of this wind had failed. As Mascart saw it, optical phenomena seemed to depend solely on the relative motion of the implied material bodies. Lorentz first theoretical explanation of this result was indirect. Solving his equations for a wave traveling through a moving transparent body, he derived Fresnel's partial drag of the wave, which implies the first-order insensibility of the laws of refraction to the Earth's motion.

Later in the same year (1892), Lorentz found a more direct proof of the first-order invariance of optical phenomena. Here he inaugurated the important technique of 'corresponding states,' which Larmor soon adopted. The method was to bring back the differential equations for the fields in a moving system to the form they have in a system at rest, through a change of fields and coordinates involving the 'local time' $t - ux/c^2$. In Lorentz's and Larmor's view, this transformation was purely formal, and belonged to a standard strategy for solving differential equations. Only the original fields had a physical meaning as states of the ether. The solutions of the transformed equations had no direct interpretation (they pertained to a 'fictitious system'); physical conclusions could be drawn from them only by reversing the transformation.

Lorentz did not expect every electromagnetic phenomenon to be independent of

[96] Wiechert's interest in individual electrons grew after he studied Lorentz. For example, he derived the 'Liénard–Wiechert' formula for the retarded potentials from a moving electron (Wiechert 1900, Liénard 1898a).

the Earth's motion. However, by extending the technique of corresponding states to second order he could show that electrostatic phenomena did not depend on the Earth's motion. By the same method he found that if the effect of the Earth's motion on molecular forces was the same as for electrostatic forces, rigid terrestrial bodies should contract in the direction parallel to the velocity of the Earth. The latter effect explained the null result of the Michelson–Morley experiment of 1887. Larmor adopted this reasoning, with a personal touch: he took the Michelson–Morley experiment to confirm his ethereal concept of matter.

Lorentz's and Larmor's efforts were not limited to the optics of moving bodies. They both aimed at reducing all physics to the electron theory. Besides optics, their considerations included the various kinds of magnetism, thermostatistics (Larmor), magneto-optics (Lorentz and disciples), atomic models (Larmor), and even gravitation.[97] Helmholtz's contribution, influential though it was, was far more restricted: it dealt only with optical dispersion (leaving to Reiff the Fresnel coefficient). Wiechert said little on the optics of moving bodies, but was first to sketch an electronic theory of conduction, and the only electron theorist to identify the electron experimentally.

Wiechert's outstanding work on cathode rays, together with J. J. Thomson's slightly later announcement of the 'corpuscle,' brought experimental life to the theoretical electron and thus strengthened the electron theory. It also removed the ambiguity on the mass of the electron: originally, Lorentz and Larmor only knew that this mass could not exceed that of the smallest electrolytic ion. Other experimental findings contributed to the progress of the electron theory. First, the discovery of X-rays brought Helmholtz's ionic theory of dispersion to the forefront of physics. As we saw in Chapter 7, Wiechert, J. J. Thomson, and others used Helmholtz's formula to reconcile the electromagnetic interpretation of the rays with their lack of refrangibility. Conversely, Wiechert argued that the behavior of X-rays confirmed a basic tenet of electron theory: that matter acted on the ether only through the electrons. The Zeeman effect was another important input into the electron theory. Lorentz offered a first explanation of this effect, and the means to test it. From Zeeman's measurement, he estimated the charge-to-mass ratio of his optical ions. So did FitzGerald and Larmor for the mass of their electron.[98]

The electron theories gained much ground at the turn of the century. As a growing number of physicists came to realize, their explanatory power largely exceeded that of earlier theories, including Maxwell's. In Germany, Lorentz became the providential conciliator of Weberian microphysics and Maxwellian field theory. The oustanding expert on optics and electrodynamics, Paul Drude, gave up the Neumannian

[97] For magnetism, see Larmor 1894: [515–6, 518–9]; 1895b: 553–4, 597; 1897b: 637–8; 1897c: 114–17 (with a derivation of Curie's law); Lorentz 1902: 121–2, 128. For thermostatistics, see Larmor 1897b: 636–8; 1897c: 29–30, 81–106. For magneto-optics, see Wind 1898, 1898–99 (discussed in Buchwald 1985a: 245–7). For atomic models, see Larmor 1894: [516–7]; 1895: 597; 1900a: 233 (discussed in Heilbron 1964: 91–2). For gravitation, see Larmor 1900a: 187; Lorentz 1900*b* (discussed in McCormmach 1970b: 476–8).

[98] FitzGerald's condideration is reported in Lodge 1897b; Larmor 1897a: 506. Larmor's theorem is found *ibid.*: 504 (cf. Warwick 1993b; Robotti and Pastorino 1998).

and Maxwellian phenomenology and integrated Lorentz's essential ideas in his teachings and theories. In general, electron theory was perceived as a progressive and fruitful trend, even by those who chose not to work on it. In England, Larmor was the eloquent defender of the more British form he had given to it. His task was not an easy one: he faced the competition of J. J. Thomson's experimental microphysics, and criticism from other Maxwellians. Yet he managed to spread his philosophy of the ultimate medium through the Cambridge Mathematical Tripos and his prestigious *Aether and matter*. Electron theory was an international success, in consonance with the cosmopolitanism of its Dutch founder.

9

Old principles and a new world-view

9.1 Introduction

On the continent the electron theory became known mostly through Lorentz's *Versuch* of 1895. At that stage this theory still had obvious defects. Lorentz himself noted that 'a very simple fact,' the absence of first-order optical effects of the ether wind, appeared in his theory 'as a fortuitous consequence of rather complicated considerations.' For the Michelson–Morley experiment of 1887 he had to introduce a new assumption on the behavior of intramolecular forces. Moreover, he admitted that future experiments would perhaps require more assumptions. This situation prompted Henri Poincaré's famous 'Of hypotheses there is never a lack.' At the close of the century, the French mathematician scrutinized the main theories of electrodynamics, and diagnosed a severe crisis. The empirically most successful theory, Lorentz's, contradicted general principles that Poincaré believed to hold generally. In particular, Poincaré called for modifications of the electron theory that would make it compatible with the relativity principle.[1]

Another driving force of the new electron theory was the electromagnetic world-view. As we saw, Larmor and Wiechert dreamt of a world that contained nothing but the ether and its singularities, the latter's inertia being purely electromagnetic. At the end of the century, Wilhelm Wien turned this beautiful idea into a systematic research program that reduced all physics, even mechanics and gravitation, to electromagnetism. Some of Germany's most gifted theorists and experimenters contributed to this project. Their assumptions and results turned out to contradict Poincaré's principles. This chapter recounts the emergence, embodiments, and conflicts of the relativistic and electromagnetic ideals until 1905/6.

9.2 Poincaré's criticism

9.2.1 French isolationism

Poincaré was mainly a mathematician when, in 1886, he was elected to the chair of 'mathematical physics and probability calculus' at the Sorbonne. The practice of

[1] Lorentz 1898b: 102; Poincaré 1902: 202: 'Les hypothèses, c'est le fond qui manque le moins.'

reserving such chairs for pure mathematicians was then common in France, and reflected a sharp disciplinary separation between experimental and mathematical physics. The former was done by physicists who wished to remain as close as possible to the facts and avoided theoretical speculation. The latter was done by mathematicians who polished the form of existing theories and exploited their purely mathematical potentialities. This organization of French physics, and the correlative mix of empiricism and conservatism, tended to isolate and rigidify French physics. This is why we have so far been able to ignore French contributions to electrodynamics after Ampère without losing much understanding of the general evolution of the subject.[2]

Admittedly, there were a few outstanding French contributions to electric measurement, and some theoretical additions to the Amperean heritage. But these were methodologically conservative, and they did not bring any fundamental innovation. French physicists and mathematician met the newer foreign trends with skepticism or even hostility. The dean of French mathematical physics, the Acamemician Joseph Bertrand, condemned Maxwell's *Treatise* for the lack of mathematical rigor and the abundance of arbitrary assumptions. In Helmholtz's hydrodynamics and electrodynamics he found mistakes that betrayed only prejudices and a poor command of the German idiom. British- and German-style theoretical physics had no place in France, for they involved tangles of physical, mathematical, and experimental arguments that were alien to French practices and institutions. Add to this incompatibility a touch of chauvinism, and the relative invisibility of French electrodynamics is explained.[3]

Fortunately, there were a few exceptions to the French isolationism. Alfred Potier, whom Poincaré knew as a *répétiteur* at the Polytechnique, arranged the translation of Maxwell's *Treatise* in 1885. Mascart and Joubert included a chapter on Maxwell's theory in their authoritative *Leçons sur l'électricité et le magnétisme* of 1882. Maxwell's text was already well known to French telegraphic engineers, who saw in it a mine of methods for solving practical problems of electricity. The leading French journal of electrical engineering, *L'éclairage électrique*, published translations or detailed accounts of foreign works, including Helmholtz's and Hertz's most theoretical memoirs.[4]

9.2.2 The Sorbonne lectures

Through his training at the Ecole Polytechnique and at the Ecole des Mines, Poincaré had access to these French inlets of openness. His teaching at the Sorbonne was far more varied and adventurous than that normally dispensed from similar chairs. He examined the newest foreign theories and discussed fresh experiments, although he did not perform any himself. He had an active interest in technological questions, as his numerous contributions to *L'éclairage électrique* demonstrate.

[2] Cf. Atten 1988b, 1992, 1996. [3] Bertrand 1891. Cf. Helmholtz 1868; Atten 1992, 1996: 37.
[4] Maxwell 1885–1889; Mascart and Joubert 1882–1886: Ch. 6. Cf. Atten 1988a, 1992; Walter 1997.

Lastly, he excelled in popularizing science and wrote a best-seller on wireless telegraphy.[5]

In the spring of 1888 Poincaré taught a course on Maxwell's electromagnetic theory, as a natural continuation of his previous lectures on the mathematical theories of light. His presentation of Maxwell is characteristic of his method: he read scientific texts quickly as a whole, and reconstructed the reasonings in his own manner. The result was often clearer than the original, revealed some essential features in full light, but overlooked other important ones. Specifically, he improved Maxwell's mathematical derivations; he highlighted his use of the Lagrangian method as a promising attenuation of mechanical reductionism; but he missed what the Maxwellians took to be the essence of Maxwell's theory, the reduction of electric charge and current to ether processes.[6]

In 1889, 1892, and 1899, Poincaré lectured on the electrodynamics of other foreign masters: Helmholtz, Hertz, Larmor, and Lorentz. In his mind such eclectism was necessary to prevent dogmatism. Not fearing intellectual strabismus, he multiplied comparisons and connections between the various theories. He was first to provide a detailed proof that Maxwell's theory was 'nothing but a limiting case of Helmholtz's'; he showed how Hertz's equations could be derived from Maxwell's; he bridged Larmor's theory with Neumann's old theory of light; he found the modifications of Hertz's equations that led to Lorentz's macroscopic field equations; and he showed how Larmor 'appropriated Lorentz's assumptions and combined them with his own.'[7]

Poincaré based these comparisons on the differential equations of the theories, which he re-expressed in standardized notation (Maxwell's) and units (electromagnetic ones). This French emphasis on mathematical equipment could obscure the contrast between different theories. For example, true Maxwellians would have rejected the statement that Maxwell's theory was a limiting case of Helmholtz's: for them Maxwell's reform of the concept of electricity was most essential, whereas Poincaré only cared to retrieve the empirical predictions, the *rapports vrais* of Maxwell's theory.[8]

Yet Poincaré sensed the individuality of each theory and tried to convey its inventor's style. His Maxwell 'd[id] not try to construct a unique, definitive, and well-ordered edifice, but rather seem[ed] to erect a great number of provisional and independent constructions, among which communication [was] difficult and sometimes impossible.' He portrayed Hertz as a rigorist who 'only admitted the equations established by Maxwell, left aside Maxwell's classical text, regarding it as obscure, and tried, by setting the final equations in advance, to make a theory that leads to them.' He saw in Larmor a clever reactionary who 'sought a common mechanical explanation for light and electricity, and [for this purpose] returned to [the older] elastic theories [of optics].' Lastly, his Lorentz was the

[5] Cf. P. Langevin 1914; Broglie 1954; Darrieus 1954; Wien 1954.
[6] Poincaré 1890. Cf. Darrigol 1993a: 215–22; 1995a: 6–8.
[7] Poincaré 1891: 83; 1894; 1901a: 345–52, 587, 627.
[8] On the notion of '*rapports vrais*,' cf. Poincaré 1900b, 1902: Ch. 10; and Stump 1989.

champion of a new microscopic vision that eliminated true magnetism and conduction currents and replaced them with circulating electrons. In his eyes diversity was a virtue: 'Each has his characteristic aptitudes, and these aptitudes should be diverse, else would the scientific concert resemble a quartet where every one wanted to play the violin.'[9]

What made Poincaré's lectures especially attractive was his luminous style of exposition. His mathematical derivations were supremely elegant, and his French beautifully clear. He was very careful to state all necessary hypotheses, but avoided the excessive abstraction of a purely axiomatic presentation. For pedagogical reasons, he preferred an inductive presentation, and provided expressive analogies for the most difficult statements. The appeal of these qualitities was not limited to French readers. The Maxwellian Andrew Gray applauded: 'Here are to be found exemplified that order and harmony which renders the work of the best French mathematical writers so exquisitely clear, and that artistic charm which is so seldom seen in the writings of scientific men of other nationalities.'[10]

More faithful Maxwellians were not so pleased with Poincaré's rendering of Maxwell's theory. FitzGerald correctly noted that Poincaré had completely misunderstood Maxwell's basic notion of electric displacement and that none of the paradoxes perceived by the French mathematician actually existed. The Cerberus of French orthodoxy, Joseph Bertrand, condemned Poincaré's lectures for the opposite reason: his young colleague brought home the grave lack of rigor of Maxwell's speculations. Fortunately, some French physicists had a more open attitude; and by the end of the century Poincaré became the supreme authority on electric theory in his own country. The Germans were most receptive to Poincaré's lectures. They usually preferred these to Maxwell's original text, and used them to write their own textbooks. They were especially receptive to the criticism of Maxwell's pictures. What Poincaré had declared unintelligible, for example the electric displacement, they usually omitted from their texts.[11]

9.2.3 The physics of principles

In his discussion of Maxwell's views on mechanical explanation, Poincaré distinguished between 'phenomenological laws' and 'mechanical interpretation': the laws are concerned with 'the parameters than can be directly reached and measured by experience' and with the differential equations giving their evolution, whereas the mechanical interpretation reduces the laws to the Newtonian motion of ordinary matter and hypothetical fluids. Poincaré then proved the following theorem: 'If a phenomenon admits a mechanical explanation, there exists an infinity of other mechanical explanations that account equally well for all aspects revealed by experience.' In the face of such arbitrariness, he recommended the principle of least

[9] Poincaré 1890: IV; 1901a: 344, 583, 422; 1929: 4.

[10] Gray 1891.

[11] FitzGerald 1892; Bertrand 1891. On the French reception, cf. Atten 1992; Darrigol 1995a: 8. On the German reception cf. Darrigol 1993a and 1995a: 8–10.

action, which implied the possibility of a mechanical representation without giving it. He regarded this transformation of the mechanistic ideal as Maxwell's greatest achievement.[12]

Poincaré perceived an inevitable evolution from the Newtonian or Laplacian ideal to a 'physics of principles':

> A day arrived when the conception of central forces no longer appeared sufficient [. . .]. What was done then? The attempt to penetrate into the detail of the structure of the universe, to isolate pieces of this vast mechanism, to analyze one by one the forces which put them in motion, was abandoned, and we were content to take as guides certain general principles the express object of which is to spare us this minute study.

Among the general principles, Poincaré included various principles of mechanics (least action, relative motion, conservation of mass, equality of action and reaction, conservation of energy), and additional thermodynamic or electrodynamic principles (Carnot's principle, conservation of electric and magnetic charge, unity of electric and magnetic force). Like Helmholtz, he meant to compromise between the older Newtonian ideal and pure phenomenology.[13]

No more than Helmholtz or Thomson did Poincaré believe in a transcendent truth of the principles. He gave them an inductive origin:

> The principles are results of experiments boldly generalized; but they seem to derive from their very generality a high degree of certainty. In fact, the more general they are, the more frequent are the opportunities to check them, and the verifications multiplying, taking the most varied, the most unexpected forms, end by no longer leaving place for doubt.

On some occasions, Poincaré famously argued that the principles of mechanics, despite their empirical origin, had become completely irrefutable and now acted as definitions or conventions. When facing empirical contradiction, the principles could always be saved by introducing invisible entities. However, Poincaré only said so to clarify the logical status of the principles. In his own practice of physics, he refused to save the principles by recourse to immaterial entities, for they would thus cease to control observable phenomena and become sterile.[14]

In his Sorbonne lectures, Poincaré judged the various electrodynamic theories according to their compatibility with general principles. For example, he found (mistakenly) that Maxwell's ether stresses were incompatible with energy conservation; agreed with Hertz that Helmholtz's theory contradicted the unity of the electric force; and found that the Helmholtz–Reiff theory of dispersion violated the conservation of charge. Only Hertz's theory complied with all relevant principles, including the principle of relative motion and the equality of action and reaction. The last judgment may surprise the reader who remembers that Hertz himself denounced the existence of the unbalanced force $\partial(\mathbf{D} \times \mathbf{B})/\partial t$ in his theory. Poincaré reinterpreted Hertz's theory as a field theory in a moving *material* medium, dilute matter being

[12] Poincaré 1890: IX, XIV. Cf. Stein 1981: 310–11. [13] Poincaré 1904b: 299.
[14] Poincaré 1904b: 301; 1901c (conventions in mechanics); 1902: 196 (against sterile principles).

assumed even in the most perfect vacuum. Then the Hertz force had something to act on.[15]

The trouble with Hertz's theory, as Hertz and Poincaré both lamented, was that it implied a complete drag of light waves in a moving transparent body, against Fizeau's result. Only Lorentz's theory gave the correct drag, as well as numerous other results of the optics of moving bodies. Poincaré judged that this theory was 'the one that best account[ed] for the facts' or 'the least defective.' Yet he found it to contradict the principle of relative motion and the principle of reaction. In order to understand the depth of this dilemma one must first consider Poincaré's opinion about the role of the ether.[16]

9.2.4 Vanishing ether

In 1888, in the foreword of his lectures on the mathematical theory of light, Poincaré wrote:

> It matters little whether the ether really exists; that is the affair of the metaphysicians. The essential thing for us is that everything happens as if it existed, and that this hypothesis is convenient for the explanation of phenomena. After all, have we any other reason to believe in the existence of material objects? That, too, is only a convenient hypothesis; only this will never cease to do so, whereas, no doubt, some day the ether will be thrown aside as useless.

Poincaré's skepticism was partially rooted in his observation that in optics, numerous mutually contradictory theories of the elastic ether were equally fit to represent the phenomena. One may also invoke the regnant empiricism of French physics in this period, and of course the repeated failures to detect effects of the ether wind. At any rate, Poincaré perceived a gradual fading of the ether concept from Fresnel to Hertz. As he recalled, Fresnel analyzed his ether in terms of the mechanical interactions of a system of molecules. Maxwell only assumed a hidden mechanical motion; the internal stresses, which gave his ether more body, Poincaré judged incompatible with the rest of the system. Finally, Hertz's theory, according to Poincaré, completely eliminated the ether.[17]

If the ether was given a new life, Poincaré believed, it could only be as a ghost. Motion with respect to the ether had to remain undetectable, and the ether could not carry momentum. In other words the principle of relative motion and the principle of reaction had to hold for matter *alone.* Unfortunately, Lorentz satisfied the first principle only partially and artificially, and crudely violated the second.

Poincaré despised the '*coup de pouce*' of the Lorentz contraction, and raised the question of the higher orders: 'I consider it very probable that optical phenomena depend only on the relative motion of the material bodies present—light sources and apparatus—*and this not only to first or second order [. . .] but*

[15] Poincaré 1890: 91–92 (Maxwell); 1891: 114–117 (Helmholtz); 1901a (Helmholtz–Reiff); 1894: 337; 1895; 394–402; 1901a: 345–420 (Hertz). Cf. Darrigol 1993a: 219–220; 1995a: 11–16.

[16] Poincaré 1901a: II, 611.

[17] Poincaré 1889: I.

exactly.' Against the opposite view that made the ether a legitimate reference body, he argued: 'Is it not evident that from the principle so understood we could no longer infer anything? It could no longer tell us anything because it would no longer fear any contradiction.'[18]

With its clear separation between ether and matter, Lorentz's theory also contradicted the reaction principle. The net sum of the forces acting on the ions is equal to the integral of the Hertz force $\partial(\mathbf{E} \times \mathbf{H})/c\partial t$, which in general does not vanish. For example a beam of electromagnetic radiation would exert pressure on a conductor. Or a source emitting radiation in one direction would recoil in the opposite direction. Maxwell had already contemplated the first effect. It was not problematic in his theory, because his ether, which carried energy, sustained stresses, and moved together with matter, could just as well carry momentum. In contrast, in Lorentz's theory Poincaré perceived a violation of the reaction principle, for the relevant ether could not carry as crude a material attribute as momentum was.[19]

9.2.5 *The crisis*

Being dissatisfied with Lorentz's theory, Poincaré examined whether Hertz's theory could be modified to include Fizeau's result and yet remain compatible with the energy principle, the reaction principle, and the conservation of electricity and magnetism. The negative conclusion of his sophisticated mathematical analysis pointed to a fundamental incompatibility between Fizeau's result and general principles. Intuitively, the partial dragging of light waves seemed to make the ether a body capable of mechanical reaction. As Poincaré put it, this effect 'seems to show us two different media interpenetrating and yet moving one with respect to the other; we seem to be touching the ether with the finger.'[20]

At that point Poincaré could have given up the generality of the reaction principle, as Lorentz himself suggested. He did not do so, because he believed that any violation of the reaction principle implied, together with the relativity principle, the possibility of perpetual motion. To show this absurdity, he considered two bodies, initially at rest, and isolated from other bodies. Suppose that the action of one body on the other differs from the reciprocal action. And let us connect the two bodies with a rigid bar. The resulting system starts to move. According to the principle of relative motion, the net force is not affected by this motion. Therefore, the system undergoes a forever accelerated motion.[21] In the face of such paradoxes, Poincaré regarded the incompatibility of Fizeau's result with the reaction principle as the symptom of a major crisis of electrodynamics: 'Some day,' he prophesied in 1895, 'we shall have to modify our ideas in some important point

[18] Poincaré 1901a: 536 (his emphasis); 1904b: 306.

[19] Poincaré 1895: 391–392. Cf. Darrigol 1995a: 17–19.

[20] Poincaré 1895: 395–413; 1900b: 149. Poincaré's physics professor at the Polytechnique, Alfred Cornu, regarded the Fizeau experiment as a direct experimental proof of the existence of the ether (Cornu [1894–1895]: 207).

[21] Poincaré 1900a: 270. A similar argument in found in the scholium to the laws in Newton's *Principia* (I thank Friedrich Steinle for this remark), and in Ampère 1826b: 1–2.

and break the frame in which we try to fit both optical phenomena and electrical phenomena.'[22]

In 1900, at the occasion of Lorentz's jubilee, Poincaré returned to the way in which Lorentz theory violated the principle of reaction. He introduced a fictitious fluid with the momentum density $(1/c)\mathbf{E} \times \mathbf{H}$ (in Hertz's units). Adding this fluid to the system of material bodies, the conservation of momentum *formally* held. Poincaré further ascribed to the fictitious fluid a mass density, obtained by dividing the energy density of the electromagnetic field by c^2. Whenever some of the field energy was absorbed by matter, he had the fictitious fluid go to rest and remain latent in space, in an energy-less state. With this deliberately artificial assumption, he obtained the uniform motion of the center of mass. Careful reading of his text shows that he did not wish to ascribe any physical meaning to the fictitious fluid. On the contrary, he regarded the contributions of the fictitious fluid to the momentum and to the inertia of the system as measures of the *physical* violation of the principle of reaction.[23]

Another concern of Poincaré's was to show that the violation of the reaction principle in Lorentz's theory implied a corresponding violation of the relativity principle. For this purpose, he considered the emission of a parallel beam of electromagnetic waves by a source moving at the velocity u through the ether, in the direction of the waves. For an observer at rest, the energy emitted by the source is equal to the energy J of the emitted radiation, plus the work $(-J/c)u$ performed by the recoil impulse $-J/c$ (the high inertia of the source prevents a significant change of velocity). For an observer moving together with the emitter, the recoil force does not work, and the observer must ascribe the energy $J(1 - u/c)$ to the radiation, if energy is still to be conserved. To this correction of the emitted energy there corresponds a correction $-Ju/c^2$ of the recoil impulse. Poincaré regarded this tiny 'complementary force' as a first-order violation of the relativity principle. Again, he spied a need 'to deeply modify our ideas on electrodynamics.'[24]

9.2.6 Apparent states

While discussing this thought experiment, Poincaré showed that the moving observer's estimates of the radiation energy and the recoil force agreed with those computed from Lorentz's 'corresponding states' of the electromagnetic field. He expected this agreement because he interpreted the corresponding states as those observed by moving observers ignoring their motion in the ether. This was a major new insight to be opposed to Lorentz's and Larmor's ascription of the corresponding states to a fictitious system at rest. Poincaré could now directly use Lorentz's transformation to deduce the results of observations performed in a moving system

[22] Poincaré 1895: 412.

[23] Poincaré 1900a: 253–8. Cf. Darrigol 1995a: 23–5.

[24] Poincaré 1900a: 273–8. Poincaré's reasoning was slightly more general: it did not assume the equality of the velocities of the emitter and the moving observer. Alfred Liénard had already deduced the complementary force from Lorentz's theory (Liénard 1898b: 323–4).

of reference, whereas Larmor and Lorentz always had to return to the 'real' field in the ether.[25]

In particular, Poincaré interpreted the local time as that given by the following procedure of synchronization:

> I suppose that observers placed in different points set their watches by means of optical signals; that they try to correct these signals by the transmission time, but that, ignoring their translatory motion and thus believing that the signals travel at the same speed in both directions, they content themselves with crossing the observations, by sending one signal from A to B, then another from B to A. The local time t' is the time indicated by watches set in this manner.

To first order this procedure leads to Lorentz's expression for the local time. The proof, which Poincaré left to his reader, goes as follows.[26]

When B receives the signal from A, he sets his watch to zero (for example), and immediately sends back a signal to A. When A receives the latter signal, he notes the time t that has elapsed since he sent his own signal, and sets his watch to the time $\tau/2$. By doing so he commits an error $\tau/2 - t_-$, where t_- is the time that light really takes to travel from B to A. This time, and that of the reciprocal travel, are given by

$$t_- = \frac{AB}{c+u}, \quad t_+ = \frac{AB}{c-u}, \tag{9.1}$$

where u is the common velocity of the two observers with respect to the ether. The time τ is the sum of these two traveling times. Therefore, to first order in u/c the error committed in setting the watch A is

$$\frac{\tau}{2} - t_- = \frac{t_+ - t_-}{2} = \frac{uAB}{c^2}. \tag{9.2}$$

In other words, at a given instant of the true time, the times indicated by the two clocks differ by uAB/c^2, in conformity with Lorentz's expression of the local time.

Poincaré transposed this synchronization procedure from his earlier discussion on the measurement of time, published in 1898. There he noted that the dating of astronomical events was based on the implicit postulate 'that light has a constant velocity, and in particular that its velocity is the same in all directions,' and he gave the above method of clock synchronization, though only for clocks at rest. The generalization to moving clocks resulted from the relativity principle: moving

[25] Poincaré 1900a: 274–7.

[26] Poincaré 1900a: 272. Most historians have overlooked this aspect of Poincaré's memoir. They usually state that Poincaré's interpretation of the local time first appeared in Poincaré 1904b, unless they give Einstein complete precedence on this matter. Exceptions are Scribner 1964; Cuvaj 1970a: 77–8; Stachel *et al.* in *ECP* 2: 308n. See also Thirring 1927: 270n.

observers could not detect their motion in the ether, and therefore could only do as if they were at rest. Hence moving clocks had to be synchronized in such a way that the velocity of light measured by means of these clocks would still be equal to c.[27]

This reasoning of Poincaré's, as well as his diagnosis of a major crisis of electrodynamics, depended on a highly original conception of the principles of relativity and reaction. The physicists who shared his endeavor to dematerialize the ether, for example Drude and Cohn, felt free to violate the general principles of mechanics. Those who, on the contrary, maintained a variety of mechanical reductionism, for example Boltzmann and (to a lesser extent) Lorentz, treated the ether as part of the mechanical system, and did not expect mechanical principles to apply to matter alone. Poincaré was unique in conjugating the general principles of mechanics and a mechanically irrelevant ether. At the turn of the century, only he detected a crisis and predicted a major alteration of Lorentz's views. Other theorists saw nothing very wrong in Lorentz's theory, and some of them imagined new promising developments, as we will now see.

9.3 The descent into the electron

9.3.1 The electromagnetic world-view

Since J. J. Thomson's old study of electric convection (1881), it was known that an electrically charged particle had an electromagnetic inertia. In their electron theories Larmor and Wiechert speculated that all inertia was of electromagnetic origin, and that mechanics could be reduced to electromagnetism. In his contribution to the Lorentz jubilee, Wien turned the speculation into a program: an electromagnetic model of the electron had to be constructed, and its consequences tested. As Wien knew from the calculations of Heaviside's disciple George Searle, the mass of an electron, if electromagnetic, had to depend on velocity when the velocity approached that of light. Earlier experiments by Lenard on highly accelerated cathode rays seemed to display this effect. Within a few months the Göttingen experimenter Walther Kaufmann confirmed it by studying the electric and magnetic deviations of fast electrons from radium salts.[28]

Kaufmann's colleague at Göttingen, Max Abraham, soon offered an elegant, quantitative theory of the electromagnetic electron. A thoroughly electromagnetic picture, he argued, implied that the electron should be ideally rigid. The charge distribution of his electron was maintained by a purely kinematic constraint of rigidity, so that the energy of this particle could only change by the work of electromagnetic forces. He assumed a spherical distribution of charge, and derived the dynamic properties of the electron in the quasi-stationary approximation. The calculations involved

[27] Poincaré 1900a: 272; Poincaré 1898: 232–3.

[28] J. J. Thomson 1881a; Larmor 1895a, 1895b; Wiechert 1894; Wien 1900; Kaufmann 1901. Cf. McCormmach 1970b; Miller 1981a: 45–54. On Kaufmann, cf. Cushing 1981; Hon 1995.

Lorentz's technique of corresponding states and the concept of electromagnetic momentum.[29]

Abraham claimed to have borrowed the latter notion from Poincaré. In fact, Poincaré had shown that a formal conservation law held for the mechanical momentum of the electrons and the integral of $(1/c)\mathbf{E} \times \mathbf{H}$ over all space, but judged the latter contribution to be pathological. On the contrary, Abraham denied that the first contribution existed at all, and gave an electromagnetic origin to all momenta. He therefore regarded $(1/c)\mathbf{E} \times \mathbf{H}$ as a real electromagnetic momentum. Yet Abraham's divorce from mechanics was not total. The very notion of momentum was mechanical. Moreover, he put the field equations in Lagrangian form, and derived a Lagrangian for the motion of the electron.[30]

Abraham's theory won immediate success: it gave a precise expression to Wien's program, and offered opportunities for interesting mathematical developments. Göttingen physicists well-versed in higher mathematics, mainly Arnold Sommerfeld, Karl Schwarzschild, and Paul Hertz, worked out the subtleties of the electromagnetic electron's motion and disserted on supraluminal motion. In 1905, with Wiechert's complicity the Göttingen mathematicians David Hilbert and Hermann Minkowski organized a very learned seminar on electron theory. By that date the subject had become highly technical and yielded papers that few physicists could understand. It nonetheless had considerable prestige, in part because in 1903 Kaufmann had confirmed Abraham's mass formulas, but also because it had a revolutionary flavor as a challenge to the old mechanical world-view.[31]

9.3.2 Lorentz's contractile electron

The cautious Lorentz gave only soft support to the electromagnetic world-view. He was not sure that all forces and all masses had an electromagnetic origin. As we saw, he used the transformation properties of electromagnetic forces to justify the Lorentz contraction. However, he only supposed that all forces should behave *as if* they were of electromagnetic origin during a global translation of the system. Lorentz's interest in the model and dynamics of the electron arose not from a belief in the electromagnetic world-view, but from three different circumstances: he wanted to answer Poincaré's criticism regarding the *ad hoc* cumulative character of his assumptions; he needed to explain various new ether drift experiments, especially Rayleigh's and Brace's failures to find the double refraction implied by the Lorentz contraction of transparent bodies; he had to take experiments on the deviation of fast electrons into account.[32]

[29] Abraham 1902. Cf. Goldberg 1970b; Miller 1981a: 55–61. See the next section on Lorentz for further description of Abraham's methods.

[30] Abraham 1902. Cf. Miller 1981a: 55–61.

[31] Cf. Pyenson 1979.

[32] Lorentz 1895, 1901, 1904a; Rayleigh 1902; Brace 1904. Cf. Hirosige 1969; McCormmach 1970b; Miller 1981a: 67–70. Other important experiments were that of Trouton and Noble concerning the couple exerted by the ether wind on a charged capacitor (cf. Warwick 1995 for the multiple contexts of this experiment and FitzGerald's and Larmor's involvements, and Janssen 1995: Ch. 1 for a thorough discussion of Lorentz's and Larmor's interpretations), and Liénard's imaginary version of the Michelson–Morley experiment in a high-index dielectric (discussed in Lorentz 1899).

In April 1904 Lorentz's efforts yielded an important Dutch memoir entitled: 'Electromagnetic phenomena in a system moving with any velocity less than that of light.' The basic assumptions were: the field equations for the stationary ether and the Lorentz force; that a global translation of the system altered all forces as if they were of electromagnetic origin; that a moving electron was longitudinally contracted at the rate $\gamma^{-1} = (1 - v^2/c^2)^{1/2}$; that all mass had an electromagnetic origin. From these assumptions Lorentz concluded that optical phenomena did not depend on a global, uniform, translatory motion of the system at any order, and he derived new expressions of the masses (transverse and longitudinal) of the electron.[33]

Lorentz first wrote the field equations with respect to a moving system of axes, that is, in modern terminology, he performed a Galilean transformation. Then he exactly retrieved the form of the equations with respect to the ether—except for the source terms—through the transformation

$$x' = \gamma\varepsilon x, \quad y' = \varepsilon y, \quad z' = \varepsilon z, \quad t' = \varepsilon(\gamma^{-1}t - \gamma ux/c^2),$$
$$\mathbf{d}' = \varepsilon^{-2}(1, \gamma)[\mathbf{d} + (\mathbf{u}/\mathbf{c}) \times \mathbf{h}], \quad \mathbf{h}' = \varepsilon^{-2}(1, \gamma)[\mathbf{h} - (\mathbf{u}/\mathbf{c}) \times \mathbf{d}], \qquad (9.3)$$

where the symbol $(1, \gamma)$ means a multiplication by 1 of the component of the following vector parallel to **u**, and the multiplication by γ of the component perpendicular to **u**. This transformation, composed with the first, Galilean transformation, yields what Poincaré later called the 'Lorentz transformation' for space–time coordinates and fields, up to a global scale factor ε. Lorentz already had this transformation in 1899, but was not able to draw full profit from it at that time.[34]

The transformations for velocity, current, and charge of the 1904 memoir differed from those which we now consider to be correct. Nevertheless, Lorentz could show that dipolar emission in the moving system transformed into dipolar emission in a system at rest. The assumption that intermolecular forces behaved like electromagnetic forces with respect to motion further implied that the image of this body through the transformation (9.3) would have the dimensions of the same body at rest. Next, an alteration of the dimensions of the electrons in the proportions $(\varepsilon^{-1}\gamma^{1}, \varepsilon^{-1})$, implied that the image of the electron would be similar to an electron at rest. In order to prove the invariance of optical phenomena with the technique of corresponding states, Lorentz needed one more result: that the transformed equation of motion of an electron was the same as the equation in the ether system. The reasoning involved Abraham's method transposed as follows to the contractile electron.[35]

The electromagnetic momentum of an electron moving at the constant velocity **u** is, by definition,

[33] Lorentz 1904a. Cf. Miller 1981a: 70–5; Nersessian 1986; Paty 1993: 45–8; Darrigol 1994a: 292–8.

[34] Lorentz 1904a, 1899. Larmor 1897c already had the exact expression of the Lorentz transformation for coordinates and fields, but had not yet realized that this transformation leaves the Maxwell equations (without sources) invariant *at any order*.

[35] Lorentz 1904a: 177–89.

$$\mathbf{p} = \frac{1}{c}\int \mathbf{d} \times \mathbf{h} \mathrm{d}\tau, \tag{9.4}$$

where **d** and **h** represent the electromagnetic field of the electron. Through the transformation (9.3), this becomes

$$\mathbf{p} = \frac{1}{c^2}\varepsilon\gamma \int \mathbf{d}'^2_\perp \mathrm{d}\tau' \tag{9.5}$$

where $\mathbf{d}'_\perp$ is the perpendicular component of the electric field of the transformed electron ($\mathbf{h}'$ is zero), which is at rest. If the charge is uniformly distributed over a sphere of radius R, the result is

$$\mathbf{p} = m_0\gamma\varepsilon\mathbf{u}, \quad \text{with } m_0 = e^2/6\pi Rc^2. \tag{9.6}$$

Like Abraham, Lorentz extended this expression to moderately accelerated electrons (quasi-stationary approximation). Then the accelerating force may be expressed in terms of the components of the acceleration **a** according to

$$\mathbf{f} = \frac{\mathrm{d}\mathbf{p}}{\mathrm{d}t} = m_{\parallel}\mathbf{a}_{\parallel} + m_\perp \mathbf{a}_\perp, \tag{9.7}$$

with

$$m_{\parallel} = m_0 \frac{\mathrm{d}\varepsilon\gamma u}{\mathrm{d}u}, \quad m_\perp = m_0\gamma\varepsilon. \tag{9.8}$$

This seducingly simple result challenged Abraham's complex, logarithmic expressions for the two masses. Lorentz verified that Kaufmann's data of 1903 were not accurate enough to decide between the two models. His main purpose, however, was to show the invariance of the equation of motion (9.7) as follows.[36]

On the one hand, a double differentiation of the coordinate transformation (9.3) yields

$$a'_{\parallel} = \varepsilon^{-1}\gamma^3 a_{\parallel}, \quad a'_\perp = \varepsilon^{-1}\gamma^2 a_\perp. \tag{9.9}$$

On the other, Lorentz assumes that the force **f** transforms like the electromagnetic force $e[\mathbf{d} + (\mathbf{u} + \mathbf{v}) \times \mathbf{h}/c]$. Neglecting the relative velocity **v**, this gives

$$\mathbf{f}'_{\parallel} = \varepsilon^{-2}\mathbf{f}_{\parallel}, \quad \mathbf{f}'_\perp = \gamma\varepsilon^{-2}\mathbf{f}_\perp. \tag{9.10}$$

Consequently, the equation of motion (9.7) transforms into the equation $\mathbf{f}' = m_0\mathbf{a}'$ for slowly moving electrons if and only if

[36] Lorentz 1904a: 191–4.

$$\frac{\mathrm{d}\varepsilon\gamma u}{\mathrm{d}u}=\varepsilon\gamma^3. \tag{9.11}$$

This is equivalent to ε = constant. The constant must be unity, since for $u = 0$ there is no contraction or dilation of lengths.[37]

Lorentz's complex memoir met Poincaré's requirement that a common explanation should be given to the invariance of optical phenomena at any order. However, it implicitly admitted non-electromagnetic forces for the cohesion of the deformable electron, as Abraham promptly noted. Not only did this admission contradict the electromagnetic world-view, but it was not clear whether such forces could be introduced in a consistent manner. Moreover, Lorentz's conclusions required certain approximations, for instance the dipolar approximation, the approximation of small relative velocities of the optical electrons, and the neglect of their spinning motion. They required long, indirect reasoning, because Lorentz, unlike Poincaré, still regarded his 'corresponding states' as belonging to a fictitious system at rest. The resulting complexity did not overly perturb Lorentz, for he still believed that motion with respect to the ether could in principle be detected.[38]

9.3.3 Poincaré's dynamics of the electron

Poincaré reacted enthusiastically to Lorentz's memoir. He saw that with few improvements he could obtain the desired exact invariance and full compatibility with what he now called 'the relativity postulate.' His '*dynamique de l'électron*' appeared in summary form in the *Comptes rendus* for June 1905, and more fully in the Palermo memoir of 1906. First he gave 'the Lorentz transformations' of coordinates, field, velocity, charge, and current in their exact and modern form, as forming the invariance group of the Maxwell–Lorentz equations. Exploiting the group structure, which Lorentz's two-step procedure had hidden, he eliminated the possibility of a global rescaling in a manner much simpler than Lorentz. He also gave the quadratic invariant of the group, and introduced the imaginary time coordinate for which the transformations formally become four-dimensional rotations.[39]

In conformity with the thought experiment he had discussed in the *Lorentzfestschrift*, Poincaré found that the Lorentz force was not invariant. However, he no longer believed that this constituted a violation of the relativity principle. If all forces, *including inertial forces*, transformed like electromagnetic forces, Poincaré reasoned, then the conditions of dynamic equilibrium were invariant because the condition that the total force (external plus inertial) should be zero was invariant. Conversely, in order to respect the relativity principle all forces had to transform like electromagnetic forces; that is, according to a representation of the

[37] Lorentz 1904a: 188–9. [38] Cf. Miller 1981a: 75–9.

[39] Poincaré 1905, 1906. Cf. Scribner 1964; Holton 1964; Goldberg 1967, 1970a; Cuvaj 1968, 1970a, 1970b; Miller 1973, 1981a: 79–86; Schaffner 1976; Keswani and Kilmister 1983; Paty 1987, 1993: 49–52.

Lorentz group. Poincaré required that the cohesive pressure of the electron should be Lorentz invariant and thus obtained a consistent, covariant dynamics of the contractile electron. He also attempted a covariant generalization of Newton's gravitation theory in which the gravitational force propagated at the velocity of light.

Clearly, Poincaré identified the formal condition of Lorentz invariance with the principle of relativity. On this point his only comment was:

> The reason why we can, without modifying any apparent phenomenon, confer to the whole system a common translation, is that the equations of an electromagnetic medium are not changed under certain transformations which I shall call *the Lorentz transformations*; two systems, one at rest, the other in translation, thus become exact images of one another.

Implicity, Poincaré meant that the Lorentz-transformed quantities described the apparent phenomena. In particular, the transformed coordinates gave the apparent space–time relations for moving observers. This was the view expressed in his contribution to the *Lorentzfestschrift* of 1900, repeated in the Saint-Louis lecture of 1904, and further developed in his Sorbonne lectures of 1906/7.[40]

9.3.4 Relativity versus electromagnetism

The chief theories of the electron may now be compared. Abraham's rigid electron was the most perfect incarnation of the electromagnetic world-view, but it implied observable effects of the Earth's motion with respect to the ether, for instance the double refraction that Rayleigh and Brace had sought in vain. Lorentz therefore preferred a contractile electron, with which he could obtain the invariance of all phenomena within experimental reach. His proof involved a generalization of the old technique of corresponding states, and some approximations. Poincaré perfected Lorentz's considerations to make them completely compatible with 'the relativity postulate.' He introduced the modern form of the Lorentz transformation, as well as the idea that all theories should be made Lorentz-invariant in order to satisfy the relativity postulate. He interpreted the Lorentz transformation as providing the *apparent* space and time for observers belonging to a moving reference frame. In his mind, the ether still provided a reference for defining *true* space and time. However, in order to conform to the relativity postulate, the distinction between absolute and apparent could only be conventional; it could not involve any detectable effects.

Despite Lorentz's and Poincaré's efforts, electron theory still suffered from the tension between the electromagnetic world-view and the relativity principle. Abraham was not convinced that the Rayleigh–Brace experiments contradicted his all-electromagnetic electron. The contradiction occurred only if simple intra-atomic oscillators were made responsible for optical dispersion, a view that had failed to account for the observed regularities of atomic spectra. Lorentz himself was not too sure about the superiority of his contractile electron. In his own words, the

[40] Poincaré 1906: 495; 1900a; 1904; [1906–1907]: Ch. 11 ('Dynamique de l'électron').

contraction was 'neither plausible nor inadmissible.' He temporarily gave it up when in 1905 Kaufmann published new, contradictory, data.[41]

Poincaré had faith in the relativity principle and suspicions about Kaufmann's results. However, beyond the great simplification he had introduced in Lorentz's scheme, he longed for a fundamental justification of his assumption that all forces would behave like electromagnetic forces with respect to motion: 'We cannot content ourselves with simply juxtaposing formulas that would agree only by some happy coincidence; the formulas should, so to say, penetrate each other. Our mind will be satisfied only when we believe that we perceive the reason for this agreement, so that we may fancy that we have predicted it.'[42]

The initiator of the electromagnetic world-view, Wilhelm Wien, emphasized the progress brought by electron theory, but agreed that it still was in a provisional form:

> We cannot overestimate the knowledge brought by the electron theory. Not that we should regard it as a closed construct covering [. . .] all physical phenomena. The numerous, significant difficulties that we have encountered rather show that this theory needs to be replaced with a more general one. However, this theory has shown that all physical concepts, including those which we are used to regard as invariable—for example, the concept of mass—may turn out to be variable upon closer analysis, that more generally we must, as our knowledge grows, depart further and further from sensorial appearances and received physical concepts, that the abstraction must be ever more general.

Wien closed his speech with Goethe's familiar verses: *Alles Vergängliche—Ist nur ein Gleichnis.*[43]

9.4 Alternative theories

9.4.1 Cohn's theory

The electrodynamic theories of the first years of the century generally started with Lorentz's theory and modified it to accommodate the electromagnetic world-view or the undetectability of the Earth's motion. There were, however, two interesting exceptions. The most important one was Cohn's electrodynamics of moving bodies, initiated in 1900 and perfected in 1904. In the name of Mach's economy of thought, Cohn avoided microphysical speculation and based his presentation of Maxwell's theory on phenomenologically defined concepts. This attitude eliminated the ether, especially the concept of ether velocity, from optics and electrodynamics. It also had Maxwellian undertones: the view of electric conduction as a decay of lines of force, and the strategy of modifying the macroscopic field equations to meet experimental challenges.[44]

[41] Abraham 1905: 389; Lorentz 1904a: 197; Kaufmann 1905, 1906; Lorentz to Poincaré (reaction to Kaufmann), 8 March 1906, in Miller 1980: 83–4.

[42] Poincaré 1906: 572 (on Kaufmann); 497 (quote).

[43] Wien 1905: 24.

[44] Cohn 1900b, 1902, 1904. Cf. Hirosige 1966: 31–7; Miller 1981a: 181–182; Darrigol 1995b.

Following this strategy, Hertz's electrodynamics of moving bodies could be made compatible with Fizeau's experiment simply by replacing Hertz's equations with Lorentz's macroscopic field equations. Whereas Lorentz had derived the latter equations by averaging his microscopic equations over macroscopic volume elements, Cohn regarded them as phenomenological equations, the form of which was dictated by stellar aberration and Fizeau's result. In a second step, Cohn faced the incompatibility of Lorentz's macroscopic equations with the Michelson–Morley experiment of 1887. Here he rejected the Lorentz contraction, for it relied on a molecular mechanism. Instead he proposed a second-order modification of Lorentz's macroscopic field equations that accounted for the Michelson–Morley experiment without disturbing the agreement with Fizeau's result and other first-order experiments.[45]

With respect to the fixed stars, Cohn's circuital equations are (in Hertz' units):

$$\nabla \times \mathbf{E} = -\frac{1}{c}\frac{\mathrm{D}}{\mathrm{D}t}\left(\mu\mathbf{H} + \frac{1}{c}\mathbf{v} \times \mathbf{E}\right),$$

$$\nabla \times \mathbf{H} = \frac{1}{c}\frac{\mathrm{D}}{\mathrm{D}t}\left(\varepsilon\mathbf{E} - \frac{1}{c}\mathbf{v} \times \mathbf{H}\right) + \frac{\mathbf{j}}{c}, \tag{9.12}$$

where **E** and **H** are the forces acting on a unit electric or magnetic pole bound to the moving matter, $\mathrm{D}/\mathrm{D}t$ the convective derivative for fluxes, and **v** the velocity of matter (or zero in a perfect vacuum). These equations differ from Hertz's by a first-order correction to the fluxes $\varepsilon\mathbf{E}$ and $\mu\mathbf{H}$. They differ from Lorentz's by a second-order correction $c^{-2}\mathbf{v} \times (\mathbf{v} \times \mathbf{E})$ to the electric displacement. On Earth the velocity **v** of matter differs little from the velocity **u** of the Earth. Then the transformations $x' = x - ut$ and $t' = t - ux/c^2$ bring back the equations to the form they would have if the Earth did not move; and this is true at any order in u/c (see Appendix 12). This 'correspondence' explains why Cohn's theory accounts for the Michelson–Morley result without the contraction of lengths.[46]

Cohn's elegant theory was consistent with the energy principle, and it explained all contemporary experiments on the electrodynamics and optics of moving bodies. However, it implied a violation of the reaction principle, and did not entirely exclude effects of the Earth's motion: it just made them inaccessible to observation. This did not bother Cohn any more than the average electron theorist. Cohn rejected reductionist attitudes and therefore did not expect mechanical principles to apply to electrodynamics. In his opinion an absolute velocity could be defined in electrodynamics even in the absence of the ether, because the fixed stars offered a natural reference frame.

In 1904 Cohn became aware of Lorentz's Dutch memoir on electron theory. He found that the macroscopic equations on Earth were the same in both theories, up to a dilation–contraction of space–time coordinates and fields (see Appendix 12). In

[45] Cohn 1900b, 1902. [46] Cohn 1902, 1904.

other words, Lorentz's theory could be made to share Cohn's system of equations, but with a different interpretation. In Cohn's interpretation, the space–time coordinates entering the equations are the true ones in terms of which the evolution of the system should be described. In Lorentz's interpretation, Cohn's coordinates differ from the true coordinates by a dilation $x' = \gamma x$ and a contraction $t' = \gamma^{-1}t$, and they pertain to the evolution of a fictitious system obtained by imagining the original system brought to absolute rest. This is so because Lorentz assumed a longitudinal contraction of lengths and (implicitly) a slowing down of processes in a moving system. Cohn further noted that the latter effects implied that in Lorentz's theory the space and time measured with clocks and rods bound to the Earth was given by Cohn's coordinates and therefore differed from Lorentz's true space and time. He commented:[47]

> Lorentz's conception requires that we distinguish between measured length and time, and true length and time. However, it fails to provide the experimental means to solve the problem—even by assuming ideal measuring instruments.

Cohn further examined the operational significance of the local time, which is $t - ux/c^2$ in terms of Cohn's space–time coordinates, and $\gamma^{-1}t - \gamma ux/c^2$ in terms of Lorentz's space–time coordinates. In terms of the local time, the propagation of light on Earth becomes isotropic. 'In optics,' Cohn went on, 'we define identical moments of time by assuming a spherical propagation in any isotropic medium that is relatively [with respect to the Earth] at rest.' Therefore the local time is the time given by optically synchronized clocks, as Poincaré had earlied noted. Cohn used the remark to ease the proof that optical phenomena on Earth did not depend on the Earth's motion. Yet he did not mean that the time t entering his equations had no physical significance. In his conception, the laws of mechanics could be strictly valid for the general time t, so that this time would be that given by mechanical synchronization.[48]

A few months earlier, Wien had proposed the following experiment to detect the Earth's motion with respect to the ether. Two toothed wheels rotate at the same speed with the same phase around the same axis, which is taken to be parallel to the velocity of the Earth. Light is sent with the same initial intensity in the parallel and antiparallel directions between the teeth of the two wheels, and the final intensities of the beams are compared (Fig. 9.1). If the ether is stationary, Wien reasoned, the traveling time of light between the two wheels depends on the direction of propagation, so that the final intensities of the two beams should be different. On the basis of his analysis of the local time, Cohn remarked that the result of the experience depended on the procedure used for synchronizing the rotations of the two wheels. Optical synchronization had to produce a negative result, for it assumed isotropic propagation. In contrast, a mechanical procedure could give a positive result according to Cohn. For a supporter of the electromagnetic world-view, or for a believer in the

[47] Cohn 1904: 1299–1300. [48] Cohn 1904: 1408.

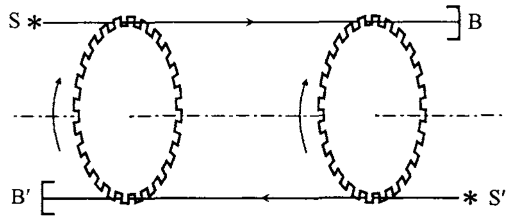

FIG. 9.1. Wien's projected device for detecting the motion of the Earth in the ether. S and S′ are two sources of light of equal intensity, B and B′ two bolometers.

relativity principle, the second procedure could only agree with the first, and the experiment could only yield a negative result. Ironically, Wien, the initiator of the electromagnetic worldview, overlooked this point.[49]

Continental experts, including Lorentz, Wien, Planck, Abraham, and Lorentz, took Cohn's theory seriously, although they perceived some difficulties. Lorentz made the most disturbing criticism: Cohn's theory implied an implausible discontinuity between the properties of very dilute matter and those of vacuum. For example, the Michelson–Morley experiment had to give a negative result in air, no matter how low the pressure was, and yet a positive result in a perfect vacuum. Cohn replied that his theory was phenomenological and therefore did not apply to the case of extreme dilutions, for which the atomic structure of matter was relevant. In 1905 Richard Gans, following a suggestion by Planck, tried to apply Cohn's equations at the electronic scale. At that date, the leaders of the field still respected Cohn's theory. Lorentz and Abraham both gave detailed accounts of it in their contemporary reviews of the electrodynamics of moving bodies.[50]

9.4.2 Bucherer's theory

Another challenge to mainstream electron theory came from Alfred Bucherer of Bonn. In 1902 this physicist resurrected an old idea of Fizeau's for detecting the Earth's motion through the ether. The intensity of the light emitted by a terrestrial source was compared for two absorbers placed at equal distance from the source but in opposite directions. Intuitively, the intensity should be smaller when the absorber is placed in the direction of the Earth's motion. After exchanging a few letters, however, Bucherer and Lorentz convinced themselves that Lorentz's theory gave no effect at all. Paul Nordmeyer, who performed the experiment under Bucherer's direction, confirmed the absence of effect.[51]

[49] Wien 1904a; Cohn 1904: 1408–9.

[50] Lorentz 1904b: 274–5; Cohn 1904: 1302; Abraham 1905: 389–91.

[51] Bucherer 1903; Bucherer to Lorentz (15 February, 6 August, and 8 December 1902), AHQP; Nordmeyer 1903. Bucherer had an English mother, had been partly educated in the USA, and was a friend of G. F. C. Searle: cf. Goldberg 1970c.

Bucherer interpreted this result as one more hint that 'electric and magnetic phenomena, as well as radiation phenomena, can only be influenced when matter moves with respect to matter.' He went on:

> One who would consistently adopt this point of view should renounce the ether-based picture of a temporal propagation of electromagnetic disturbances. But does this renouncement weigh much against the fact that the hypothesis of an ether at rest contradicts both the experiment—I mean Michelson–Morley's—and an important principle of mechanics, the conservation of the center of mass? With the principle: There are only actions from matter to matter, one would return to matter the properties artificially lent to the ether, and thus move from a dualistic to a monistic conception of nature.

The ether was just a 'scaffolding' that had helped when constructing electromagnetic theory and unifying physics. The time was ripe 'to bring down the scaffolding and to show the greatness and beauty of the monument.'[52]

Bucherer proposed to formally maintain Lorentz's equations but to reinterpret the velocities entering the solutions as relative velocities from matter to matter. The same idea is found in a different form in his well-received booklet on electron theory of 1904: Lorentz's equations had to be valid for any system in uniform translatory motion. Bucherer claimed that he could thus reproduce all known laws of the optics and electrodynamics of moving bodies, without Lorentz's 'complicated and very artificial assumptions about changes in the dimensions of bodies.' However, he did not explain how he could use the same Lorentz equations in every inertial system and at the same time maintain the usual Galilean transformation of space–time coordinates.[53]

By that time Bucherer knew about Lorentz's Dutch memoir of 1904, and he also tried, more conservatively, to improve the contractile electron model. He thought the Lorentz contraction was very implausible, for it required artificial cohesive forces and implied an infinite electric density for velocities close to light. As a substitute he proposed a constant-volume electron. The equilibrium shape of a moving electron was still ellipsoidal, and the rate between the major and minor axes was the same as for Lorentz's electron. Bucherer believed that he could thus retrieve all of Lorentz's predictions for the optics of moving bodies, although his expressions for the masses of the electron were different.[54]

At that time, in mid-1904, Kaufmann's data were equally compatible with Abraham's, Lorentz's, and Bucherer's predictions. However, the new data of 1905 were only compatible with Abraham's and Bucherer's electrons. Since Abraham's theory contradicted the Rayleigh–Brace result, Bucherer believed that his model was the only one to have survived competition. However, he soon became aware that the constant-volume electron also contradicted the Rayleigh–Brace result: only Lorentz's expressions for the electromagnetic masses of the vibrating ions provided the necessary compensation of the contraction of the dispersing body. At the *Naturforscherversammlung* of March 1906, Bucherer asserted that all past electron

[52] Bucherer 1903: 282; 1904: 131. [53] Bucherer 1903: 282–3; 1904: 131.
[54] Bucherer 1904: 57–9.

dynamics, including Einstein's, had failed; returning to his ideas of 1903, he outlined a new theory based on the principle that 'there are only actions from matter to matter.'[55]

Bucherer maintained the ordinary kinematics and started from the following assumption: 'Whenever we speak of the dynamical interaction of the systems we stipulate that the system acted upon [. . .] experiences the same force as it would in the Maxwellian theory on the assumption *that it were at rest in the aether* and the other system moving relatively to it.' The resulting theory is like an emission theory in which the roles of emitter and receiver would be inverted. For two electric or magnetic poles in uniform relative motion—the only case that Bucherer investigated—the resulting interaction automatically satisfies the principle of relativity and the principle of reaction, whereas Lorentz's theory violates the latter principle. Like Poincaré in 1900, Bucherer perceived an intimate connection between the two principles, though only in the case of uniform relative motion. Consider an electron A moving away from another electron B at a constant velocity **v**. According to the relativity principle the forces acting between the two electrons should be the same in the symmetrical situation, for which the electron A is at rest and the electron B moves with the velocity −**v**. Therefore the action must be equal and opposed to the reaction.[56]

Although Bucherer used the Maxwell–Lorentz equations, he did it in a way that excluded the ether. In his theory the fields depend on the force to be calculated. In the case of an electron and a magnet in relative motion, for example, he used an electric field to calculate the force acting on the electron, and a magnetic field to calculate the force acting on the magnet.[57] As a substitute for the ether, Bucherer proposed the notion of physical 'links' between electrons and even imagined closed links starting and ending at the same electron to explain radiation and self-interactions. In order to allow closed links, space had to be Riemannian, a fashionable speculation at that time. Except for this very vague picture, Bucherer viewed his theory as 'a phenomenological method of calculating electromagnetic effects which should harmonize with all facts of observation, leaving it to further endeavors to find a physical interpretation of this method.'[58]

Bucherer had little luck with his theory, which he formulated awkwardly. The chief editor of *Annalen der Physik*, Max Planck, completely misunderstood the manuscript that Bucherer submitted. Being already engrossed with Einstein's relativity, Planck accused Bucherer of using Maxwell's equations in different reference systems without changing the kinematics. The charge was in fact unjustified, since, as we just saw, Bucherer's fields were only computational aids, not physical entities as in Einstein's theory. After the *Annalen*'s rejection, Bucherer managed to publish his paper in the more tolerant *Physikalische Zeitschrift* and an improved version in the *Philosophical Magazine*. The English herald of Einstein's relativity, Ebenezar Cunningham, repeated Planck's confusion and triggered a public controversy. The

[55] Bucherer 1905; Discussion of Planck 1906: 760. [56] Bucherer 1906, 1907: 414.

[57] Perhaps the argument was reminiscent of that found at the beginning of Einstein 1905b.

[58] Bucherer 1906, 1908a: 316. Cf. Miller 1981a: 267.

fatal blow came from Walther Ritz, who showed in his memoir of 1908 on emission theory that Bucherer's theory contradicted the standard electrodynamics of closed currents. Yet Bucherer did not only draw bitterness from his solitary speculations. In 1908 his experimental attempts to check their consequences led to an apparent confirmation of the Lorentz–Einstein theory, for which he became a celebrity.[59]

To summarize, Lorentz's theory did not go unchallenged in the early years of this century. The highly respected electrodynamicist Emil Cohn avoided the ether, atoms, electrons, and the Lorentz contraction. In his opinion, Lorentz's contraction of lengths and the implicit dilation of times were not only artificial, they led to a distinction between true and measured coordinates of space and time that had no empirical counterpart. As a substitute Cohn proposed macroscopic field equations that accounted for the optics and electrodynamics of moving bodies but still allowed for practically inobservable violations of the relativity principle. Bucherer did not share Cohn's dislike of the atomistic approach. He agreed that the ether should be eliminated, but for different reasons. Whereas Cohn evoked the economy of thought, Bucherer appealed to the relativity principle and to his conviction that the older German conception of electricity was in many respects truer than the Maxwellian or the Lorentzian approaches. In Bucherer's theory, as in Ritz's later emission theory, the fields were only computational aids, and electromagnetic interactions satisfied the relativity principle despite the lack of Galilean invariance of the field equations. A third case of a physicist who came to reject the ether will now be considered.

9.5 Einstein on electrodynamics

9.5.1 The mystery of the electric current

Electricity was a wonder for many a child born in the late nineteenth century. Certainly it was for the young Albert Einstein, whose family owned a small electrotechnical company. Hertz's discovery of electromagnetic waves and the confirmation of Maxwell's field theory impressed him so much, that in 1895—he was then 16—he wrote a little essay on the state of the ether in a magnetic field. He thought of this state as an elastic deformation that should affect the propagation of light. An experimental investigation of this effect would produce information on the structure of the ether and, indirectly, shed light on 'the mysterious nature of the electric current.' Einstein also concluded that the storage of elastic energy in the ether implied a 'passive resistance' to the variations of an electric current.[60]

[59] Planck to Wien, 29 November 1906, AHQP, discussed in Pyenson 1985: 201; Cunningham 1907, 1908; Bucherer 1908a; Ritz 1908: 204; Bucherer 1908b. On the last experiment, cf. Miller 1981a: 345–9. The result was later questioned, and almost 10 years elapsed before a conclusive verification of the Lorentz–Einstein mass formula: cf. Stachel *et al.*, *ECP*2: 272.

[60] Einstein [1895]. On the Einstein business, cf. Pyenson 1982; Hughes 1993. This section includes extracts from Darrigol 1996, with kind permission of the University of California Press.

Einstein's knowledge of electrodynamics at that time was fragmentary and amateurish: the 'passive resistance' he predicted was already well known as self-induction, and there were many arguments against a static interpretation of the magnetic field. However, his essay reflects two outstanding features of German electrodynamics in the mid-1890s: the belief in the existence of the electromagnetic ether, and the lack of a physical picture for the electric current.[61]

In 1896 Einstein entered the Zürich *Polytechnikum*. There he learned standard continental electrodynamics from Heinrich Weber. In the spring of 1898 he studied Maxwell's theory from Drude's *Physik des Aethers*.[62] If by that date he still fancied a mechanistic conception of the ether, the book would have been a good cure. Even before he read Mach, Einstein could learn from Drude the principle of the economy of thought and the critical attitude toward mechanism. Drude's phenomenology excluded any picture of ether processes, and suggested redefining the ether as space endowed with special physical properties. His book omitted Hertz's electrodynamics of moving bodies and avoided the concept of ether velocity. His treatment of induction in moving conductors was based on Faraday's empirical rule of the intersected lines of force, and required no velocity but that of moving matter. Regarding the nature of electric current, Drude shared Hertz's agnostic attitude.

In the summer of 1899 Einstein studied Hertz's *Untersuchungen*, which contain his experimental papers on electric oscillations, his theoretical papers on Maxwell's theory, and a historical introduction with a comparison between the various conceptions of electricity. Thereupon he wrote Mileva Marić:[63]

> I am more and more convinced that the electrodynamics of moving bodies, as it is presented today, does not agree with the truth, and that it should be possible to present it in a simpler way. The introduction of the name 'ether' into the electric theories has led to the notion of a medium of whose motion one could speak of without being able, I believe, to associate a physical meaning to this statement. I believe that electric forces can be directly defined only for empty space, [which is] also emphasized by Hertz. Further, electric currents will have to be regarded not as 'the vanishing of electric polarization in time' but as motion of true electric masses, whose physical reality seems to result from the electrochemical equivalents [. . .]. Electrodynamics would then be the science of the motions in empty space of moving electricities and magnetisms.

The first sentence of this very dense quote reflects Hertz's own opinion on the matter. Hertz mentioned that his electrodynamics of moving bodies contradicted certain results of the optics of moving bodies (although he did not say which ones), as well as the common-sense requirement that no force should act on the ether in vacuum. Einstein's then writes about 'den Namen "Aether",' probably alluding to Hertz's statement that electric charge and current were only 'Namen' that eased communication between physicists but did not add anything to the foundation of

[61] The famous reminiscence about the pursuit of a ray of light (Einstein 1949: 52–3) belongs to the same period. Historians should renounce any interpretation of this thought experiment that contradicts the evidence just given that Einstein at that time believed in the concrete existence of the ether.

[62] Drude 1894. Cf. Einstein to Marić, April 1898, *ECP*1: 213.

[63] Einstein to Marić, August 1899, *ECP*1: 225–7.

the theory. However, Hertz did not count the ether as a name. The ether, the notion of electric and magnetic forces representing the state of the ether, and the differential equations ruling these forces were basic notions of his theory. Apparently, Einstein's intention was to rid the ether of its most crudely materialistic attribute, velocity. Before him, Heaviside had expressed a similar reticence toward ether motion; Drude and Heaviside's German admirer Föppl had simply avoided the suspicious concept.

The denial of ether motion constitutes a first blow against the Maxwellian notion of a single medium that has to move wherever there is moving matter. Einstein goes on with a second, operationalistic argument against the Maxwellian view: a direct, operational definition of the electric force **E** is only possible in a vacuum. This is an allusion to Hertz's procedure based on measuring the mechanical force acting on a test charge. Within matter, Hertz followed William Thomson's old presciption based on carving a thin cylindrical cavity before introducing the test charge. Consequently, what is directly measured is always a force in vacuum. Only the theory can tell that the carving does not perturb the values of **E**. Hertz saw no difficulty here. He suspected, however, that the electrodynamics of the future would disentangle ether and matter: 'The correct theory might [. . .] be a theory that, at every point, distinguishes the states of the ether from those of matter.'[64]

Einstein's letter continues with a rejection of the Maxwellian concept of the electric current as the decay of electric polarization. Before studying Hertz, Einstein had been reading the third volume of Helmholtz's scientific papers, which included the Faraday lecture of 1881 and the electromagnetic dispersion theory of 1893. In the former text, Helhmoltz concluded that the laws of electrolysis implied the existence of 'atoms of electricity,' and in the latter he made elastically bound ions responsible of the dispersion phenomenon. Einstein somehow grafted Helmholtz's atomistic vision of electricity on Drude's immobile ether, and thus reached the conception of electrodynamics as 'the science of the motion of electric and magnetic masses in empty space,' a view similar to Lorentz's and Wiechert's.[65]

When Einstein wrote these words, he might well have heard of Lorentz's theory, which was being widely discussed. In any case, his considerations reflected contemporary interest in ionic theories as well as the natural character of Lorentz's and Wiechert's assumptions. More surprisingly, Einstein appears to have been aware of the Maxwellian concept of electric current. This was not to be found in Drude's and Hertz's books, and even less in Heinrich Weber's course at the ETH, which strictly adhered to the substantialistic view of electricity.[66] Perhaps Einstein had read Maxwell's *Treatise* or Föppl's rendering of Maxwell. Or perhaps Maxwellian ideas had been floating in the Zürich air.

In his youth Einstein always accompanied his theoretical speculations with suggestions for experimental tests. His proposition for a new electrodynamics ended

[64] Hertz 1892a: 211–12, 285. Einstein's criticism is akin to Boltzmann 1893: 13–14.
[65] Helmholtz 1881a, 1893a. On Einstein having read Helmholtz, cf. Einstein to Marić [August 1899], *ECP*1: 226.
[66] Cf. Einstein's lecture notes, in *ECP*1: 148–210.

with the words: 'Which of the two conceptions must be chosen will have to be decided by the radiation experiments.' Einstein did not specify the experiments he had in mind. Perhaps it was the comparison of the intensities of the light emitted from the same source in two opposite directions, which he later remembered to have planned during his student days. As was mentioned, Fizeau already had this idea in 1854, and Nordmeyer realized it in 1903 under Bucherer's direction. Einstein probably reasoned that the stationary ether implied different path lengths and therefore different intensities, whereas the fully dragged ether of Maxwell and Hertz gave a null result.[67]

In September 1899 Einstein thought of another crucial experiment: 'In Aarau a good idea occurred to me for investigating which effect the relative motion of bodies with respect to the luminiferous ether has on the velocity of propagation of light in transparent bodies. Also, I have hit upon a theory of this matter, which seems to me to be highly probable.' Apparently, Einstein had in mind an experiment similar to Fizeau's measurement of the dragging of light waves by running water. His theory may have been akin to Lorentz's or Reiff's derivation of the Fresnel coefficient. What is certain is that Einstein expected effects of the motion of matter with respect to the ether, and that he regarded them as a confirmation of his new conception of electrodynamics. It is also clear that Einstein was completely uninformed about the current results of the optics of moving bodies.[68]

Einstein wrote out his ideas and submitted them to Heinrich Weber. The Professor knew about Lorentz's theory and recent discussions of ether motion, and therefore reacted in a '*stiefmütterlich*' manner. He told Einstein to read Wien's 1898 survey of the problem of ether motion, which included a short description of Lorentz's viewpoint, and a fairly complete list of relevant experiments, including Fizeau's and Michelson and Morley's. Einstein found this 'very interesting,' and wrote Wien about his own ideas (the letter is lost). We may surmise that Fizeau's result especially pleased him: it provided the desired refutation of Hertz's electrodynamics of moving bodies, and confirmed the picture of ions interacting through a stationary ether. However, as Wien himself emphasized, there was a major difficulty: the stationary ether implied a positive result for the Michelson–Morley experiment, unless a special contraction of lengths was assumed. Einstein's first reaction to this difficulty seems to have been one of skepticism. One year after reading Wien, he was still trying to imagine a simple method to seek the motion of the ether with respect to matter.[69]

To summarize, in the early phase of his reflections on electrodynamics, Einstein was highly sensitive to the historical transitions of late nineteenth century electrodynamics. He fully appreciated the impact of Hertz's discovery on German

[67] Einstein to Marić [August 1899], *ECP*1: 227. The reminiscence about an experiment of the Nordmeyer type is in Einstein [1922]: 45–7.

[68] Einstein to Marić, 10 September 1899, *ECP*1: 229–30.

[69] Einstein to Marić, 28? September 1899, *ECP*1: 233–35; Einstein to Grossmann, 6? September 1901, *ECP*1: 315–16: 'For the search of the relative motion of matter with respect to the luminiferous ether, I have thought of a much simpler method that rests on usual interference experiments.'

electrodynamics, and meditated about the remaining difficulties: the enigma of the electric current, and the electrodynamics of moving bodies. He was aware of the contemporary need to integrate the atomistic conception of electricity, and used it to solve the difficulties. Eager to be at the forefront of physics, he focused on the newest, most fashionable theories and tracked the experts' judgments on the remaining difficulties and possible directions of development. His reflections were singularly deep and bold for a beginner. However, until at least 1901, they were not truly original: they belonged to a growing trend of research of which Helmholtz, Lorentz, and Wiechert were the pioneers.

Einstein's reflections involved critical remarks on the grounding of physical concepts. This early epistemological awareness could not derive from his reading of Mach and Hume, which occurred later.[70] The source is found in the previous history of electrodynamics: Hertz's discovery triggered a conceptual revolution, which in turn prompted reflections on the aims and foundations of physical theory. Hertz himself was concerned with eliminating unnecessary ornaments, with providing unambiguous reference for the symbols entering the fundamental equations, and with respecting the general principles of mechanics. He gave epistemological criteria a significant role in the criticism of theories. Drude similarly evoked Mach's principle of the economy of thought. In conformity with these views, Einstein's criticism of ether motion was based on considerations of physical meaning and on operational definiteness. He also sought an experimental refutation of Hertz's electrodynamics of moving bodies, but only to confirm his suspicion that the ether was stationary.

At that time Einstein avoided the mechanistic conception of the ether; he regarded it as a name for that in which light waves propagates. However, his critical attitude did not go so far as to reject the ether completely until at least 1901. He regarded motion of matter with respect to the ether as well defined, and expected observable effects. In this regard the results he found in Wien's memoir of 1898 must have puzzled him: whereas the Fizeau experiment confirmed one of these effects, the Michelson–Morley experiment denied another.

9.5.2 *Emission theory*

At some point Einstein ceased to look for effects of the ether wind, and assumed quite generally their non-existence. In other words, he adopted the principle of relativity. Unfortunately, no known letter or manuscript documents this capital transition. We may look, however, for the motivations of contemporary believers in the relativity principle, and examine whether Einstein might have shared them. We may also examine Einstein's later published arguments, and see whether they could have been made in the early years of the century.

Only two famous electrodynamicists believed in a general validity of the relativ-

[70] Einstein read Mach, presumably the *Mechanics*, in September 1899: cf. Einstein to Marić, 10 September 1899, *ECP1*: 230 and the editorial note 8, *ibid.* He read Hume's *Treatise of human nature* 'shortly before the discovery of the theory of relativity,' probably for the 'Olympia Academy,' which was formed in 1902: cf. Einstein to Schlick, 14 December 1915, quoted in *ECP2*: xxiv, note 37.

ity principle at the beginning of the century. One was Poincaré. His conviction, which dated from 1895, had a triple origin: his skepticism toward the ether, his belief in the general principles of mechanics, and the failure of attempts to detect the Earth's motion in the ether. To put it briefly, he expected that the ether, being only a convention for describing propagation phenomena, should not count as a body in the application of the principle of relative motion to a system of bodies. The failure to detect first- and second-order effects of the Earth's motion in the ether confirmed this intuition. Similarly, the other believer in the relativity principle, Bucherer, viewed the ether as a scaffolding and interpreted the repeated failures to detect the Earth's motion in it as an indication of the general validity of the relativity principle. He added that the complete elimination of the ether would permit a return to the good old conception of electricity and complete Lorentz's retreat from the Maxwellian concept.

Einstein had called the ether 'a name' and had been taught the older concept of electricity. Therefore, he might easily have followed the same line of reasoning as Poincaré and Bucherer. In fact, in all his early presentations of the relativity principle, Einstein evoked the failure of attempts to detect the Earth's motion through the ether. In the relativity paper of 1905 he referred globally to 'the failed attempts to detect any motion of the Earth with respect to the "light medium".' In later accounts of 1907 and 1910 he described Michelson and Morley's result. Einstein also insisted, as Poincaré had done, that the relativity principle was a principle of mechanics that controlled electromagnetic phenomena as well. Possibly, Poincaré's arguments played a role in shaping Einstein's conviction. Poincaré's address at the Paris congress of 1900 was published in *Physikalische Zeitschrift*, and most of it was included in *La science et l'hypothèse* (1902), which Einstein read before 1905. However, it seems likely that Einstein also used an epistemological argument of his own.[71]

As many commentators have noted, Einstein's relativity paper starts not with the general failure to detect the motion of the Earth with respect to the ether, but with a criticism of Maxwell's electrodynamics 'as it is now understood,' that is, Lorentz's electrodynamics. The magneto-induction current, Einstein reminded his reader, depends only on the relative motion of the conductor and the magnet. Yet the theory gives two different descriptions of this phenomenon according to whether it is the magnet or the conductor that moves in the ether. In the first case, an electric field with a certain energy density is responsible for the induced current; in the other case there is no electric field, and the induction current is ascribed to an electromotive force with no corresponding field energy. More generally, when Lorentz's theory is applied to phenomena involving moving bodies, it leads to asymmetries that have no empirical counterpart.[72]

Einstein was by no means the first physicist to note asymmetries in theoretical representations of the induction phenomenon. In 1885 Heaviside noted 'the great difference' between the case of the moving magnet and the case of the moving

[71] Einstein 1905b: 891; 1907a: 412; 1910: 11–13; Poincaré 1901b.

[72] Einstein 1905b: 891. I agree with Paty 1993: 54–5 that Einstein's argument was epistemological rather than aesthetical.

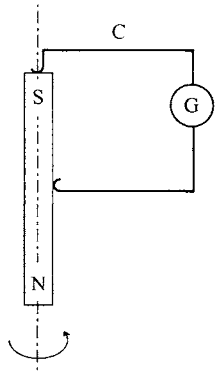

FIG. 9.2. Faraday's device for unipolar induction. The cylinder magnet rotates around its axis; the wire C and the galvanometer G are fixed.

conductor. In 1894 Föppl made the same remark, and emphasized that the dependence of induction on relative motion only was an empirical result, not to be derived from pre-existing theoretical representations. In 1885 a telegraphic engineer, Tolver Preston, also noted that Faraday's description of unipolar induction (Fig. 9.2) differed according to whether it was the magnet or the conductor that was rotating. Another blatant case of asymmetry appeared in the reports of the Düsseldorf meeting of 1898, which Einstein read. Here Wien noted that Röntgen had failed to detect the force between a magnet and an electric charge (both at rest in the laboratory) that the motion of the Earth seemed to imply. Lorentz immediately answered that his theory implied a compensatory electrification of the magnet. There remained a patent theoretical asymmetry between the case when the magnet and the charge are at rest and the case when both are moving together.[73]

Among these authors, Preston was the only one who wished to eliminate the theoretical asymmetry. Heaviside and Föppl believed that the source of the asymmetry, the ether or absolute space, really existed, and that other electrodynamic phenomena, or finer details of the induction phenomenon, would depend on absolute motion. Wien's and Lorentz's only concern was that the asymmetries should have no undesired experimental consequences. The pertinent question is not how Einstein became aware of asymmetries without empirical counterpart, but what made them so intolerable to him.

At least three factors may be invoked. First, Lorentz's theory, with its stationary ether, greatly enhanced the asymmetries: in Maxwell's theory, there was no distinction between electromotive force and electric field, and all electromotive forces of induction were energy-carrying electric fields. Second, at the turn of the century the

[73] Heaviside 1885–1887: 446; Föppl 1894: 311; Preston 1885: 134; Wien 1898: 55 (*VDNA*); Lorentz 1898b: 59. Cf. Darrigol 1993b: 311 (Heaviside), 329–30 (Föppl), 299–300 (Preston), 333–5 (anticipations of Lorentz's reasoning by FitzGerald and Budde).

effects of absolute motion that would have justified the asymmetry were still wanting, despite the experimental efforts to detect them; and in many cases Lorentz's theory implied the absence of such effects. Third, and most important, Einstein required that the correspondence between theoretical representation and phenomena should be unambiguous. Here we stand on purely epistemological grounds. Einstein later referred to his reading of Mach and Hume. Mach's economy of thought did proscribe theoretical distinctions without empirical counterparts. However, Hertz could also be invoked. In the foreword to his *Untersuchungen* Hertz wrote: 'In this presentation I have tried to reduce to a minimum the number of representations that we arbitrarily introduce in the phenomena and to allow only for such elements that cannot be removed or changed without simultaneously altering possible experiments.' Einstein's early search for an operational definition of field quantities and his non-ambiguity requirement may both be seen as manifestations of this concern.[74]

In sum, Einstein's belief in the relativity principle had multiple sources, including the failure of attempts to detect the motion of the Earth with respect to the ether, his faith in the general principles of mechanics, and the epistemological awareness he could gain from his reading of Hertz and Mach, among others. What the lack of contemporary documentation leaves open is the relative importance of these sources and when the belief developed. According to his Kyoto lecture of 1922, Einstein became convinced of the generality of the relativity principle soon after knowing about the Michelson–Morley result, that is, soon after September 1899. Our certainty is only that Einstein was working on a 'capital memoir' on the electrodynamics of moving bodies at the end of 1901. This may have been his first attempt at a theory based on the relativity principle.[75]

In a theory assuming the stationary ether, Fizeau's experiment is the paradigm, and the Michelson–Morley experiment the paradox. In a theory assuming the relativity principle, the situation is reversed. According to this principle, the laws for the propagation of light in a transparent body, including the value of the velocity of light with respect to the body, should not depend on the uniform motion of the body. Therefore, light should be completely dragged by the moving transparent body, contrary to Fizeau's result. Moreover, if the laws of propagation of light are the same in two inertial systems, the velocity of light should be the same in each system, which contradicts the law of the addition of velocities.

As we saw, Poincaré reasoned in this way at the turn of the century, and so did Einstein according to the Kyoto lecture. Both men were aware of the difficulty of imagining a theory that would accommodate both the relativity principle and Fizeau's result, and both denied that Lorentz's theory, which explained Fizeau's

[74] Einstein 1949: 52: 'The critical reasoning required for the discovery of this central point [that simultaneity is a relative notion] was decisively promoted, in my case, especially by the reading of David Hume's and Ernst Mach's philosophical writings'; Hertz 1892a: 30.

[75] Einstein [1922]; Einstein to Marić, 17 December 1901, *ECP*1: 325–6. Our knowledge of the contents of the Kyoto lecture is indirect (through Ishiwara's notes) and parts of it have been judged dubious on other grounds (see Holton 1988: 479–80); however, several of the points questioned by historians have now been confirmed by the Einstein–Marić correspondence.

result, properly satisfied the relativity principle. However, they sought different cures. Whereas Poincaré believed that an adequate modification of Lorentz's theory would agree with the relativity principle, Einstein immediately sought to eliminate the ether. In his opinion, any ether theory complying with the relativity principle would necessarily lead to the disagreeable asymmetries found in Lorentz's theory of electromagnetic induction.

Einstein thus found himself compelled to imagine a theory of electromagnetic propagation that would respect the relativity principle without disturbing the confirmed predictions of Lorentz's theory. Like Bucherer he thought of modifying the expression of the retarded interactions between two particles so that they would depend only on the relative motion of the particles. He did this in the manner of Ritz's later emission theory, that is, by making the propagation velocity depend on the velocity of the source at the emission time. Then the paradoxes of the stationary ether disappeared. The usual composition of velocities was saved, since the velocity of light was defined only with respect to its source; and Fizeau's result was no longer paradoxical, since the velocity of light in a transparent body depended not only on the constitution of the body but also on the velocity of the luminous source with respect to the body. Although no written trace of this attempt has survived, there is no reason to doubt Einstein's later mentions of it.[76]

9.5.3 The new kinematics

Einstein later remembered to have encountered two difficulties in his emission theory. He could not find a system of differential equations on which to base the theory; and he discovered that the light emitted by an accelerated source would be very 'mixed up' owing to the dependence of phase on source velocity. In some cases the light could even 'back up on itself.'[77]

This failure in part explains that six years elapsed between Einstein's first projected electrodynamics of moving bodies and his first publication on the subject. However, Einstein had many other interests in this period. While reading the 'magnificent' Boltzmann in 1900, he became fascinated with the kinetic theory of heat, 'a further step toward the dynamical explanation of physical phenomena.' He especially enjoyed considerations that combined kinetic theory, electrons, and radiation, which explains his enthusiastic reaction to Drude's electron theory of metals in 1901: 'Der Drude ist ein genialer Kerl.' Einstein's first publications dealt with molecular forces and with the foundations of statistical thermodynamics.[78]

In early 1903 Einstein planned a 'comprehensive study of electron theory.' Very likely, Wien's electromagnetic world-view and Abraham's electron theory had

[76] Einstein to Ehrenfest, 25 April 1912, quoted in *ECP*2: 263: 'Ritz's conception [the emission theory], which before the theory of relativity was also mine'; Other letters referred to in *ECP*2: 264, note 64; Probable allusion in Einstein [1922] to an attempt at making Lorentz's equations valid in a moving reference system (attached to the source?). Cf. Schankland 1963; Stachel 1982.

[77] Reported in Schankland 1963. See also Einstein to Ehrenfest, 20 June 1912, *ECP*5: #409.

[78] Einstein to Marić, 4 April 1901, *ECP*1: 284. Cf. Abiko 1991 and Renn 1993.

attracted his attention. Despite his taste for unified, monistic theories, Einstein could not have much sympathy for the new electron theories, for at least three reasons. First, neither their form nor their empirical predictions complied with the relativity principle. Second, their mathematical sophistication was foreign to his style of physics.[79] Third: somewhen before 1900 and 1905, as a result of his interest in thermo-statistical problems, Einstein became convinced that current electrodynamic laws broke down for the interactions between matter and high-frequency radiation. This made investigations of the structure of the electron look premature, if not totally meaningless.[80]

Yet it seems likely that Einstein kept up with literature on electron theory as well as with anything he could find on the electrodynamics of moving bodies. He would then have known before the end of 1904 about Lorentz's Dutch memoir including the Lorentz transformation, even though he almost certainly did not read the memoir. Several mentions of Lorentz's theory and the Lorentz transformation occur in contemporary literature. For example, a paper by Wien on the electrodynamics of moving bodies published in March 1904 in *Annalen der Physik* involved the Lorentz transformation of coordinates, albeit in a recondite form. The issue of *Physikalische Zeitschrift* for July 1904 contained a paper by the same author on electron theory, with the complete expression of the Lorentz transformation for fields and coordinates and a reference to Lorentz's Dutch memoir. In his booklet on electron theory, published in the same year, Bucherer summarized Lorentz's new results and also gave the complete Lorentz transformation. If Einstein actually read one of these sources, he would have come to suspect, as Poincaré did, that complete relativity could be reached within the framework of the Maxwell–Lorentz equations. He would also have learned the formal invariance properties of these equations and thus be left with the task of improving the connection between this invariance and the relativity principle.[81]

Einstein did not know Poincaré's latest work on this problem, since its first publication, in summary form, occurred in June 1905. He had, however, read and admired *Science and Hypothesis*, in which Poincaré criticized Lorentz's accumulation of hypotheses, gave reasons to believe in a general validity of the relativity principle, and declared:[82]

There is no absolute time. To say two durations are equal is an assertion which has by itself no meaning and which can acquire one only by convention. Not only have we no direct intuition of the equality of two durations, but we have not even direct intuition of the simultaneity of two events occurring in different places: this I have explained in an article entitled *La mesure du temps*.

[79] Cf. McCormmach 1976; Pyenson 1980, 1985. [80] Cf. Klein 1967; McCormmach 1970a.

[81] Wien 1904b, 1904c; Bucherer 1904: 229. Abraham 1904 contained an account of Lorentz's contractile electron, but without the Lorentz transformation. His book of 1905 gave the Lorentz transformation, but came out too late (in the spring) to be used by Einstein. It may be recalled that the Lorentz transformation already appeared in Lorentz 1899 and in Larmor 1900a.

[82] Poincaré 1902: 111. That Einstein read this text before 1905 is attested in Solovine 1956: vii–viii: 'This book profoundly impressed us and kept us breathless for weeks on end.' Cf. also Einstein to Besso, 6 March 1952, in Speziali 1972: 464.

Most important, Einstein almost certainly read Poincaré's memoir for the Lorentz jubilee of 1900. He knew it very well in 1906, since he used it for a new derivation of the mass–energy equivalence. Very likely, he studied this text before 1905, because the electron theorists frequently referred to it as the source of the concept of electromagnetic momentum. Here Poincaré gave the definition of simultaneity which Einstein used in 1905, and revealed that to first order Lorentz's local time was the time measured by moving clocks synchronized according to this definition.[83]

This last insight does not seem to have caught the attention of other theorists for several years. However, in 1904 Cohn asserted that Lorentz's local time was the time for which the propagation of light was isotropic, and in early 1905 Abraham showed that optically synchronized clocks gave Lorentz's local time at any order, if only the Lorentz contraction was assumed.[84] Einstein could not have read Abraham's book before he wrote the relativity paper. But he may well have studied Cohn's papers and appreciated his elimination of the ether as well as the remark that Lorentz's theory introduced an empirically meaningless distinction between true and apparent coordinates.[85]

Like Cohn and Abraham, Einstein very probably became aware of Poincaré's interpretation of Lorentz's local time and corresponding states. This interpretation allowed a direct connection between the invariance properties of the Maxwell–Lorentz equations and the relativity principle, since the transformed coordinates and fields were now those measured under natural conventions. At that point, Einstein may or may not have been aware that the result, initially proved by Poincaré only to first order, was an exact one, if the exact form of the Lorentz transformation was used. In any case, he could not have been satisfied with the current state of affairs. His conception of the relativity principle, as illustrated by his discussion of electromagnetic induction, required not only an invariance of observable phenomena, but also an invariance of the theoretical representation of these phenomena. On the contrary, Poincaré maintained the ether and distinguished between true and apparent states, the true ones referring to the ether, and the apparent ones to moving observers.

Einstein's next step was purely epistemological: he decided that the space and time measured in any inertial system were all on the same footing, that the ether could no longer serve as a privileged system. From the idea of a medium of propagation he kept only the requirement that in a given inertial system the velocity of light was a constant independent of its source. This principle, together with the relativity principle, implies that the velocity of light is a constant, and the same

[83] Einstein 1906: 627: 'The simple formal considerations that must be developed to prove this assertion [that the conservation of the motion of the center of mass for a system involving electromagnetic radiation implies the relation between mass and energy content] are already, for the main part, contained in a work by Poincaré.' Cf. Darrigol 1995a.

[84] Cohn 1904: 1299–300; Abraham 1905: 366–79. Neither Abraham nor Cohn referred to Poincaré. In 1908 Abraham rejected the dilation of time and rather had $t' = t - ux/c^2$ for the apparent time, and $c' = c\,(1 - u^2/c^2)^{1/2}$ for the velocity of light in the moving system (Abraham 1908: 368–9).

[85] Einstein 1907a: 413n refers to 'E. Cohn's penetrating works, of which, however, I have made no use here.'

constant, in any other inertial system. This result replaces Poincaré's convention that moving observers do so as if the velocity of light were a constant. Einstein thus founded a new doctrine of space and time, in which velocities no longer add and time depends on the reference system. Naturally, he meant this new kinematics to frame all future theories.[86]

We may now follow the order of the relativity paper of 1905. Einstein managed to extend Poincaré's synchronization reasoning so that it yielded the Lorentz transformation for time and space. He provided the physical interpretation of the transformation, as well as the law for the composition of velocities. Then he verified that the Maxwell–Lorentz equations were invariant under such transformations. In these two steps, a previous knowledge of the transformation would have helped, but it was not necessary. Next, Einstein derived the Doppler effect, stellar aberration, and the transformation law for radiation energy and pressure. Finally, he derived the equations of a new mechanics that complied with the new kinematics and agreed with the older mechanics for slow motions.[87]

Compared to previous electron theories, the most evident characteristic of Einstein's theory is that it is based on two general principles that do not refer at all to the structure of matter and radiation. The corresponding kinematics efficiently constrain theory construction, as Einstein demonstrated by deriving the dependence of the electron masses on velocity without any assumption about the structure of the electron. This was an important achievement for one who believed that the properties of high-frequency radiation contradicted the known theories of the electron. Perhaps Einstein had deliberately sought a 'theory of principles' independent of the questionable details of electromagnetic interactions, as suggested in his autobiography.[88] At any rate, extirpating the ether from Poincaré's notion of apparent space and time in itself led to a new kinematics based on two principles.

9.5.4 The inertia of energy

In 1900 Poincaré had shown that in Lorentz's theory the application of energy conservation to the emission of radiation by two different observers, one attached to the source, the other moving with the velocity u in the direction of emission, led to an apparent violation of the relativity principle. If the radiation J was emitted in a single direction, the two observers' estimates of the recoil impulse differed by Ju/c^2 (the Liénard force). Poincaré later seems to have forgotten this paradox. He contented himself with showing that at the electronic level Lorentz's theory could be made fully compatible with the relativity principle.[89]

[86] Einstein 1905b: 892–903. According to Einstein [1922], he got the idea of changing the kinematics during a conversation with his friend Michele Besso, a few weeks before he completed the relativity paper.

[87] Einstein 1905b. For some of the most insighful commentaries on this work, see Holton 1973a; Miller 1981a; Pais 1982; Paty 1993; Renn 1993; Stachel 1995.

[88] Einstein 1949: 52–3.

[89] Poincaré 1900a. See above, pp. 357–8. For the sake of concision, I have slightly altered the expression of the paradox.

In 1905 Einstein encountered a similar paradox while discussing the dynamic consequences of his own relativity theory. Like Poincaré, he considered a light-emission process from two different point of views. He preferred a symmetrical process, for which there is no recoil of the source: the same quantity of light $J/2$ is emitted in two opposite directions. For an observer moving at the velocity u on the emission line, the emitted energy reckoned from the Lorentz-transformed electromagnetic fields is

$$J' = \gamma\left(1+\frac{u}{c}\right)\frac{J}{2} + \gamma\left(1-\frac{u}{c}\right)\frac{J}{2} \sim J + \frac{1}{2}\frac{J}{c^2}u^2. \tag{9.13}$$

Einstein saw only one way to avoid a violation of the relativity principle: to admit that during the emission process the mass of the emitter has decreased by J/c^2. Then for the moving observer the kinetic energy of the emitter decreases by $(1/2)(J/c^2)u^2$, which explains the difference in the emitted energy.[90]

The mass of a body, Einstein surmised, depended quite generally on its energy content. Hence, to the enormous energies involved in radioactive transformations should correspond appreciable mass defects: 'The thing is pleasant to consider,' Einstein commented to a friend, 'but isn't God laughing at it and is he pulling me by the nose?'[91]

We may note that Einstein's assumption works equally well for Poincaré's unidirectional process: there the decrease of the emitter's mass implies, for the moving observer, a variation $(J/c^2)u$ of the momentum, which exactly balances the effect of the Liénard force. The momentum balance can also be discussed in Einstein's case of double emission. This is how Langevin reached the inertia of energy in 1906, independently of Einstein. For the moving observer, the total momentum of the emitted radiation is, to first order,

$$\left(1-\frac{u}{c}\right)\frac{J}{2c} - \left(1+\frac{u}{c}\right)\frac{J}{2c} = -\frac{Ju}{c^2}. \tag{9.14}$$

The conservation of momentum implies that the momentum of the source changes by the opposite amount, uJ/c^2. Since the velocity $(-u)$ of the source does not change, its mass must decrease by J/c^2.[92]

In 1906 Einstein further showed that the inertia of energy saved the theorem of the center of mass. For the first time he explicitly refered to Poincaré's paper of 1900, from which he borrowed 'the relevant formal considerations.' Poincaré had shown that the uniform motion of the center of mass of an electrodynamic system could only be obtained by including a contribution of a fictitious fluid, whose density was the energy of the electromagnetic field divided by c^2. In his mind this result

[90] Einstein 1905c. [91] Einstein to Habicht (undated, June–September 1905), in *ECP5*: #28.

[92] P. Langevin 1913: 418–19. *Ibid.*: 414, Langevin asserts that his considerations were independent of Einstein's and were included in his course at the Collège de France of 1906. See also E. Bauer's testimony, reported in André Langevin 1971: 58–9.

only confirmed the violation of the theorem for the true center of mass. Einstein read the same formulas as implying a variation of the mass of the field sources during the emission or absorption of radiation. He also proposed the following thought experiment, in the spirit of Poincaré's argument with the two interacting bodies connected by a rigid bar.[93]

A source of radiation and an absorber face each other and belong to the same solid. They undergo the following cyclic process. The source emits a radiation pulse with energy J in the direction of the absorber, which implies a recoil momentum J/c for the solid. When the pulse reaches the absorber, the solid returns to rest. A massless carrier then brings back the energy J to the emitter, completing the cycle. During this whole operation, the solid shifts by the approximate amount $-(J/Mc)(L/c)$, where M is the mass of the solid, and L the distance between the emitter and the absorber. This uncompensated displacement constitutes a kind of perpetual motion. Einstein avoided it by assuming that the return of the energy J to the emitter involved a mass transfer J/c^2. This transfer implies a global shift of the solid by the approximate amount $(J/c^2)L/M$, which compensates the displacement in the first phase of the cycle.[94]

Einstein and Poincaré shared the same concern with general principles and a similar ability to penetrate by thought experiment the consequences of their violation. With that kind of reasoning, Poincaré detected a major paradox of Lorentz's electrodynamics, and Einstein solved it five years later by inventing the inertia of energy. This long delay is not so surprising, considering the singularity of Poincaré's worries, and the radical revision of the concepts of mass and energy that Einstein's proposition brought with.[95]

9.5.5 *The alleged superiority of Einstein's theory*

Whoever studies modern physics can appreciate the central role that special relativity plays in the formulation of fundamental theories. He is also taught to despise the awkwardness of older ether theories and to venerate the wonderful simplicity brought by Einstein's revolutionary concepts of space and time. Accordingly, historians usually tend to regard the superiority of Einstein's approach as obvious even around 1905. Resistance to relativity is imputed to blindness or conservatism. Developments of electrodynamic theory preceding Einstein's intervention are often treated as moderately rewarding efforts in the wrong direction. Connections with or

[93] Poincaré 1900a; Einstein 1906. Cf. Darrigol 1995a: 41–4.

[94] Einstein's argument is slightly defective, because the absorption of radiation normally implies energy dissipation. It remains true that the mass–energy equivalence removes the violation of the theorem of the center of mass in the first part of the cycle: the transfer of luminous energy now involves a transfer of mass so that the center of mass does not move despite the global shift of the body.

[95] There is no direct evidence that Poincaré accepted the mass–energy equivalence after 1905. Nor is there evidence for the contrary. His statement of 1908 (*PO9*: 568) that 'l'énergie n'a pas de masse' occurred in a discussion of the violation of the reaction principle in radiation processes; it only meant that energy did not have a *mechanical* mass. In the following paragraph (*PO9*: 571, 573) Poincaré recalled that according to the new mechanics there would be no mechanical mass at all and that every mass would be of electromagnetic origin.

similarities to Einstein's theory are ignored or played down. To give just one example, most historians of relativity have been unaware that Poincaré's physical interpretation of the local time was first published in 1900, in a volume that was read by every expert in the field.[96]

A different picture of the genesis of relativity theory emerges from the present account. Einstein's reflections on the electrodynamics of moving bodies started from the German Maxwellian viewpoint and went through several steps: the criticism of ether motion, the introduction of charged particles in a stationary ether, the adoption of the relativity principle, a failed attempt at an emission theory, the realization that Lorentz's local time derives from a simple synchronization procedure, and the introduction of a new kinematics. When measured by the extent of their departure from the original Maxwellian viewpoint, all these steps have a comparable magnitude. In his autobiography Einstein himself insisted that the first step, which he ascribed to Lorentz, was a radical one: 'The physicist of the present generation regards the point of view achieved by Lorentz as the only possible one; at that time, however, it was a surprising and audacious step, without which the later development would not have been possible.'[97] Not only this step, but all the others except the last, have antecedents or parallels in contemporary electrodynamic researches, and some of them very likely derived from these antecedents. For the most part Einstein's thinking was not unique; it fitted very well into the stormy developments of electrodynamics at the turn of the century.

By 1906 the electrodynamics of moving bodies had been the object of several competing theories. I will illustrate the positions of the various actors by a fictitious discussion between Cohn, Einstein, Poincaré, Abraham, Bucherer, and Lorentz. The footnotes indicate the extent to which the dialogue is fictional.

Cohn: I have read Dr Einstein's interesting memoir on the electrodynamics of moving bodies. It makes perfect sense to me, and it improves over Prof. Lorentz's recent works. As you probably know, I have myself tried to eliminate the ether, following Mach's dictum that we should never forget the origins of our concepts and lend to them a necessity they do not have. However, changing our concepts of space and time is a very radical step. I do not see that it is a necessary one at the present stage of physics. I believe that my approach is more economical, for it is based on a simple modification of the Maxwell equations and does not require any assumption on the structure of matter, whereas the electrodynamic part of your paper implicitly assumes Prof. Lorentz's reductionist approach. Moreover, my theory is a complete electrodynamics of macroscopic moving bodies, whereas you do not examine the macroscopic consequences of your microscopic assumptions. For example, you have not derived Fizeau's result on the dragging of light waves.[98]

[96] Exceptions are Scribner 1964; Cuvaj 1970a: 77–8; and Stachel *et al.*, *ECP*2: 308n.

[97] Einstein 1949: 34–6.

[98] Cohn's early reaction to Einstein's paper is not known. In a discussion held in Zurich in 1911, Einstein emphasized the difference between Lorentz and Cohn: 'Cohn's theory must be regarded as fundamentally different' (*ECP*3: 445, after downplaying the difference between Lorentz and Minkowski). Einstein regarded Cohn 1913 as 'an excellent presentation' of relativity theory: cf. Miller 1981a: 182.

Einstein: I admit that I have not yet provided a complete electrodynamics of moving bodies in the macroscopic sense. For the sake of clarity and concision, I did not discuss moving dielectrics in my paper. But I trust that this can be done without much difficulty, using Prof. Lorentz's averaging procedure. Regarding Fizeau's result, I can easily transpose Prof. Lorentz's explanation: I just have to replace the word 'ether' with the phrase 'conventionally chosen system at rest' and do the same calculations in this system. Even better, the relativity principle and my new law for the composition of velocities directly lead to Fizeau's result: you only have to compose the relative velocity c/n of light in water with the velocity of the water.[99] In the end my theory will turn out more complete than yours. For you ignore atoms and electrons, and have nothing to say about the dynamics of the electron, which is accessible to experiment.

Cohn: I agree that my theory is only phenomenological, and that the recent progress of atomistics will probably require a consistent connection between the atomic and macroscopic levels. One of my former students has already tried to apply my equations at the electronic level.[100] It is too early to judge the outcome. In the meantime, I do not feel compelled to adopt your theory and to give up absolute space and time.

Einstein: Whichever theory you opt for, you must admit that the time of moving clocks synchronized by optical means is identical to Prof. Lorentz's local time. Also, I do not see how, as an admirer of Mach, you so readily admit an absolute space in your theory. Do you not see that if there is no ether, as we both assume, we can only talk about motion of matter with respect to matter?

Cohn: Regarding your first point, I would be more cautious than you are. Optical synchronization is only one option. Quite possibly, a mechanical procedure of synchronization would give the absolute time. This would surely violate the relativity principle, since one could detect the Earth's motion simply by comparing optically and mechanically synchronized clocks. But I do not share your belief in an unlimited validity of this principle. After all, it only is a principle of mechanics. You may as well reject it since you have already given up other mechanistic notions such as the ether. I certainly agree with you and Mach that there is only motion of matter with respect to matter. But what I mean by absolute space is the system defined by the fixed stars, which are matter.

Poincaré: The validity of the relativity principle, as Dr Einstein and myself define it, can be deduced neither from its mechanical origin nor from the grammar of 'relative motion.' The principle is an inductive generalization of experimental results, just like the energy principle.[101] However, the number of optical and electrodynamic experiments that confirm the relativity principle is so large, that it seems

[99] This reasoning was first given in Laue 1907, and was reproduced in Einstein 1907a: 426. Cf. Miller 1981a: 278–80.

[100] Cf. Gans 1905. Laue criticized Gans's calculations: cf. Darrigol 1995b: note 37.

[101] Poincaré argued for the inductive nature of principles, e.g. in Poincaré 1904b: 301: 'These principles are results of experiments boldly generalized; but they seem to derive from their very generality a high degree of certainty.'

reasonable to take the principle as a postulate and to try to construct a theory that complies with it. I have done this myself, on the basis of Prof. Lorentz's recent Dutch memoir. The resulting theory is entirely equivalent to Dr Einstein's, as far as experimental predictions and constraints on theory construction are concerned. For example, we both agree about the mass formulas of the electron, and we both expect future theories to be invariant under the Lorentz group. However, I do not share Dr Einstein's view that we should adopt a new doctrine of space and time. We can very well, without any infraction of logic, maintain the idea of a privileged reference system called the ether, to which true space, true time, and true field refer. Then my convention for clock synchronization and the Lorentz contraction yield the *apparent* space and time for moving observers.[102]

Einstein: Although Prof. Poincaré's viewpoint is perfectly consistent, I cannot make it mine. An adequate theory should not introduce distinctions that have no empirical counterpart. The distinction between apparent and true time is precisely of that kind. If you assume the relativity principle, you must not only assume that the laws of physics are the same in every inertial system, but also that their theoretical expression is invariant. Keeping the ether introduces an unnecessary complication. Moreover, I find it unsatisfactory to take the Lorentz contraction—not exactly a natural idea—as a premise of the theory.

Poincaré: I will first answer your last point. In my latest thinking, the Lorentz contraction is no longer an additional assumption. It in fact derives from a convention on length measurement—the same as that found in your paper—together with the relativity principle.[103] Your elimination of the ether does not imply such a greater logical simplicity. In your derivation of the Lorentz transformation, you introduce, for the convenience of expression, a 'system at rest.'[104] In order to make your deductions compatible with my view, I would just have to replace 'system at rest' with 'ether,' and to call 'apparent' every quantity referred to the system in motion. Is this a prohibitive complication? Furthermore, I believe that your introduction of a new kinematics could be harmful from a pedagogical point of view. Common physical phenomena fit ordinary kinematics and geometry very well, and this will always be so. Should we change deep habits of thought just because we find it more convenient in some extreme situations, involving exceedingly small effects or velocities approaching that of light? I do not think so.[105] When we have to choose between

[102] Poincaré never publicly commented on Einstein's theory. After meeting him at the Solvay conference in 1911, Einstein reported to his friend Zangger: 'Poinkaré [*sic*] was in general simply antagonistic and, for all his acuity, showed little understanding of the situation' (Einstein to Zangger, 15 November 1911, *ECP5*: 308).

[103] Cf. Poincaré 1908: 567. More exactly, Poincaré derived the Lorentz contraction from the convention that lengths are measured by the time taken by light to travel through them and from the negative result of the Michelson–Morley experiment. However, the latter result can be derived from the relativity principle.

[104] Einstein 1905b: 892: 'In order to distinguish this coordinate system verbally from others which will be introduced later, and for the precision of the representation (*Vorstellung*), we call it the "system at rest".'

[105] Poincaré 1908 reasoned in similar terms to defend Newtonian mechanics.

two equally permissible conventions, we should take the one that is most convenient. And convenience is not only a matter of logical simplicity; it involves psychology, pedagogy, and tradition.[106] Many great physicists of the past have not shared your epistemological criterion of non-ambiguity in theory representation. For example, the physicists who used mechanical models in electrodynamics had an infinite number of choices for their models. Also, the electromagnetic potentials, which played a central role in Maxwell's theory, are essentially ambiguous.

Abraham: Prof. Poincaré and Dr Einstein may well disagree on epistemological issues, but they predict the same values for the masses of the electron, that is, Prof. Lorentz's. Dr Kaufmann's latest experiments contradict this prediction and instead agree with my theory of the rigid, spherical electron. I should add that Prof. Poincaré's argument in favor of the principle of relativity does not convince me at all. It may be true that so far we know of no exception to this principle. But it contradicts the electromagnetic world-view.[107] This view offers so great a hope for a new, unified physics that we should resist the temptation to sacrifice it on the altar of an old-fashioned principle of mechanics. Prof. Poincaré finds Mr Einstein too iconoclastic, I instead find him too conservative.[108] The situation is somewhat comparable to what we have witnessed in the case of the entropy principle. For a while, inductive generalization led physicists to believe that the latter principle held generally and absolutely. However, this view contradicts the kinetic theory and the atomistic conception of matter, which is now regarded as a privileged route to the unification of physics.

Poincaré: We are all aware of Dr Kaufmann's results. However, not too much trust can be put on a single, difficult experiment, even when it is performed by someone as skillful as Dr Kaufmann. We must wait before drawing a final conclusion.[109]

Einstein: In my opinion, the probability of the theory of relativity is so high that Dr Kaufmann's results have every chance of being flawed.[110] Dr Abraham's argument against the relativity principle simply does not hold. The electromagnetic view of nature may have been very hopeful when it was first introduced, but in its present form it contradicts what we know of blackbody radiation, as I have shown in my paper 'On a heuristic viewpoint. . . .' Hence, I do not believe that we yet have a sat-

[106] Poincaré is likely to have reasoned in these terms, by analogy with his defense of Euclidean geometry: cf. Paty 1993: 264–71.

[107] Such arguments can be found, e.g., in Abraham 1908.

[108] At the *Naturforscherversammlung* of 1906, Planck was the only physicist to support Einstein's theory. Other physicists favored the electromagnetic worldview, which they judged more progressive. Sommerfeld declared: 'I would suspect that the gentlemen under forty would prefer the electrodynamic postulate, those over forty the mechanical-relativist postulate.' (*PZ* 7 (1906): 759–61). Cf. Jungnickel and McCormmach 1986, Vol. 2: 250.

[109] This opinion is expressed in Poincaré 1908: 572.

[110] Cf. Einstein 1907a: 439: 'In my opinion, a small probability should be ascribed to these theories [of Abraham and Bucherer], since their fundamental assumptions about the mass of a moving electron are not supported by theoretical systems that embrace wider complexes of phenomena.'

isfactory basis to discuss the make up of the electron. Temporarily, we can use the relativity principle to derive the dynamic properties of the electron that are independent of its constitution. Beyond that, I do not even know what a model of the rigid electron could be like, since we do not yet have a concept of rigidity that is compatible with the relativity principle.[111]

Bucherer: Prof. Poincaré and Dr Einstein seem to take for granted that their theory is the only one that respects the relativity principle and accounts for the established laws of electrodynamics. I must take exception to this view. I have shown that the interactions between electric or magnetic poles predicted by Prof. Lorentz's theory can be modified in such a way as to comply with the relativity principle, without leaving the framework of ordinary kinematics. I personally agree with Dr Einstein's epistemological requirement about the non-ambiguity of representation. I also agree with him that the ether should be completely eliminated from electromagnetic theory. However, this can be done without introducing Dr Einstein's strange doctrine of space and time.

Einstein: I am quite sympathetic with Dr Bucherer's attempt. In fact, I had myself a similar idea some time ago. However, I have a strong suspicion that Dr Bucherer's theory, after being sufficiently developed, will lead to absurdities, which happened in my own past attempt. Moreover, my present theory is simpler than his, for in a given system of reference it maintains Prof. Lorentz's simple prescriptions for calculating the interactions between electrons.[112]

Lorentz: To conclude this discussion, I would like to make a few comments. Regarding Dr Kaufmann's new experiments, I have no definite opinion. My first reaction, as expressed in a letter to Prof. Poincaré, was one of complete distress. Now, I have become aware of some defects of Dr Kaufmann's device that cast some doubt on his conclusions.[113] Then my theory is still an open possibility. Dr Abraham's attempt seems less fortunate. Even if the electron's masses turned out to agree with his theory, he would still have to face a contradiction with the null result of Rayleigh and Brace. Prof. Cohn's theory has nothing to say on electron dynamics as yet, and it has some strange consequences: for instance the Michelson–Morley experiment performed in a perfect vacuum would yield a positive result. Dr Bucherer's theory is so underdeveloped that I cannot help sharing Dr Einstein's suspicion that it will ultimately fail. There remains my own theory. Here I must applaude Prof. Poincaré and Dr Einstein for the great simplification they have brought to my original ideas. Whereas my 'corresponding states' were only formal intermediates, in the hands of Prof. Poincaré and Dr Einstein they have acquired a physical meaning. Most important, the space and time coordinates given by what Prof. Poincaré kindly calls the 'Lorentz transformation' are those measured by moving observers who adopt the convention that light has the constant velocity c. This interpretation makes it obvious that the formal invariance of the theory implies its physical invariance. At that stage,

111 Einstein 1905a. This view is expressed in Einstein 1907b.
112 No comment of Einstein's on Bucherer's theory of relativity is known.
113 Cf. Planck 1906: 753–61.

we may either keep the ether and differentiate apparent space and time from true space and time, or we may, as Dr Einstein suggests, regard all reference systems as completely equivalent. The difference between the two point of views is purely epistemological. Regarding physical predictions and theory construction they are completely equivalent. For my part, I prefer Prof. Poincaré's view, because I have been accustomed to think in terms of ether and absolute time and can see no benefit in renouncing these familiar concepts.[114]

This imaginary discussion of the electrodynamics of moving bodies may help appreciate the relative strength of the positions held by the various continental experts. In 1906 none of their theories was clearly refuted, and they could all be rationally maintained. However, Poincaré's and Einstein's theories were the easiest to defend. Abraham still held a strong position, mostly because of Kaufmann's latest result. Cohn's and Bucherer's credibility depended on their ability to complete their theories, which did not cover as much ground as their competitors'. Their attempts were essentially isolated, whereas the theories of Poincaré, Einstein, and Lorentz were deeply interconnected, and Abraham's theory had a number of followers in Göttingen and elsewhere. Therefore, a non-adventurous bettor of the year 1906 would have been left with two experimentally distinguishable possibilities: Abraham's electromagnetic world-view, or the Poincaré–Einstein–Lorentz relativity principle.

Believers in the relativity principle still had a choice between two alternatives: Einstein's new kinematics, and the Poincaré–Lorentz ether theory. In the improved version found in Poincaré's Sorbonne lectures of 1906/7 and in most of Lorentz's later writings, the latter theory was equivalent to Einstein's, both empirically and programatically. For example, Poincaré no longer appealed to the Maxwell–Lorentz equations to derive the Lorentz transformation: he now combined his synchronization procedure with the Lorentz contraction. And he no longer based his electron dynamics on a specific model of the electron: like Einstein, he directly exploited the Lorentz invariance. The only remaining difference was the survival of the ether as a privileged reference system. This was a matter of epistemological taste.

It should now be clear that the superiority of Einstein's view is only retrospective. Around 1906 a number of alternative views could be reasonably held, according to the diverging interests of the main actors. In harmony with this fact, the best

[114] Lorentz's reaction to Bucherer's relativity theory is unknown. However, he had a good opinion of Bucherer's works in general, as reveals a report he wrote after 1908 (AHQP, following the Bucherer–Lorentz letters). Lorentz's objection to Abraham's electron is in Lorentz 1909: 218–19. For his (belated) opinion on Poincaré's contribution, cf. Lorentz 1914: 258–66. For his opinion on Einstein's theory, cf., e.g., Lorentz 1909: 223–30; and 1920: 23, quoted in Pais 1982: 166: 'It is certainly remarkable that these relativity concepts, also those concerning time, have found such a rapid acceptance. The acceptance of these concepts belongs mainly to epistemology [. . .]. It is certain, however, that it depends to a large extent on the way one is accustomed to think whether one is most attracted to one or another interpretation. As far as this lecturer is concerned, he finds a certain satisfaction in the older interpretations, according to which the ether possesses at least some substantiality, space and time can be sharply separated, and simultaneity without further specification can be spoken of.' On Lorentz's interpretation of the Lorentz transformation after 1905, cf. Janssen 1995: 240–90.

historians of the reception of special relativity have refused to regard Einstein's seminal text as self-evident truth. Although Einstein's intelligent readers probably grasped most of his intentions, they could reasonably reject part of them and filter out aspects that could be profitably integrated in their own researches. The process by which the new kinematics conquered some German elite was gradual, complex, and roundabout. It involved circumstances as diverse as Max Planck's prompt support, Bucherer's electronic deflection experiments of 1908, Hermann Minkowski's four-dimensional world proposed in the same year, and tentative applications to models of the electron. What requires explanation is not a supposed blindness of Einstein's readers, but rather the factors that ultimately made Einstein's approach more attractive than other valid approaches.[115]

9.6 Conclusions

At the turn of the century, Lorentz's theory was commonly regarded as the best basis for integrating atomism into electrodynamics. Yet the physicists' attitude toward this theory varied considerably. This diversity can only be understood by referring to the previous history of continental electrodynamics.

One must first remember the old opposition between constructive, microphysical approaches to electrodynamics (Weber's) and more phenomenological ones (Neumann's and Kirchhoff's). At the end of the century, a similar contrast existed in German physics in general. The blazing debates about the usefulness of kinetic molecular theory are well known. In electrodynamics there was a similar antagonism, between Lorentz's and Wiechert's neo-Weberianism on the one hand, and the positivism of physicists like Cohn and Voigt on the other. Whereas the electron theories aimed at a detailed microscopic picture of the world, Cohn avoided any microphysical speculation and modified the macroscopic field equations according to experimental needs. With this strategy, he reached an electrodynamics that included all classical results of the optics of moving bodies, including Fizeau's and Michelson–Morley's. Other experts respected this theory, although they usually adopted the electronic picture of matter.

Continental electrodynamics was also marked by the clash between indigenous and Maxwellian conceptions. The confrontation with the radical novelty of British field concepts prompted epistemological reflections, and reactivated the issue of mechanical reductionism. Most continental physicists were skeptical of British attempts at a detailed mechanical picture of the ether. Some of them, like Cohn, rejected any kind of reduction in the name of Mach's economy of thought. More commonly, they adopted the moderate kind of mechanical reductionism inaugurated by Maxwell and Helmholtz, which sought to subsume physics under the general principles of mechanics: energy conservation, least action, and equality of action and reaction.

[115] Cf. Warwick 1989, 1992, 1993a; Staley 1992; Walter 1996, 1999.

At the end of the century, Henri Poincaré was the most penetrating advocate of this 'physics of principles.' Quite originally, he combined his faith in the general principles of mechanics with a skepticism about the ether. In his opinion the ether could not possess material attributes as crude as inertia and momentum. He required that the principle of relativity and the principle of reaction should apply to matter *alone*. With this criterion he diagnosed a severe crisis of electrodynamics: no known theory, not even Lorentz's, could account for Fizeau's experiment without contradicting the two principles. Poincaré's faith in the relativity principle had another consequence. He saw that the velocity of light measured by moving observers could only be the same as for observers at rest, and used this criterion to define apparent simultaneity in a moving system of axes. He thus provided Lorentz's fictitious coordinates with a physical content and greatly simplified the technique of corresponding states.

Other physicists were more willing to ascribe material attributes to the ether, and they regarded the failure to detect terrestrial effects of the ether wind as temporary. A few of them, Wilhelm Wien and Max Abraham to the fore, tried to reduce the mechanics of matter to electromagnetism. They regarded all matter as made of electrons, and the momentum of the moving electrons as entirely due to the corresponding ether disturbance. The experimental dependence of the electron's mass on velocity seemed to corroborate this electromagnetic world-view. Several German physicists of the younger generation adopted it enthusiastically as a revolutionary substitute for the decaying mechanical world-view.

Poincaré and Lorentz were skeptical of a full electromagnetic reduction, and paid more attention to the ongoing failure of ether wind experiments. However, they approved the new focus on the inner constitution and dynamics of the electron, and adopted the notions of electromagnetic momentum and mass. Thanks to the contractile electron model and the extension of his earlier technique of corresponding states, in 1904 Lorentz managed to prove the invariance of optical experiments at any order (but in the dipolar approximation). The following year Poincaré improved Lorentz's new theory to obtain exact compatibility with the 'postulate of relativity.' He defined the Lorentz group, and required that every interaction, including non-electromagnetic ones, should be invariant under that group. Yet he maintained the ether as a privileged but undetectable reference frame for defining 'true' space and time.

By 1905, the electrodynamics of moving bodies had become a popular topic, with a broad spectrum of approaches. Physicists disagreed on issues as fundamental as the existence of the ether, the need for a purely microscopic theory, the validity of the relativity principle, and the pertinence of the electromagnetic world-view. Among various conflicting schemes were: Abraham's purely electromagnetic theory of the rigid electron, Lorentz's theory of the contractile electron, Poincaré's relativistic reformulation of it, Cohn's ether-less macroscopic field theory, and Bucherer's field-less microscopic theory.

Once placed in this context, Einstein's electrodynamics of 1905 no longer appears as a singular, isolated attempt. Most components of this theory are found in contemporary literature. Cohn and Bucherer had already rejected the ether concept; Cohn had criticized the distinction between true and measured time in Lorentz's theory; Poincaré and Bucherer had expressed their belief in the general validity of the relativity principle; Poincaré had a physical interpretation of Lorentz's local time and the complete form of the Lorentz transformation; and he had detected the paradoxes which Einstein later solved by introducing the inertia of energy. Although the overall organization of Einstein's theory was unique, its only major novelty was the reform of the basic concepts of space and time. To which we may add a new derivation of the Lorentz transformations without reference to the Maxwell's equations, and a new derivation of the dynamics of the electron without specific model. However, Poincaré independently used similar reasonings in his Sorbonne lectures of 1906–7.

Einstein's theory was only one possibility among others. The Rayleigh–Brace null result and Kaufmann's measurements on fast electrons failed to sharply decide between Abraham's electromagnetic world-view and the principle of relativity. No present or future experiment could in principle decide between Poincaré's and Einstein's understandings of the relativity principle, since Poincaré's ether was strictly undetectable. Cohn and Bucherer still hoped that future developments would fill the gaps in their theories. In the history of electrodynamics, 1905 was a year of abundance, not a year of conclusion. It seems appropriate, however, to close the curtains at a time when the conflicting historical forces of nineteenth century electrodynamics were still at work.

Appendices

Appendix 1
Ampère's forces

The forces between two elements of current

Ampère's formula for the force between two current elements reads

$$\mathrm{d}^2 f = ii'\frac{\mathrm{d}s\mathrm{d}s'}{r^2}\left(\sin\alpha\sin\beta\cos\gamma - \frac{1}{2}\cos\alpha\cos\beta\right), \tag{A.1}$$

where i and i' are the intensities of the two currents in electrodynamic units (see Appendix 2), $\mathrm{d}s$ and $\mathrm{d}s'$ the lengths of the two elements, r their distance, α and β the angles between the elements and the line joining them, and γ the angle between the two planes passing through this line and containing one of the elements (see Fig. 1.3, p. 9). In Ampère's convention, the force is positive when attractive.[1]

This force may be re-expressed in modern vector notation, in terms of the vectors $\mathbf{l}$ and $\mathbf{l}'$ giving the position of the two elements on their respective circuits. The vector $\mathbf{r}$ denotes the difference $\mathbf{l} - \mathbf{l}'$. By definition of the angles α, β, and γ, we have

$$\begin{aligned} \mathbf{r}\cdot\mathrm{d}\mathbf{l} &= r\mathrm{d}s\cos\alpha, \\ \mathbf{r}\cdot\mathrm{d}\mathbf{l}' &= r\mathrm{d}s'\cos\beta, \\ \mathrm{d}\mathbf{l}\cdot\mathrm{d}\mathbf{l}' &= \mathrm{d}s\mathrm{d}s'(\cos\alpha\cos\beta + \sin\alpha\sin\beta\cos\gamma). \end{aligned} \tag{A.2}$$

Consequently, the vector form of the Ampère force acting on the element $\mathrm{d}\mathbf{l}$ is

$$\mathrm{d}^2\mathbf{f} = -ii'\frac{\mathbf{r}}{r}\left[\frac{\mathrm{d}\mathbf{l}\cdot\mathrm{d}\mathbf{l}'}{r^2} - \frac{3}{2}\frac{(\mathbf{r}\cdot\mathrm{d}\mathbf{l})(\mathbf{r}\cdot\mathrm{d}\mathbf{l}')}{r^4}\right]. \tag{A.3}$$

A third form of Ampère's law, given by Ampère himself, uses the derivatives of the distance r with respect to the curvilinear abcissae s and s' along the trajectories of the two linear currents. Differentiating the identity $r^2 = (\mathbf{l} - \mathbf{l}')^2$ with respect to s and s' yields

[1] Ampère 1822d: 418; Ampère 1826b: 21, 44.

$$\mathbf{r}\cdot\mathrm{d}\mathbf{l} = r\mathrm{d}s\frac{\partial r}{\partial s},$$

$$-\mathbf{r}\cdot\mathrm{d}\mathbf{l}' = r\mathrm{d}s'\frac{\partial r}{\partial s'}. \tag{A.4}$$

In turn, differentiating the first of these two identities with respect to s' yields

$$-\mathrm{d}\mathbf{l}\cdot\mathrm{d}\mathbf{l}' = \mathrm{d}s\mathrm{d}s'\left(\frac{\partial r}{\partial s}\frac{\partial r}{\partial s'} + r\frac{\partial^2 r}{\partial s\partial s'}\right). \tag{A.5}$$

Consequently, the formula (A.3) is equivalent to

$$\mathrm{d}^2 f = -ii'\frac{\mathrm{d}s\mathrm{d}s'}{r^2}\left(r\frac{\partial^2 r}{\partial s\partial s'} - \frac{1}{2}\frac{\partial r}{\partial s}\frac{\partial r}{\partial s'}\right), \tag{A.6}$$

which Ampère liked to see as[2]

$$\mathrm{d}^2 f = -ii'\mathrm{d}s\mathrm{d}s'\frac{2}{\sqrt{r}}\frac{\partial^2\sqrt{r}}{\partial s\partial s'}. \tag{A.7}$$

To Ampère's formula Grassmann preferred

$$\mathrm{d}^2\mathbf{f}_{\mathrm{G}} = \frac{1}{2}i\mathrm{d}\mathbf{l}\times\frac{i'\mathrm{d}\mathbf{l}'\times\mathbf{r}}{r^3}, \tag{A.8}$$

which was simpler from the point of view of his theory of extension. When the second linear current is closed, this formula is equivalent to Ampère's because the difference

$$\mathrm{d}^2\mathbf{f} - \mathrm{d}^2\mathbf{f}_{\mathrm{G}} = (\mathrm{d}\mathbf{l}'\cdot\nabla')\left(\frac{1}{2}ii'\frac{\mathbf{r}(\mathbf{r}\cdot\mathrm{d}\mathbf{l})}{r^3}\right) \tag{A.9}$$

vanishes when integrated with respect to $\mathbf{l}'$. The formulas (1.6) and (1.7) of Chapter 1 immediately result from this equivalence.[3]

Grassmann's formula, unlike Ampère's, does not satisfy the equality of action and reaction. However, it agrees with modern electrodynamics in the approximation of stationary currents. Indeed, in electromagnetic units the magnetic field created by the current element $i'\mathrm{d}\mathbf{l}'$ is

$$\mathrm{d}\mathbf{H} = \frac{i'\mathrm{d}\mathbf{l}'\times\mathbf{r}}{r^3}, \tag{A.10}$$

and the force $\mathrm{d}\mathbf{f}$ acting on a current element $i\mathrm{d}\mathbf{l}$ placed in the magnetic field $\mathbf{H}$ is

$$\mathrm{d}\mathbf{f} = i\mathrm{d}\mathbf{l}\times\mathbf{H}. \tag{A.11}$$

[2] Ampère [1822e]; 1826b: 44. [3] Grassmann 1845.

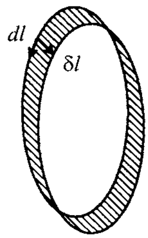

FIG. A.1. Displaced loop.

Combining these two formulas gives Grassman's formula (A.8), up to a factor half corresponding to the change from electrodynamic to electromagnetic units (see Appendix 2). The rest of this appendix uses electromagnetic units.

Equivalence between magnets and systems of currents

Ampère's theory of magnets depended on the equivalence between the ends of infinitely thin solenoids and magnetic poles. This equivalence derives from a more basic fact: the mechanical actions between two rigid, infinitesimal current loops are identical to those between two magnetic dipoles. Seen as a pile of small loops, a solenoid is then equivalent to a linear sequence of dipoles of constant strength, or else to two poles placed at its extremities.[4]

The basic fact can be proven in two steps. Consider first the action of an arbitrary closed-current system on an infinitesimal, rigid, and flat loop. It is given by integrating the formula (A.11), the vector **H** being given by the integration of (A.10) (the field-theoretical interpretation of **H** is here unnecessary). Consider, a bit anachronistically, the work δW produced by the forces (A.11) when the loop is submitted to a displacement of higher infinitesimal order (translation, rotation, or both). Calling $\delta\mathbf{l}$ the shift of the element $\mathrm{d}\mathbf{l}$ of the loop during this displacement, we have

$$\delta W = \oint i\delta\mathbf{l}\cdot(\mathrm{d}\mathbf{l}\times\mathbf{H}) = \oint i\mathbf{H}\cdot(\delta\mathbf{l}\times\mathrm{d}\mathbf{l}). \tag{A.12}$$

The latter integral represents the flux of the vector $i\mathbf{H}$ entering the surface swept by the loop in its displacement (see Fig. A.1). Since **H** is divergence-less, this flux is also equal to the variation of the flux $i\mathrm{d}\mathbf{S}\cdot\mathbf{H}$ accoss the surface $\mathrm{d}\mathbf{S}$ of the loop during its displacement. Consequently, the mechanical action on the loop derives from the potential $-i\mathrm{d}\mathbf{S}\cdot\mathbf{H}$, which is the same as the potential of a magnetic dipole $\mathbf{M} = i\mathrm{d}\mathbf{S}$ in the magnetic field **H**.

In a second step, we further assume that the current i' runs on a second infinitesimal, flat, and rigid loop. We submit this loop to a translation $\delta\mathbf{l}'$ of higher infinitesimal order. The following identities hold:

$$\mathbf{H}\cdot\delta\mathbf{l}' = \oint i'\delta\mathbf{l}'\cdot\frac{\mathrm{d}\mathbf{l}'\times\mathbf{r}}{r^3} = \oint i'\frac{\mathbf{r}}{r^3}\cdot(\delta\mathbf{l}'\times\mathrm{d}\mathbf{l}'). \tag{A.13}$$

The field $\mathbf{r}/r^3$ being divergenceless in the vicinity of the loop, the latter integral is equal to the variation of the flux $i'\mathrm{d}\mathbf{S}'\cdot\mathbf{r}/r^3$ across the surface $\mathrm{d}\mathbf{S}'$ of the loop during its translation.

[4] Ampère 1826b: 73–83.

Consequently, $\mathbf{H}$ is the gradient of this quantity with respect to $\mathbf{l}'$, or the gradient of its opposite with respect to $\mathbf{l}$. Hence $\mathbf{H}$ has the same form as the force from a magnetic dipole $\mathbf{M}' = i'\mathrm{d}\mathbf{S}'$, which derives from the potential $\mathbf{M}' \cdot \nabla'(1/r) = \mathbf{M}' \cdot \mathbf{r}/r^3$. This result ends the equivalence proof.

Appendix 2
Absolute units

In the electrostatic unit- system, the unit of charge is chosen so that the force f between two point charges q and q' separated by the distance r is simply given by $f = qq'/r^2$, with no numerical coefficient. Then the unit of current is defined by the transfer of a unit charge in a unit time.[5]

In the electrodynamic system, the unit of current is such that Ampère's law (A.1) holds, with no additional numerical coefficient. Then the unit of charge is defined as the charge transferred by a unit current in a unit time.[6]

In the electromagnetic system, a unit magnetic pole is first defined so that two unit poles separated by a unit length exert on each other a unit force. Then the unit of current is chosen so that the force $\mathrm{d}f$ between a magnetic pole with the strength m and a current element $i\mathrm{d}l$ at the distance r is $mi\mathrm{d}l/r^2$ when the element is normal to the line joining it to the pole. The unit of charge follows.[7]

The comparison between eqn. (A.8), which derives from Ampère's law in electrodynamic units, and the eqns. (A.10) and (A.11), which hold in electromagnetic units, implies that the electromagnetic unit of current is $\sqrt{2}$ times larger than the electrodynamic unit of current. The dimensional analysis of Ampère's law and Coulomb's law implies that the ratio of the electromagnetic unit of current (or charge) to the electrostatic unit of current (or charge) has the dimension of velocity. In this book this constant is denoted by the letter c (this is not Weber's notation!). In Maxwell's theory, it must be the same as the velocity of light in vacuum. The very large value of c (3×10^8 m/s) implies that the charges transferred by galvanic currents are enormous compared with those produced by electrostatic means.

To make matters more complicated, for the electrostatic measure of current Weber considered only the flow of the positive electricity, which is half the transfer of charge in the Fechner–Weber picture of the current as a symmetrical double flow. Consequently, the constant a which for Weber (Weber 1846: 115) gave the ratio of the electrodynamic to the electrostatic measure of current, is in fact twice the usual definition of this ratio. Hence, $a = 2\sqrt{2}/c$. In 1850 (Weber 1850: 268) Weber used the constant $C = 4/a = c\sqrt{2}$, because the Weber force between two electric particle vanishes when their relative velocity is C and their relative acceleration vanishes. Although his notation for the latter constant was c, I have called it C in order to avoid confusion with the modern notation for the velocity of light.

[5] Weber 1850: 267–70. [6] Weber 1846: 51–60. [7] Weber 1846.

Appendix 3
Neumann's potential

Call **E** the electromotive force induced in an element dl of a linear conductor moving at the velocity **v** in the vicinity of constant currents or magnets at rest. In modern vector notation, Neumann's 'elementary law' (eqn. 2.1) reads

$$\mathbf{E}\cdot\mathrm{d}\mathbf{l} = -\varepsilon\mathbf{v}\cdot\mathrm{d}\mathbf{f}_1, \qquad \text{(A.14)}$$

where $\mathrm{d}\mathbf{f}_1$ is the electromagnetic force that would act on the element if it were carrying a unit electric current. Unlike Neumann, I choose electromagnetic units, for which the constant ε is unity.[8]

When the acting currents are closed, or in the presence of a magnet, according to the consequence (A.11) of Ampère's law the force $\mathrm{d}\mathbf{f}_1$ is equal to $\mathrm{d}\mathbf{l}\times\mathbf{H}$, where **H** is the force acting on a unit magnetic pole. Consequently, we have

$$\mathbf{E}\cdot\mathrm{d}\mathbf{l} = -\mathbf{v}\cdot(\mathrm{d}\mathbf{l}\times\mathbf{H}) = \mathrm{d}\mathbf{l}\cdot(\mathbf{v}\times\mathbf{H}), \qquad \text{(A.15)}$$

so that[9]

$$\mathbf{E} = \mathbf{v}\times\mathbf{H}. \qquad \text{(A.16)}$$

The time integral of the electromotive force acting in an open linear conductor may be written

$$\int e\,\mathrm{d}t = \iint(\mathbf{v}\,\mathrm{d}t\times\mathbf{H})\cdot\mathrm{d}\mathbf{l} = \iint_{\Sigma}\mathbf{H}\cdot(\mathrm{d}\mathbf{l}\times\delta\mathbf{l}), \qquad \text{(A.17)}$$

where $\delta\mathbf{l}$ is the vector by which a point of the conductor is translated in the time dt, and Σ is the surface swept by the conductor. In other words, the induced electromotive force is proportional to the number of magnetic lines of force crossed by the conductor, in conformity with Faraday's rule.

Neumann naturally ignored such field notions. Instead he introduced the potential P from which the forces acting on a current-carrying conductor derive. In the case of two interacting linear circuits with the currents i and i', this potential reads

$$P = -ii'\oint\oint\frac{\mathrm{d}\mathbf{l}\cdot\mathrm{d}\mathbf{l}'}{|\mathbf{l}-\mathbf{l}'|} \qquad \text{(A.18)}$$

in electromagnetic units (Neumann used Ampère's electrodynamic units, for which a factor half enters the potential formula).[10] In general, the electrodynamic potential of a circuit with respect to other currents or magnets has the form

$$P = -i\oint\mathbf{A}\cdot\mathrm{d}\mathbf{l}, \qquad \text{(A.19)}$$

[8] Neumann 1846: 13–16.
[9] Here **H** is identical to Maxwell's **B**, since the permeability of the medium is unity.
[10] Neumann 1846: ##10–11.

which Neumann, however, did not use. The variation of this potential during a virtual deformation $\delta\mathbf{l}$ of the circuit is

$$\delta P = -i\oint \delta\mathbf{A}\cdot d\mathbf{l} - i\oint \mathbf{A}\cdot d\delta\mathbf{l}. \tag{A.20}$$

Partial integration of the second integral gives

$$\delta P = -i\oint[\delta\mathbf{l}\cdot[\nabla(\mathbf{A}\cdot d\mathbf{l}) - (d\mathbf{l}\cdot\nabla)\mathbf{A}] = -i\oint \delta\mathbf{l}\cdot[d\mathbf{l}\times(\nabla\times\mathbf{A})]. \tag{A.21}$$

Consequently, the force acting on the element $\delta\mathbf{l}$ is equal to

$$d\mathbf{f} = i d\mathbf{l}\times(\nabla\times\mathbf{A}), \tag{A.22}$$

in conformity with the consequence (A.11) of Ampère's law, if $\mathbf{A}$ is chosen such that

$$\mathbf{H} = \nabla\times\mathbf{A}. \tag{A.23}$$

Using Stokes's theorem, the integral electromotive force (A.17) can be rewritten as

$$\int e\,dt = \oint_{\gamma} \mathbf{A}\cdot d\mathbf{l}, \tag{A.24}$$

wherein γ denotes the contour of Σ (with the proper orientation). Hence there follows Neumann's induction law:

The time integral of the electromotive force induced in a linear conductor is equal to the potential of a unit current flowing in an imaginary circuit made of the conductor at its initial position, the conductor at its final position, and the traces of the two extremities of the conductor during the motion.[11]

When applied to a *closed linear conductor*, this law implies that *the time integral of the electromotive force is equal to the variation of the potential of a unit current in this conductor* (with respect to the other conductors or magnets). This is Neumann's 'principle,' which also applies when the cause of the variation of the potential is the variation or motion of the acting currents and magnets.[12]

[11] Neumann 1846: 68. [12] Neumann 1848.

Appendix 4
Weber's formula and consequences

The fundamental law

Weber's fundamental law for the pair of forces between two electric fluid particles e and e' is[13]

$$f=\frac{ee'}{r^2}\left[1-\frac{1}{2c^2}\left(\frac{\mathrm{d}r}{\mathrm{d}t}\right)^2+\frac{r}{c^2}\frac{\mathrm{d}^2r}{\mathrm{d}t^2}\right], \tag{A.25}$$

where r is the distance between the two particles and c is the ratio of the electromagnetic to the electrostatic unit of charge. In Weber's convention, a positive f means a repulsion. The two forces are directed on the line joining the two particles. As Weber proved in 1848, they derive from the potential[14]

$$V=\frac{ee'}{r}\left(1-\frac{\dot{r}^2}{2c^2}\right) \tag{A.26}$$

if the gradient $\mathrm{d}V/\mathrm{d}r$ is interpreted as $(\mathrm{d}V/\mathrm{d}t)/(\mathrm{d}r/\mathrm{d}t)$. Consequently, the work $f\mathrm{d}r/\mathrm{d}t$ performed by the two forces in a unit of time is $-(\mathrm{d}V/\mathrm{d}r)(\mathrm{d}r/\mathrm{d}t) = -\mathrm{d}V/\mathrm{d}t$. During a cycle for which r and $\mathrm{d}r/\mathrm{d}t$ return to their initial values, this work vanishes, in conformity with the energy principle. Moreover, Weber's forces obey Lagrange's equation[15]

$$f=-\frac{\partial L}{\partial r}+\frac{\mathrm{d}}{\mathrm{d}t}\frac{\partial L}{\partial \dot{r}}, \tag{A.27}$$

with

$$L=\frac{ee'}{r}\left(1+\frac{\dot{r}^2}{2c^2}\right). \tag{A.28}$$

Derivation of Ampère's law

Consider two linear conductors carrying the currents i and i'. These currents represent double flows of positive and negative electricity. The following notation is used: λ_+ for the density of the positive fluid in the first current, u_+ for its velocity, and so forth. The force between two current elements $i\mathrm{d}s$ and $i'\mathrm{d}s'$ results from the sum of the forces between the included fluid particles:

$$\mathrm{d}^2f=-\frac{\mathrm{d}s\mathrm{d}s'}{r^2}\sum_{\varepsilon\varepsilon'}\lambda_\varepsilon\lambda'_{\varepsilon'}\left(1-\frac{\dot{r}^2}{2c^2}+\frac{r\ddot{r}}{c^2}\right)_{\varepsilon\varepsilon'}, \tag{A.29}$$

[13] Weber 1846: 108, 119. [14] Weber 1848a. [15] See Riemann 1875 [1861]: 318.

where ε and ε' denote the signs of the fluid particles (the global sign reversal corresponds to Ampère's convention, according to which an attractive force is positive). With Ampère's curvilinear notation, we have

$$\dot{r} = u_\varepsilon \frac{\partial r}{\partial s} + u'_{\varepsilon'} \frac{\partial r}{\partial s'},$$
$$\ddot{r} = u_\varepsilon^2 \frac{\partial^2 r}{\partial s^2} + u'^2_{\varepsilon'} \frac{\partial^2 r}{\partial s'^2} + 2u_\varepsilon u'_{\varepsilon'} \frac{\partial^2 r}{\partial s \partial s'} + \dot{u}_\varepsilon \frac{\partial r}{\partial s} + \dot{u}'_{\varepsilon'} \frac{\partial r}{\partial s'}. \tag{A.30}$$

If the net charges of the two conductors vanish ($\lambda_+ + \lambda_- = 0$, $\lambda'_+ + \lambda'_- = 0$), the only terms surviving the summation are those proportional to $u_\varepsilon u'_{\varepsilon'}$. The resulting force is

$$\mathrm{d}^2 f = -\frac{2ii'\,\mathrm{d}s\mathrm{d}s'}{c^2}\frac{1}{r^2}\left(r\frac{\partial^2 r}{\partial s \partial s'} - \frac{1}{2}\frac{\partial r}{\partial s}\frac{\partial r}{\partial s'}\right), \tag{A.31}$$

which retrieves the form (A.6) of Ampère's law, up to a factor $2/c^2$ corresponding to the different choice of units (electrostatic in the above reasoning, electrodynamic in Ampère's formula). Note that the derivation does not require the currents to be constant.[16]

If the net charge $\lambda = \lambda_+ + \lambda_-$ of the first conductor does not vanish, there are two additional contributions to the net force. The first comes from the terms proportional to $\mathrm{d}u_\varepsilon/\mathrm{d}t$. This force corresponds, in Maxwell's theory, to the action of the electric field generated by the variation of the magnetic field of the current in the second conductor. This effect is very small since electrostatic charges are always very small when measured in electromagnetic units. The other contribution comes from the terms proportional to $\lambda'_{\varepsilon'} u'^2_{\varepsilon'}$. It has the order of magnitude of

$$\frac{\mathrm{d}s\mathrm{d}s'}{r^2}\frac{1}{c^2}\lambda\left(\lambda'_+ u'^2_+ + \lambda'_- u'^2_-\right). \tag{A.32}$$

If, as Fechner and Weber originally assumed, the positive and negative electricity move with equal and opposed velocities, this force vanishes.

However, in later writings Weber and his disciples admitted unequal velocities. Then there should be an interaction between an electric charge at rest and a current. No force of that kind having been observed, Maxwell and Clausius both claimed that an asymmetrical motion of electricity was incompatible with Weber's law. They were wrong. Suppose, for example, that only the negative electricity moves (as is the case in metals). Then, in electromagnetic units the new force is like $qi'^2\mathrm{d}s'/r^2\rho_- S$, where q is the charge of the first conductor, S the section of the second, and ρ_- the charge density of the fluid running in it. Taking the value of this density from the Hall effect in copper ($\rho_- \sim 10^{10}$ coulomb/m^3), we obtain a force of about 10^{-12} newton for typical laboratory values of the variables: $q = 10^{-10}$ coulomb, $i = 1$ ampere, $\mathrm{d}s' = r = 1$ cm, and $S = 1$ mm^2. This force is so extremely small that tests performed in the 1970s (!) remained inconclusive. *A fortiori*, it should not have been held against Weber's law in the 1870s.[17]

[16] Weber's proofs assume Fechner's symmetrical double flow. The proof given in Weber 1846: 154–8 does not assume the constancy of currents.

[17] Maxwell 1873a: ##847–8; Clausius 1877a. Cf. Assis 1994: 168.

Electromagnetic induction by a variable current

The electromotive force is the 'separating force' of the two electricities, that is, the difference between the force acting on a positive unit of charge minus the force acting on a negative unit of charge. In the case of two linear conductors, the electromotive force created in the first conductor by the element $d\mathbf{l}'$ of the second conductor is

$$d\mathbf{E} = \frac{\mathbf{r}}{r}\frac{ds'}{r^2}\sum_{\varepsilon\varepsilon'}\lambda'_{\varepsilon'}\left(1-\frac{\dot{r}^2}{2c^2}+\frac{r\ddot{r}}{c^2}\right)_{\varepsilon\varepsilon'}. \tag{A.33}$$

As long as the two conductors are at rest we may use the expressions (A.30) for the time derivatives of r. Assuming a symmetrical motion of the two electricities ($u_+ + u_- = u'_+ + u'_- = 0$) and a zero net charge of the second conductor ($\lambda_+' + \lambda_-' = 0$), the only terms contributing to the sum are those proportional to $\lambda'_{\varepsilon'} du'_{\varepsilon'}/dt$. They sum to[18]

$$d\mathbf{E} = \frac{1}{c^2}\frac{\mathbf{r}}{r^3}\frac{di'}{dt}\frac{\partial r}{\partial s'}ds' = -\frac{1}{c^2}\frac{\partial}{\partial t}\left(\frac{\mathbf{r}}{r^3}(\mathbf{r}\cdot i'd\mathbf{l}')\right). \tag{A.34}$$

The three-dimensional generalization of the latter formula is

$$\mathbf{E} = -\frac{1}{c^2}\frac{\partial \mathbf{A}}{\partial t}, \tag{A.35}$$

with

$$\mathbf{A}(\mathbf{x}) = \int d^3x' \frac{\mathbf{x}-\mathbf{x}'}{|\mathbf{x}-\mathbf{x}'|^3}(\mathbf{x}-\mathbf{x}')\cdot\mathbf{j}'(\mathbf{x}'). \tag{A.36}$$

This is Kirchhoff's expression of the vector potential.[19] It may be rewritten as

$$\mathbf{A}(\mathbf{x}) = \int \frac{\mathbf{j}(\mathbf{x}')d^3x'}{|\mathbf{x}-\mathbf{x}'|} + \nabla\psi, \tag{A.37}$$

with

$$\psi = -\int \mathbf{j}(\mathbf{x}')\cdot\nabla|\mathbf{x}-\mathbf{x}'|d^3x' = -\int \nabla'\cdot\mathbf{j}(\mathbf{x}')|\mathbf{x}-\mathbf{x}'|d^3x'. \tag{A.38}$$

When the inducing currents are closed, the latter integral vanishes, and Weber's theory gives the same electromotive force as Neumann's.[20]

If we no longer assume a symmetrical flow of the two electricities, and if the net electric charge of the second conductor does not vanish, there are several new contributions to the electromotive force. However, as long as the velocity of the electric fluids remains a minute fraction of c (the Hall effect in copper gives a velocity of 10^{-4} m/s for a current of 1 ampere/mm^2), these contributions are negligible except for the purely electrostatic terms, which sum up to $-\nabla\phi$, where ϕ is the electrostatic potential of the net charge.

[18] Weber 1846: 144–7. [19] Kirchhoff 1857b. [20] Cf. Helmholtz 1870b.

Electromagnetic induction due to relative motion of the conductors

Let the element dl move with the velocity **v**, while the current element i'dl′ remains at rest and the current i' is kept constant. In this case, to the expressions (A.30) of the derivatives of r we must add

$$\delta\dot{r} = v\frac{\partial r}{\partial \sigma},$$
$$\delta\ddot{r} = \dot{v}\frac{\partial r}{\partial \sigma} + v^2\frac{\partial^2 r}{\partial \sigma^2} + 2u_\varepsilon v\frac{\partial^2 r}{\partial s \partial \sigma} + 2u'_{\varepsilon'} v\frac{\partial^2 r}{\partial s' \partial \sigma}, \quad \text{(A.39)}$$

where σ denotes the curvilinear abscissa on the path of the element dl (and $\dot{v} = v\partial v/\partial\sigma + u_\varepsilon \partial v/\partial s$). When the second conductor has no net charge, the only terms contributing to the electromotive force (A.33) are those proportional to $u'_{\varepsilon'}v$. They have the same form as the terms proportional to $u_\varepsilon u'_{\varepsilon'}$ that yield the Ampère force. Consequently, the electromotive force is formally the same as the force acting on a current element id**l** = **v** according to Ampère's law.[21] When the current i' is closed, we can use Grassmann's formula, and the electromotive force is $\mathbf{v} \times \mathbf{H}$, where **H** is given by eqn. (A.10). This result agrees with Neumann's and Maxwell's results.

When the net charge of the second conductor is not zero, the additional contributions to the electromotive force are all negligible, as long as all velocities remain very small compared with c and the velocity v changes very little in the time r/c.

The other case of induction by motion, where the element dl′ moves and the element dl is at rest, does not require a separate investigation, since Weber's forces depend only on the relative motion of the electric fluid particles.

[21] Weber 1846: 128–32.

Appendix 5
Convective derivatives

Deformations

Consider a continuous medium that can be deformed: for instance an elastic solid, a liquid, or Maxwell's electromagnetic medium. Helmholtz distinguishes three kinds of quantities defined at every point of the medium, according to their behavior during a deformation of the medium:

1. A density is such that its volume integrals are invariable.
2. A force is such that its line integrals are invariable.
3. A flux is such that its surface integrals are invariable.

It is understood that the integration domains follow the deformation of the medium. Among the field quantities encountered in this book, the electric charge density ρ and the energy density are densities; the electric force **E**, the magnetic force **H**, and the vector potential **A** are forces; and the electric current **j**, the electric displacement **D**, and the magnetic induction **B** are fluxes.[22]

During an infinitesimal deformation $\delta\mathbf{r}$ of the medium, the variation of a density ρ at a fixed point of space is

$$\delta\rho = -\nabla\cdot(\rho\delta\mathbf{r}), \tag{A.40}$$

That of a force **A** is

$$\delta\mathbf{A} = \delta\mathbf{r}\times(\nabla\times\mathbf{A}) - \nabla(\mathbf{A}\cdot\delta\mathbf{r}), \tag{A.41}$$

and that of a flux **B** is

$$\delta\mathbf{B} = \nabla\times(\delta\mathbf{r}\times\mathbf{B}) - \delta\mathbf{r}(\nabla\cdot\mathbf{B}) \tag{A.42}$$

These theorems can be proved as follows.[23]

The characteristic property of a density is

$$0 = \delta\int_V \rho\mathrm{d}\tau = \int_V \delta\rho\mathrm{d}\tau + \int_{V'} \rho\mathrm{d}\tau - \int_V \rho\mathrm{d}\tau, \tag{A.43}$$

where V and V' are the integration volumes before and after the deformation. As is clear from Fig. A.2, we have

$$\int_{V'} \rho\mathrm{d}\tau - \int_V \rho\mathrm{d}\tau = \int_{\partial V} \rho\delta\mathbf{r}\cdot\mathrm{d}\mathbf{S} = \int_V \nabla\cdot(\rho\delta\mathbf{r})\mathrm{d}\tau, \tag{A.44}$$

where ∂V denoted the surface delimiting V. Since these identities hold for any V, $\delta\rho$ must be as given in eqn. (A.40).

[22] Helmholtz 1892: 491–7.

[23] The formulas for a force and for a flux are implicitly contained in Maxwell 1861: 479–82. However, the first explicit proof is in Maxwell 1873a: #602 for a force (**A**), and in Helmholtz 1874a: 730–4 for a flux. The following proofs are mine (Helmholtz reasoned on infinitesimal elements).

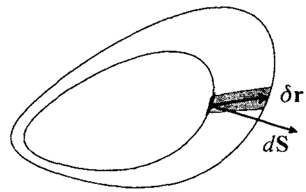

FIG. A.2. Section of a volume deformation. The volume of the shaded element is $\delta\mathbf{r}\cdot d\mathbf{S}$.

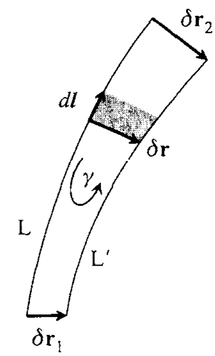

FIG. A.3. Deformation of a line L. The surface of the shaded element is $\delta\mathbf{r}\times d\mathbf{l}$.

The characteristic property of a force is

$$0=\delta\int_L \mathbf{A}\cdot d\mathbf{l}=\int_L \delta\mathbf{A}\cdot d\mathbf{l}+\int_{L'}\mathbf{A}\cdot d\mathbf{l}-\int_L \mathbf{A}\cdot d\mathbf{l}, \tag{A.45}$$

where L and L' are the integration lines before and after the deformation. As is clear from Fig. A.3, we have

$$\int_{L'}\mathbf{A}\cdot d\mathbf{l}-\int_L \mathbf{A}\cdot d\mathbf{l}=\oint_\gamma \mathbf{A}\cdot d\mathbf{l}-\mathbf{A}\cdot\delta\mathbf{r}_1+\mathbf{A}\cdot\delta\mathbf{r}_2=\int_S(\nabla\times\mathbf{A})\cdot(\delta\mathbf{r}\times d\mathbf{l})+\int_L\nabla(\mathbf{A}\cdot\delta\mathbf{r})\cdot d\mathbf{l}, \tag{A.46}$$

where γ is the closed path made of L', $-L$, $\delta\mathbf{r}_1$ and $-\delta\mathbf{r}_2$, and S is the surface delimited by this path. The integrand of the surface integral may be transformed according to

$$(\nabla\times\mathbf{A})\cdot(\delta\mathbf{r}\times d\mathbf{l})=d\mathbf{l}\cdot[(\nabla\times\mathbf{A})\times\delta\mathbf{r}]. \tag{A.47}$$

Consequently, the identity (A.45) will hold for any L if and only if $\delta\mathbf{B}$ is as given in eqn. (A.42).

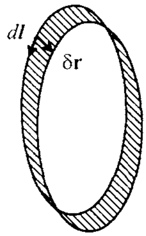

FIG. A.4. Deformation of a surface.

The characteristic property of a flux is

$$0 = \delta \int \mathbf{B} \cdot d\mathbf{S} = \int_S \delta\mathbf{B} \cdot d\mathbf{S} + \int_{S'} \mathbf{B} \cdot d\mathbf{S} - \int_S \mathbf{B} \cdot d\mathbf{S}, \tag{A.48}$$

where S and S' are the integration surfaces before and after the deformation. As is clear from Fig. A.4, we have

$$\int_{S'} \mathbf{B} \cdot d\mathbf{S} - \int_S \mathbf{B} \cdot d\mathbf{S} = \int_\Sigma \mathbf{B} \cdot d\mathbf{S} - \int_{\partial S} \mathbf{B} \cdot (d\mathbf{l} \times \delta\mathbf{r}), \tag{A.49}$$

where ∂S is the line delimiting S, Σ is the closed surface made of S, S', and the trace of ∂S. Calling V the volume delimited by Σ, we further have

$$\oint_\Sigma \mathbf{B} \cdot d\mathbf{S} = \int_V \nabla \cdot \mathbf{B} d\tau = \int_S (\nabla \cdot \mathbf{B}) \delta\mathbf{r} \cdot d\mathbf{S}, \tag{A.50}$$

and

$$\int_{\partial S} \mathbf{B} \cdot (d\mathbf{l} \times \delta\mathbf{r}) = \int_{\partial S} d\mathbf{l} \cdot (\delta\mathbf{r} \times \mathbf{B}) = \int_S [\nabla \times (\delta\mathbf{r} \times \mathbf{B})] \cdot d\mathbf{S}. \tag{A.51}$$

Combining these identities, we reach

$$\int_S \delta\mathbf{B} \cdot d\mathbf{S} + \int_S [\delta\mathbf{r}(\nabla \cdot \mathbf{B}) - \nabla \times (\delta\mathbf{r} \times \mathbf{B})] \cdot d\mathbf{S} = 0. \tag{A.52}$$

This will hold for any surface S if and only if the formula (A.42) holds.

Derivatives

Consider a moving medium, with the velocity **v**. By definition, the convective derivative of a quantity is its time derivative with respect to a fixed point of the medium. It is equal to the derivative with respect to a fixed point of space (that is, the partial derivative $\partial/\partial t$) minus the rate of variation of the quantity owing to the deformation of the medium. Hence, the convective derivative of a density is

$$\frac{D\rho}{Dt} = \frac{\partial \rho}{\partial t} + \nabla \cdot (\rho \mathbf{v}). \tag{A.53}$$

That of a force is

$$\frac{D\mathbf{A}}{Dt} = \frac{\partial \mathbf{A}}{\partial t} - \mathbf{v} \times (\nabla \times \mathbf{A}) + \nabla(\mathbf{v} \cdot \mathbf{A}). \tag{A.54}$$

And that of a flux is

$$\frac{D\mathbf{B}}{Dt} = \frac{\partial \mathbf{B}}{\partial t} - \nabla \times (\mathbf{v} \times \mathbf{B}) + \mathbf{v}(\nabla \cdot \mathbf{B}). \tag{A.55}$$

Appendix 6
Maxwell's stress system

In conformity with Faraday's idea of a tension along and a mutual repulsion of the lines of force, Maxwell derived the mechanical forces of electric and magnetic origin from the stress system[24]

$$\sigma_{ij} = D_i E_j - \frac{1}{2}\delta_{ij}\mathbf{D}\cdot\mathbf{E} + B_i H_j - \frac{1}{2}\delta_{ij}\mathbf{B}\cdot\mathbf{H}. \quad \text{(A.56)}$$

For isotropic media the resulting force density is $\partial_i \sigma_{ij}$ in tensor notation, or

$$\mathbf{f} = \mathbf{E}(\nabla\cdot\mathbf{D}) + (\nabla\times\mathbf{H})\times\mathbf{B} + (\nabla\times\mathbf{E})\times\mathbf{D} - \frac{1}{2}\mathbf{E}\cdot\mathbf{D}\frac{\nabla\varepsilon}{\varepsilon} - \frac{1}{2}\mathbf{B}\cdot\mathbf{H}\frac{\nabla\mu}{\mu} \quad \text{(A.57)}$$

in vector notation. For a homogenous medium at rest, this amounts to

$$\mathbf{f} = \rho\mathbf{E} + \mathbf{j}\times\mathbf{B} + \frac{\partial}{\partial t}(\mathbf{D}\times\mathbf{B}). \quad \text{(A.58)}$$

The first part is the electric force deriving from Maxwell's definition of the electric field $\mathbf{E}$.[25] The second, together with $(\partial\mathbf{D}/\partial t)\times\mathbf{B}$, is the electrodynamic force resulting from Maxwell's dynamical reasoning. Heaviside added the very small 'magnetoelectric force' $\mathbf{D}\times(\partial\mathbf{B}/\partial t)$, which is necessary to the energy balance if the 'magnetic motional force' $\mathbf{D}\times\mathbf{v}$ is included in the magnetic circuital equation.[26]

More generally, Hertz and Heaviside showed that in a complete, Maxwellian electrodynamics of moving body, the conservation of energy implied that the mechanical forces of electric and magnetic origin derived from Maxwell's stress system. Heaviside's general field equations are

$$\begin{aligned} \nabla\times(\mathbf{E}-\mathbf{e}_0) &= -\mathbf{G}, \\ \nabla\times(\mathbf{H}-\mathbf{h}_0) &= \mathbf{J}, \end{aligned} \quad \text{(A.59)}$$

where $\mathbf{e}_0$ and $\mathbf{h}_0$ are impressed forces, $\mathbf{G}$ is the total 'magnetic current,' and $\mathbf{J}$ is the total electric current. These currents are defined as

$$\begin{aligned} \mathbf{G} &= \frac{\mathrm{D}\mathbf{B}}{\mathrm{D}t}, \\ \mathbf{J} &= \mathbf{j} + \frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t}, \end{aligned} \quad \text{(A.60)}$$

[24] Maxwell 1861: 456 for the magnetic stress system; Maxwell 1873a: #105–11 for the electric stress system.

[25] The definition is in Maxwell 1873a: #68. However, *ibid.*: #619, Maxwell mistakenly wrote $-\rho\nabla\phi$ instead of $\mathbf{E}$. FitzGerald 1883b corrected him.

[26] Heaviside 1885–7: 546; 1886–7: 175.

where D/Dt is the convective derivative (A.55) of a flux. The activity of impressed forces is

$$\mathbf{e}_0 \cdot \mathbf{J} + \mathbf{h}_0 \cdot \mathbf{G} = (\mathbf{e}_0 - \mathbf{E}) \cdot \mathbf{J} + (\mathbf{h}_0 - \mathbf{H}) \cdot \mathbf{G} + \mathbf{E} \cdot \mathbf{J} + \mathbf{H} \cdot \mathbf{G}. \tag{A.61}$$

In the two first terms of the right-hand side, we replace **G** and **J** with the curls (A.59). In the two last terms, we replace them with the definitions (A.60). Developing the convective derivatives and regrouping terms, we reach

$$\mathbf{e}_0 \cdot \mathbf{J} + \mathbf{h}_0 \cdot \mathbf{G} = \frac{\partial w}{\partial t} + \nabla \cdot (\mathbf{\Pi} + \mathbf{u} \cdot \mathbf{v}) + \mathbf{j} \cdot \mathbf{E} - \sigma_{ij} \partial_i v_j, \tag{A.62}$$

where

$$w = \frac{1}{2} \mathbf{E} \cdot \mathbf{D} + \frac{1}{2} \mathbf{B} \cdot \mathbf{H} \tag{A.63}$$

is the field energy density,

$$\mathbf{\Pi} = (\mathbf{E} - \mathbf{e}_0) \times (\mathbf{H} - \mathbf{h}_0) \tag{A.64}$$

is the Poynting vector, and σ_{ij} is Maxwell's stress system (A.56). In words, the activity of impressed forces in a unit time is equal to the variation of the field energy plus the outward flux of the energy current (which involves a convective component), plus the Joule heat developed in the conductors, plus the work of Maxwell's stresses.[27]

[27] Hertz 1890b: 275–85; Heaviside 1891–1892. The derivation given here borrows the convective derivative from Hertz, and the general form of the balance from Heaviside. Cf. Darrigol 1993b: 321–7.

Appendix 7
Helmholtz's electrodynamics

In order to save writing, the $*$ sign is used to denote the convolution product of two functions:

$$f*g(\mathbf{r}) = \int f(\mathbf{r}')g(\mathbf{r}-\mathbf{r}')\mathrm{d}\tau'. \quad \text{(A.65)}$$

Electrostatic units are used, in conformity with Helmholtz's choice. In the main text electromagnetic units were used to avoid the abundance of c's in the electrodynamic equations. However, electrostatic units are more advantageous when dealing with the relation between Helmholtz's polarization theory and Maxwell's theory.

Helmholtz's theory is based on two potentials. The electrostatic potential ϕ is defined as

$$\phi = \rho * \frac{1}{r}, \quad \text{(A.66)}$$

where ρ is the charge density. The electrodynamic potential $\mathbf{A}$ is defined as

$$\mathbf{A} = \frac{1}{c}\mathbf{j} * \frac{1}{r} + \frac{1-k}{2}\nabla\xi, \quad \text{(A.67)}$$

where $\mathbf{j}$ is the conduction current and

$$\xi = -\frac{1}{c}\mathbf{j} * \nabla r = -\frac{1}{c}(\nabla\cdot\mathbf{j}) * r. \quad \text{(A.68)}$$

For $k = 1$, the total electrodynamic potential,

$$P = -\frac{1}{2c}\int \mathbf{j}\cdot\mathbf{A}\mathrm{d}\tau, \quad \text{(A.69)}$$

is a staightforward extension of Neumann's potential formula (A.18).

For $k = -1$ the potential $\mathbf{A}$ is that (eqn. A.36) derived by Kirchhoff from Weber's theory. When all currents are closed ($\nabla\cdot\mathbf{j} = 0$), all choices of the electrodynamic potential become equivalent since the function ξ vanishes.[28]

The mechanical forces of electrostatic origin have the volume density

$$\mathbf{f}_s = -\rho\nabla\phi. \quad \text{(A.70)}$$

Those of electrodynamic origin have the density $\mathbf{f}_d$ satisfying

$$-\int \mathbf{f}_d\cdot\delta\mathbf{r} = \delta P = -\frac{1}{c}\int \mathbf{A}\cdot\delta\mathbf{j}, \quad \text{(A.71)}$$

[28] Helmholtz 1870b: 568–9.

for any deformation $\delta\mathbf{r}$ of the material medium. Based on Appendix 5, the variation $\delta\mathbf{j}$ of the flux $\mathbf{j}$ is

$$\delta\mathbf{j} = \nabla\times(\delta\mathbf{r}\times\mathbf{j}) - (\nabla\cdot\mathbf{j})\delta\mathbf{r}. \tag{A.72}$$

Injecting this expression into the previous formula and integrating the contribution of the first term by parts, we get[29]

$$\mathbf{f}_d = \frac{1}{c}\mathbf{j}\times(\nabla\times\mathbf{A}) - \frac{1}{c}\mathbf{A}(\nabla\cdot\mathbf{j}). \tag{A.73}$$

The motion of electricity is determined by Ohm's law

$$\mathbf{j} = \sigma\mathbf{E} \tag{A.74}$$

and the expression

$$\mathbf{E} = -\nabla\phi - \frac{1}{c}\frac{\mathrm{D}\mathbf{A}}{\mathrm{D}t} + \mathbf{E}_0 \tag{A.75}$$

of the electromotive force, where $\mathbf{E}_0$ is the external electromotive force (of chemical origin for instance) and *D/Dt* denotes the convective derivative (A.54) of a force. This motion is further constrained by the conservation of electricity,

$$\nabla\cdot\mathbf{j} + \frac{\mathrm{D}\rho}{\mathrm{D}t} = \nabla\cdot(\mathbf{j}+\rho\mathbf{v}) + \frac{\partial\rho}{\partial t} = 0, \tag{A.76}$$

where $\mathbf{v}$ denotes the velocity of matter.[30]

Energy conservation

The work provided to the system by the external electromotive source in a unit time is, using Ohm's law,

$$\int\mathbf{j}\cdot\mathbf{E}_0\mathrm{d}\tau = \int\frac{j^2}{2\sigma} + \int\mathbf{j}\cdot\nabla\phi\mathrm{d}\tau + \frac{1}{c}\int\mathbf{j}\cdot\frac{\mathrm{D}\mathbf{A}}{\mathrm{D}t}\mathrm{d}\tau. \tag{A.77}$$

The first integral represents the Joule heat. Integrating the second by parts and using eqn. (A.76) for the conservation of electricity, we get

$$\int\mathbf{j}\cdot\nabla\phi\mathrm{d}\tau = -\int\phi(\nabla\cdot\mathbf{j})\mathrm{d}\tau = \int\phi\frac{\partial\rho}{\partial t} + \int\phi\nabla\cdot(\rho\mathbf{v})\mathrm{d}\tau = \frac{1}{2}\frac{\mathrm{d}}{\mathrm{d}t}\int\rho\phi\mathrm{d}\tau + \int\mathbf{v}\cdot\mathbf{f}_s\mathrm{d}\tau. \tag{A.78}$$

Here we recognize the sum of the time variation of the electrostatic energy and the work of electrostatic forces. Using the definition of the convective derivative, the third integral of eqn. (A.77) becomes

[29] Helmholtz 1874a: 730–4. [30] Helmholtz 1870b, 1874a: 742–5 (for moving bodies).

$$\frac{1}{c}\int \mathbf{j}\cdot\frac{\mathrm{D}\mathbf{A}}{\mathrm{D}t}\mathrm{d}\tau=\frac{1}{c}\int \mathbf{j}\cdot\frac{\partial \mathbf{A}}{\partial t}\mathrm{d}\tau-\frac{1}{c}\int \mathbf{j}\cdot\frac{\delta \mathbf{A}}{\delta t}\mathrm{d}\tau=\frac{1}{2c}\frac{\mathrm{d}}{\mathrm{d}t}\int \mathbf{j}\cdot\mathbf{A}\mathrm{d}\tau+\frac{1}{c}\int \mathbf{A}\cdot\frac{\delta \mathbf{j}}{\delta t}\mathrm{d}\tau. \tag{A.79}$$

The last integral is equal to the work performed by the mechanical forces of electrodynamic origin, as can be seen by dividing eqn. (A.71) by δt. The penultimate integral is equal to $-dP/dt$, where P is the total potential (A.69). If, following Helmholtz, we identify $-P$ with the electrodynamic energy and if we put together the interpretations of the free terms of eqn. (A.77), we obtain the following statement of the conservation of energy: the energy furnished by the external sources is equal to the Joule heat, plus the increase of the electrostatic and electrodynamic energies of the system, plus the work of the forces of electrostatic and electrodynamic origins.[31]

Comparison with Maxwell's equations, in absence of polarization

Taking the divergence of eqn. (A.67), we get

$$\nabla\cdot\mathbf{A}=\frac{1}{c}(\nabla\cdot\mathbf{j})*\frac{1}{r}+\frac{1-k}{2}\Delta\xi. \tag{A.80}$$

The conservation of electricity (A.76) and the expression (A.68) of ξ yield

$$\Delta\xi=-\frac{1}{c}(\nabla\cdot\mathbf{j})*\Delta r=\frac{2}{c}\frac{\mathrm{D}\rho}{\mathrm{D}t}*\frac{1}{r}. \tag{A.81}$$

Consequently, we have

$$\nabla\cdot\mathbf{A}=-\frac{k}{c}\frac{\mathrm{D}\rho}{\mathrm{D}t}*\frac{1}{r}. \tag{A.82}$$

Taking the Laplacian of eqn. (A.67), we get

$$\Delta\mathbf{A}=-\frac{4\pi}{c}\mathbf{j}+\frac{1-k}{2}\nabla\Delta\xi=-\frac{4\pi}{c}\mathbf{j}+\frac{1-k}{c}\frac{\mathrm{D}\rho}{\mathrm{D}t}*\nabla\frac{1}{r}. \tag{A.83}$$

Consequently, we have

$$\nabla\times(\nabla\times\mathbf{A})=\nabla(\nabla\cdot\mathbf{A})-\Delta\mathbf{A}=\frac{4\pi}{c}\mathbf{j}-\frac{1}{c}\frac{\mathrm{D}\rho}{\mathrm{D}t}*\nabla\frac{1}{r}. \tag{A.84}$$

When all bodies are at rest, the latter equation may be rewritten as

$$\nabla\times(\nabla\times\mathbf{A})=\frac{4\pi}{c}\mathbf{j}-\frac{1}{c}\frac{\partial\nabla\phi}{\partial t}, \tag{A.85}$$

[31] Helmholtz 1874a: 748–51. Helmholtz did not include the motion of electrified bodies in his analysis. If there is such motion, energy is conserved only if the convection currents are electrodynamically inactive. This issue is related to the unity of the electric force: cf. Darrigol 1993b: 340–3.

or, in the absence of external electromotive forces,

$$\nabla\times(\nabla\times\mathbf{A})-\frac{1}{c^2}\frac{\partial^2\mathbf{A}}{\partial t^2}=\frac{4\pi}{c}\mathbf{j}+\frac{1}{c}\frac{\partial\mathbf{E}}{\partial t}. \tag{A.86}$$

Using eqn. (A.82), the divergence of the electromotive force (A.75) reads

$$\nabla\cdot\mathbf{E}=4\pi\rho+\frac{k}{c^2}\frac{\partial^2\phi}{\partial t^2}. \tag{A.87}$$

Its curl is

$$\nabla\times\mathbf{E}=-\frac{1}{c}\frac{\partial}{\partial t}\nabla\times\mathbf{A}. \tag{A.88}$$

These equations have some resemblance with the Maxwell–Hertz equations in the case of constant and uniform ε and μ. However, they still contain the potentials ϕ and $\mathbf{A}$ besides the forces $\mathbf{E}$ and $\nabla\times\mathbf{A}$. Equation (A.86) differs from Maxwell's expression of the Ampère theorem by a term $\partial^2\mathbf{A}/c^2\partial t^2$. Call l and τ the scales of the space and time variations of $\mathbf{A}$. This extra term becomes appreciable when l/τ is comparable to the velocity of light. Similarly, eqn. (A.87) departs from Maxwell's corresponding equation when $k^{1/2}l/\tau$ is comparable to the velocity of light. As a corollary, the choice of k has negligible empirical consequences, as long as Hertzian frequencies are not reached. Helmholtz saw this in specific examples.[32]

The resemblance between the predictions of Maxwell's and Helmholtz's theories is even greater when $k=0$. In this case, the divergence of $\mathbf{E}$ is the same as in Maxwell's theory, so that no charge can subside within a conductor in a time appreciably longer than σ, which is about 10^{-17}s for copper. Consequently, for an infinite conductor the term $-\partial\nabla\phi/c\partial t$ in eqn. (A.85) quickly vanishes. In Maxwell's corresponding equation, this term is replaced with the displacement current. The latter is very small compared to the conduction current, as long as the variation of the electric field in the time σ is negligible. Hence, in an infinite conductor the consequences of Maxwell's theory are the same as those of the potential theory for $k=0$, up to frequencies nearly as high as optical frequencies.[33]

This is no longer true if the conductor is finite, in which case the effect of surface charges may be felt. Consider for instance a double-plate air condenser. Maxwell's displacement current between the plates is equal to the conduction current in the metal of the plates. In Helmholtz's theory, the open-ended conduction current is formally closed by a pseudo-current $-\partial\nabla\phi/c\partial t$ in the air. This pseudo-current differs from Maxwell's displacement current by $\partial^2\mathbf{A}/c^2\partial t^2$, which becomes appreciable when l/τ is of the same order as c. Although such rapid variations never occur in usual condensers, they do at Hertzian frequencies, which are much lower than optical frequencies.

A far more important difference in the empirical predictions of Helmholtz's and Maxwell's theory concerns the mechanical forces of electrodynamic origins. The second term of Helmholtz's formula (A.73) implies an action on the extremity of open currents that does not

[32] Helmholtz 1870b: 599–611. The general reasoning is mine.
[33] See Helmholtz 1870b: 588, 578, 603.

exist in Maxwell's theory (nor in any other known theory). Correlatively, Helmholtz's expression (A.75) for the electromotive force in a moving body differs from Maxwell's by a term $-(1/c)\nabla(\mathbf{v}\cdot\mathbf{A})$. The electrokinetic energy developed by this force is exactly opposed to the kinetic energy gained by the conductors under the effect of the open-end forces, for we have

$$\int -\mathbf{j}\cdot\nabla(\mathbf{v}\cdot\mathbf{A})\mathrm{d}\tau = \int \mathbf{v}\cdot\mathbf{A}(\nabla\cdot\mathbf{j})\mathrm{d}\tau. \quad \text{(A.89)}$$

The new electromotive force has no effect in closed circuits. In open circuits, it contributes to the charges accumulated at the end of the circuit. In electrostatic units the order of magnitude of these charges is v/c for usual magnetic fields. As explained in the main text, Helmholtz managed to test these feeble charges and thus to exclude the potential theory without vacuum polarization.[34]

Polarization waves

In a polarizable medium submitted to the electromotive forces of electrostatic and electrodynamic origin, Helmholtz assumes the polarization

$$\mathbf{P} = \frac{\kappa}{4\pi}\mathbf{E}. \quad \text{(A.90)}$$

This polarization implies a net charge density

$$\rho_P = -\nabla\cdot\mathbf{P}, \quad \text{(A.91)}$$

A variation in time of the polarization implies the current[35]

$$\mathbf{j}_P = \frac{\mathrm{D}\mathbf{P}}{\mathrm{D}t}. \quad \text{(A.92)}$$

Consider an infinite dielectric with a constant, uniform polarizability. Taking the divergence of (A.90) and using the expression (A.87) of $\nabla\cdot\mathbf{E}$, we get

$$(1+\kappa)\nabla\cdot\mathbf{P} = \frac{k\kappa}{4\pi c^2}\frac{\partial^2\phi}{\partial t^2}. \quad \text{(A.93)}$$

Taking the Laplacian of this equation, and using the expression (A.66) of ϕ, we reach

$$\left(\frac{k\kappa}{c^2}\frac{\partial^2}{\partial t^2} - (1+\kappa)\Delta\right)\nabla\cdot\mathbf{P} = 0, \quad \text{(A.94)}$$

which implies the possibility of longitudinal polarization waves, with the velocity

$$c_{\parallel} = c\sqrt{\frac{1+\kappa}{k\kappa}}. \quad \text{(A.95)}$$

[34] Helmholtz 1873a, 1873b, 1874a, 1875a. [35] Helmholtz 1870b: 611–28.

Taking the curl of the curl of eqn. (A.90), and using the expression (A.75) of **E** and eqn. (A.85), we get

$$\nabla\times(\nabla\times\mathbf{P})=-\frac{\kappa}{c^2}\frac{\partial^2\mathbf{P}}{\partial t^2}+\frac{\kappa}{4\pi c^2}\frac{\partial}{\partial t}\nabla\phi. \tag{A.96}$$

Taking again the curl of this equation, we reach

$$\left(\frac{\kappa}{c^2}\frac{\partial^2}{\partial t^2}-\Delta\right)\nabla\times\mathbf{P}=\mathbf{0}, \tag{A.97}$$

which implies the possibility of transverse polarization waves with the velocity

$$c_\perp=\frac{c}{\sqrt{\kappa}}. \tag{A.98}$$

As is explained in Chapter 6, these formulas, when applied to an infinitely polarizable vacuum, lead to a purely transverse propagation, with a velocity equal to the ratio of the electromagnetic to the electrostatic unit of charge (taking into account the renormalization of charge).[36]

The Maxwell–Hertz equations derived

We now assume that vacuum itself is polarizable, with the polarizability κ_0. In matter the polarizability is $\kappa=\varepsilon\kappa_0$, the coefficient ε being larger than unity. In the sources of Helmholtz's potentials, we must now add the polarization charge (A.91) and current (A.92) to the ordinary charge and currents ρ and $\mathbf{j}$. Hence eqn. (A.84) becomes

$$\nabla\times\mathbf{B}=\frac{4\pi}{c}\left(\mathbf{j}+\frac{\mathrm{D}\mathbf{P}}{\mathrm{D}t}\right)-\frac{1}{c}\left(\frac{\mathrm{D}}{\mathrm{D}t}(\rho-\nabla\cdot\mathbf{P})\right)*\nabla\frac{1}{r}, \tag{A.99}$$

with $\mathbf{B}=\nabla\times\mathbf{A}$ and

$$\nabla\cdot\mathbf{B}=0. \tag{A.100}$$

Taking the curl of the electromotive force (A.75) (with $\mathbf{E}_0=\mathbf{0}$) gives

$$\nabla\times\mathbf{E}=-\frac{1}{c}\frac{\mathrm{D}\mathbf{B}}{\mathrm{D}t}. \tag{A.101}$$

Taking its divergence gives

$$\nabla\cdot(1+\kappa)\mathbf{E}=4\pi\rho-\frac{1}{c}\nabla\cdot\frac{\mathrm{D}\mathbf{A}}{\mathrm{D}t}. \tag{A.102}$$

[36] Helmholtz 1870b: 626–8.

In these four equations the various quantities are measured with reference to a fictitious electrostatic unit of charge, obtained by doing as if vacuum were not polarizable. The true electrostatic unit of charge is such that a unit force acts between two unit point charges separated by the unit distance in the polarizable vacuum. As is seen from eqn. (A.102), the force in the polarizable vacuum is $(1 + \kappa_0)$ times smaller than in a non-polarizable medium. Therefore, the true unit of charge is γ times larger than the fictitious unit, with

$$\gamma = \sqrt{1+\kappa_0}. \tag{A.103}$$

When referred to the true electrostatic unit, the various electric and magnetic quantities become

$$\rho' = \gamma^{-1}\rho, \quad \mathbf{E}' = \gamma\mathbf{E}, \quad \phi' = \gamma\phi, \quad c' = \gamma^{-1}c, \quad \mathbf{j}' = \gamma^{-1}\mathbf{j}, \quad \mathbf{B}' = \mathbf{B}. \tag{A.104}$$

In terms of these renormalized quantities, the equations (A.99)–(A.102) become

$$\nabla\times\mathbf{B}' = \frac{4\pi}{c'}\left(\mathbf{j}' + \frac{\mathrm{D}}{\mathrm{D}t}\frac{\kappa_0\varepsilon\mathbf{E}'}{1+\kappa_0}\right) - \frac{1}{c'}\left[\frac{\mathrm{D}}{\mathrm{D}t}\left(\rho' - \frac{\kappa_0}{1+\kappa_0}\nabla\cdot\varepsilon\mathbf{E}'\right)\right] * \nabla\frac{1}{r}, \tag{A.105}$$

$$\nabla\cdot\mathbf{B}' = 0. \tag{A.106}$$

$$\nabla\times\mathbf{E}' = -\frac{1}{c'}\frac{\mathrm{D}\mathbf{B}'}{\mathrm{D}t}, \tag{A.107}$$

and

$$\nabla\cdot\frac{1+\varepsilon\kappa_0}{1+\kappa_0}\mathbf{E}' = 4\pi\rho' - \frac{\gamma^{-2}}{c'}\nabla\cdot\frac{\mathrm{D}\mathbf{A}'}{\mathrm{D}t}. \tag{A.108}$$

In the limit of infinite κ_0 and γ, the fourth of these equations goes to

$$\nabla\cdot(\varepsilon\mathbf{E}') = 4\pi\rho'. \tag{A.109}$$

Consequently, the last term of the first equation goes to zero, and we are left with

$$\nabla\times\mathbf{B}' = \frac{4\pi}{c'}\left(\mathbf{j}' + \frac{\mathrm{D}\varepsilon\mathbf{E}'}{\mathrm{D}t}\right). \tag{A.110}$$

In sum, we retrieve the Maxwell–Hertz equations for the electrodynamics of moving bodies (for $\mu = 1$).[37]

It remains to be seen whether this convergence extends to the ponderomotive forces. For simplicity, we only consider the case of a uniform ε. Then the force acting on a volume

[37] This proof is mine. In print Helmholtz only treated particular cases (pure dielectric in Helmholtz 1870b: 611–28, and rotating condenser in Helmholtz 1875a: 788–90 (cf. Darrigol 1993a: 343–6). The manuscript #622 in the archive of the Berlin Akademie der Wissenschaften, probably written around 1875, contains the equations for a full electromagnetic of moving bodies in polarizable media. The equivalence proof of Poincaré 1891: Ch. 5 covers only the case of bodies at rest, and it relies on a concept of polarization different from Helmholtz's.

element of the medium has three parts, corresponding to the three kinds of energy stored in the medium. The electrostatic part is

$$\mathbf{f}_s = -(\rho - \nabla \cdot \mathbf{P})\nabla\phi. \tag{A.111}$$

The electrodynamic part is (compare with eqn. (A.73))

$$\mathbf{f}_d = \frac{1}{c}\left(\mathbf{j} + \frac{\mathrm{D}\mathbf{P}}{\mathrm{D}t}\right)\times\mathbf{B} + \frac{1}{c}\mathbf{A}\frac{\mathrm{D}}{\mathrm{D}t}(\rho - \nabla\cdot\mathbf{P}). \tag{A.112}$$

The polarization part derives from the energy $(1/2)\int \mathbf{P}\cdot\mathbf{E}\,\mathrm{d}\tau$. By analogy with the electrodynamic force (A.73), which derives from $-(1/2)\int \mathbf{j}\cdot\mathbf{A}\,\mathrm{d}\tau$, we get

$$\mathbf{f}_P = \mathbf{E}(\nabla\cdot\mathbf{P}) - \mathbf{P}\times(\nabla\times\mathbf{E}). \tag{A.113}$$

After renormalization, and for an infinite vacuum polarizability, the sum of these forces goes to

$$\mathbf{f} = \rho'\mathbf{E}' + \frac{1}{4\pi}(\nabla\times\mathbf{B}')\times\mathbf{B}' + \frac{1}{4\pi}(\nabla\times\mathbf{E}')\times\varepsilon\mathbf{E}', \tag{A.114}$$

which is exactly the force resulting from Maxwell's stress system.

Appendix 8
Hertz's 1884 derivation of the Maxwell equations

Consider a system of closed currents in any of the continental theories of electrodynamics, which are equivalent for this purpose. Call $\mathbf{A}$ the corresponding electrodynamic potential and $\mathbf{B}$ its curl. According to Hertz, the unity of electric force requires a mechanical force between the 'magnetic currents' $\partial\mathbf{B}/\partial t$ (see pp. 236–9). This force is to magnetic currents what Ampère's forces are to electric currents. More exactly, $(-1/c)(\partial\mathbf{B}/\partial t)$ is the counterpart of $(4\pi/c)\mathbf{j}$, since one is the curl of $\mathbf{E}$ while the other is the curl of $\mathbf{H}$. Therefore, Hertz reasoned, the new force derives from the potential (in electrostatic units)

$$p=\frac{1}{32\pi^2c^2}\iint\frac{\dot{\mathbf{B}}(\mathbf{r})\cdot\mathbf{B}(\mathbf{r}')}{|\mathbf{r}-\mathbf{r}'|}\mathrm{d}\tau\mathrm{d}\tau'. \qquad \text{(A.115)}$$

Imitating Helmholtz's 'derivation' of electromagnetic induction from energy conservation, Hertz deduced from the new potential the existence of a 'magnetoelectric' induction. In symbols, the corresponding magnetomotive force is

$$\mathbf{H}'=-\frac{1}{c}\frac{\partial\mathbf{a}}{\partial t}, \qquad \text{(A.116)}$$

with

$$\mathbf{a}=-\frac{1}{4\pi c}\dot{\mathbf{B}}*\frac{1}{r}. \qquad \text{(A.117)}$$

To this new magnetic force corresponds a new magnetic flux $\mathbf{B}'$, and therefore a new electromotive force of induction $\mathbf{E}'$ through Faraday's law. For $\mu=1$, we have

$$\mathbf{B}'=\nabla\times\mathbf{A}',\quad \mathbf{E}'=-\frac{1}{c}\frac{\partial\mathbf{A}'}{\partial t}, \qquad \text{(A.118)}$$

with

$$\mathbf{A}'=-\frac{1}{4\pi c^2}\frac{\partial^2\mathbf{A}}{\partial t^2}*\frac{1}{r}. \qquad \text{(A.119)}$$

In sum, from the electric and magnetic forces deriving from the usual potential $\mathbf{A}_0$, we have switched to an improved system of forces deriving from the potential $\mathbf{A}_1=\mathbf{A}_0+\mathbf{A}'$. A better approximation results from taking $\mathbf{A}_1$ instead of $\mathbf{A}_0$ as the basis for the calculation of $\mathbf{A}'$. The nth iteration gives

$$\mathbf{A}_{n+1}=\mathbf{A}_0-\frac{1}{4\pi c^2}\frac{\partial^2\mathbf{A}_n}{\partial t^2}*\frac{1}{r}. \qquad \text{(A.120)}$$

If the sequence converges (as Hertz verified on a specific example), the limit $\mathbf{A}$ satisfies

$$\mathbf{A} = \mathbf{A}_0 - \frac{1}{4\pi c^2}\frac{\partial^2 \mathbf{A}}{\partial t^2} * \frac{1}{r}. \tag{A.121}$$

Applying the Laplacian Δ to both members of this equation yields the differential equation

$$\Delta \mathbf{A} = -\frac{4\pi}{c}\mathbf{j} + \frac{1}{c^2}\frac{\partial^2 \mathbf{A}}{\partial t^2}. \tag{A.122}$$

For the forces **E** and **H** deriving from **A**, this gives

$$\nabla \times \mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{H}}{\partial t},$$
$$\nabla \times \mathbf{H} = \frac{1}{c}\frac{\partial \mathbf{E}}{\partial t} + \frac{4\pi}{c}\mathbf{j}, \tag{A.123}$$

in conformity with Maxwell's equations for $\varepsilon = 1$ and $\mu = 1$.[38]

[38] Hertz 1884: 302–11. Hertz does not include **j** in the final equations.

Appendix 9
Electrodynamic Lagrangians

The electric current as a generalized velocity

Maxwell gives the electrodynamic Lagrangian of a system of closed linear currents i_α as

$$T=\frac{1}{2}\sum_{\alpha\beta} M_{\alpha\beta} i_\alpha i_\beta, \tag{A.124}$$

where the $M_{\alpha\beta}$ are coefficients depending on the spatial configuration of the circuits. The electromotive force of induction around each circuit, and the mechanical forces of electrodynamic origin are given by the Lagrange equations corresponding respectively to the generalized velocities i_α and to the spatial configuration variables of the circuits.[39]

Lorentz and Helmholtz both provided the three-dimensional generalization of Maxwell's dynamical reasoning. For the divergenceless current $\mathbf{J}$, the effective electrodynamic Lagrangian reads (in electromagnetic units):

$$T=\frac{1}{2}\int \mathbf{J}\cdot\mathbf{A}\,\mathrm{d}\tau+\int \xi\nabla\cdot\mathbf{J}\,\mathrm{d}\tau, \tag{A.125}$$

where $\mathbf{A}$ is a potential vector such that

$$\nabla\times\frac{\nabla\times\mathbf{A}}{\mu}=\mathbf{J}, \tag{A.126}$$

and ξ is the Lagrange parameter corresponding to the constraint $\nabla\cdot\mathbf{J}=0$. The electromotive force of induction is the generalized inertial force corresponding to the generalized velocity $\mathbf{J}$:

$$\mathbf{E}=-\frac{\mathrm{D}}{\mathrm{D}t}\frac{\delta L}{\delta\mathbf{J}}=-\frac{\mathrm{D}}{\mathrm{D}t}(\mathbf{A}-\nabla\xi), \tag{A.127}$$

where the δ's denote a functional derivative, and the D's a convective derivative. Eliminating the Lagrange parameter and introducing $\mathbf{B}=\nabla\times\mathbf{A}$, we get

$$\nabla\times(\mathbf{E}-\mathbf{v}\times\mathbf{B})=-\frac{\partial\mathbf{B}}{\partial t}, \tag{A.128}$$

which is the Maxwell–Hertz form of Faraday's induction law.[40]

The mechanical forces are obtained by writing the Lagrange equations corresponding to a deformation of the current carriers. Consequently, $-T$ plays the role of Neumann's potential, and we may use eqn. (A.73) to obtain

[39] Maxwell 1873a: #578–83.
[40] Lorentz 1892a: 173–88, 206–27 (Lorentz directly used d'Alembert's principle); Helmholtz 1892.

$$\mathbf{f} = \mathbf{J} \times (\nabla \times \mathbf{A}) \tag{A.129}$$

for the force density in the case of uniform μ.

The Larmor force

It is essential, in the previous calculation, to take into account the physical nature of **J** as flux and to use the expression (A.42) for the variation of **J** under a deformation of the current carrier. Larmor mistakenly treated **J** like a density. The resulting variation,

$$\delta \mathbf{J} = -\mathbf{J}(\nabla \cdot \delta \mathbf{r}) - (\delta \mathbf{r} \cdot \nabla)\mathbf{J}, \tag{A.130}$$

differs from the correct one by $-(\mathbf{J} \cdot \nabla)\delta\mathbf{r}$. To this change corresponds a contribution

$$\int \mathbf{A} \cdot (-\mathbf{J} \cdot \nabla)\delta \mathbf{r} \mathrm{d}\tau = \int \delta \mathbf{r} \cdot (\mathbf{J} \cdot \nabla)\mathbf{A} \mathrm{d}\tau \tag{A.131}$$

to the variation of T, which yields the Larmor force $(\mathbf{J} \cdot \nabla)\mathbf{A}$.[41]

The electric displacement as a generalized coordinate

As Helmholtz showed in 1892, in the case of an insulator Maxwell's dynamical reasoning can be completed to include the potential energy of the electric field. The corresponding Lagrangian is

$$L = \frac{1}{2}\int \mathbf{A} \cdot \frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t} \mathrm{d}\tau - \int \frac{D^2}{2\varepsilon} \mathrm{d}\tau + \int \xi \nabla \cdot \frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t} \mathrm{d}\tau, \tag{A.132}$$

where **A** is such that

$$\nabla \times \frac{\nabla \times \mathbf{A}}{\mu} = \frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t}, \tag{A.133}$$

and ξ is the Lagrange parameter corresponding to the constraint that the total current $\mathrm{D}\mathbf{D}/\mathrm{D}t$ is closed. Varying with respect to **D** yields

$$\frac{\mathbf{D}}{\varepsilon} = -\frac{\mathrm{D}}{\mathrm{D}t}(\mathbf{A} - \nabla \xi), \tag{A.134}$$

the curl of which is Faraday's induction law in the Maxwell–Hertz form.[42]

It may be wondered why Maxwell and his British disciples never used this Lagrangian. A plausible reason is that they required that the Lagrangian coordinates should correspond to the positions of the particles of some fluid or solid. It cannot be so for **D**: the assumption would lead to the continuity of **D** at the border between two different dielectrics and thus contradict Fresnel's laws of refraction.

In any case, this Lagrangian is not really an improvement over Maxwell's, because it does

[41] Larmor 1894: 529; 1895b: 546–50. [42] Helmholtz 1892.

not allow for conduction currents. For this reason Helmholtz switched to another variational procedure. As he knew, the Lagrange equations corresponding to the Lagrangian $L(q, \dot{q})$ are equivalent to the canonical equations obtained from varying the time integral of the function $\Phi = H(q, p) - p\dot{q}$ with respect to p and q, if H is the Hamiltonian function corresponding to L. In the present problem, the generalized coordinates of the system are the values which the displacement **D** takes at different points of the medium, and the conjugate momenta are the values of the vector potential **A**. Hence the function Φ reads

$$\Phi = \int \frac{1}{2\mu}(\nabla \times \mathbf{A})^2 \mathrm{d}\tau + \int \frac{D^2}{2\varepsilon} \mathrm{d}\tau - \int \mathbf{A} \cdot \frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t} \mathrm{d}\tau. \tag{A.135}$$

The variables **A** and **D** being here independent, no constraint needs to be *a priori* imposed on $\mathrm{D}\mathbf{D}/\mathrm{D}t$ and no Lagrange parameter is necessary. The variation of the time integral of Φ with respect to **A** leads to eqn. (A.133), and the variation with respect to **D** gives eqn. (A.134) with $\xi = 0$. Helmholtz's final formulation is obtained by adding to Φ an external contribution Φ' involving the 'forces' $\mathbf{E}_0$, **j**, and $-\mathbf{f}$ acting respectively on **D**, **A**, and the position **R** of the particles of the medium:

$$\Phi' = -\int \mathbf{E}_0 \cdot \mathbf{D} \mathrm{d}\tau - \int \mathbf{j} \cdot \mathbf{A} \mathrm{d}\tau + \int \mathbf{f} \cdot \mathbf{R} \mathrm{d}\tau, \tag{A.136}$$

The variations with respect to **A** and **D** now yield

$$\frac{\mathbf{D}}{\varepsilon} = \mathbf{E}_0 - \frac{\mathrm{D}\mathbf{A}}{\mathrm{D}t},$$
$$\nabla \times \frac{\nabla \times \mathbf{A}}{\mu} = \mathbf{j} + \frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t}, \tag{A.137}$$

which imply the Heaviside–Hertz equations for the electrodynamics of moving bodies.[43]

The variation with respect to **R** is more difficult to perform. Varying **A** like a force (see eqn. (A.41)) in the magnetic energy gives the magnetic force density

$$\mathbf{f}_m = (\nabla \times \mathbf{H}) \times \mathbf{B} - \frac{1}{2} \mathbf{B} \cdot \mathbf{H} \frac{\nabla \mu}{\mu}, \quad \text{with} \quad \mathbf{B} = \nabla \times \mathbf{A} = \mu \mathbf{H}. \tag{A.138}$$

Varying **D** like a flux (see eqn. (A.42)) in the electric energy gives the electric force density

$$\mathbf{f}_e = \mathbf{E}(\nabla \cdot \mathbf{D}) + (\nabla \times \mathbf{E}) \times \mathbf{D} - \frac{1}{2} \mathbf{E} \cdot \mathbf{D} \frac{\nabla \varepsilon}{\varepsilon}, \quad \text{with} \quad \mathbf{D} = \varepsilon \mathbf{E}. \tag{A.139}$$

These expressions agree with those given by Maxwell's stress tensor (see Appendix 6). The variation of the remaining integrals, corresponding to the third term of eqn. (A.135) and to the two first terms of eqn. (A.136), is zero. Originally, Helmholtz verified this by using the explicit form of the variations of the implied vectors. The resulting calculations are quite complex. Fortunately, Helmholtz sketched a much simpler proof in an unfinished manuscript written the year of his death. His reasoning may be completed as follows.[44]

[43] Helmholtz 1892. [44] Helmholtz [1894].

A first remark is that the convective derivative of a flux is also a flux. Indeed, by definition of the convective derivative its flux $(D\mathbf{B}/Dt)\cdot d\mathbf{S}$ across the surface element $d\mathbf{S}$ of the medium is equal to $d(\mathbf{B}\cdot d\mathbf{S})/dt$, which shares with $\mathbf{B}\cdot d\mathbf{S}$ the property of being invariant with respect to a deformation of the medium. The second remark is that the variation of any integral of the form

$$I=\int \mathbf{A}\cdot\mathbf{B}d\tau \tag{A.140}$$

vanishes if **A** is varied as a force and **B** as a flux. In order to show this Helmholtz rewrote the integral I as

$$I=\int[(A_x dx)(B_x dydz)+\dots], \tag{A.141}$$

wherein the element of length dx and the surface element $dydz$ follow the motion of the medium. By definition of forces and fluxes, the products $A_x dx$ and $B_x dydz$ are invariant. Thanks to these two remarks, the stationarity of the integrals of $\mathbf{A}\cdot(D\mathbf{D}/Dt)$, $\mathbf{E}_0\cdot\mathbf{D}$, and $\mathbf{j}\cdot\mathbf{A}$ becomes obvious.

The potential vector as a generalized coordinate

MacCullagh's optical Lagrangian,

$$L=\frac{1}{2}m\left(\frac{\partial\boldsymbol{\xi}}{\partial t}\right)^2+\frac{1}{2}k(\nabla\times\boldsymbol{\xi})^2, \tag{A.142}$$

admits of two electromagnetic interpretations. The first that FitzGerald thought of assimilates $\boldsymbol{\xi}$ with the vector potential. The corresponding electromagnetic Lagrangian is

$$L=\frac{1}{2}\varepsilon\left(\frac{\partial\mathbf{A}}{\partial t}\right)^2+\frac{1}{2\mu}(\nabla\times\mathbf{A})^2. \tag{A.143}$$

The resulting equation of motion is

$$\frac{\partial}{\partial t}\left(\varepsilon\frac{\partial\mathbf{A}}{\partial t}\right)=-\nabla\times\frac{\nabla\times\mathbf{A}}{\mu}, \tag{A.144}$$

in conformity with Maxwell's expression of the Ampère law in dielectrics. Boltzmann later adopted this Lagrangian, and it is the one now most in favor. Yet FitzGerald and other British Maxwellians rejected it, for they wished to interpret the Lagrangian coordinates as the position of the ether particles. They could not do that with the vector potential, for this would have required a permanent creation or annihilation of ether around electrified objects. Moreover, this interpretation would imply that **A** and **E** are continuous at the border between two different dielectrics, which is incompatible with the divergenceless character of $\mathbf{D}=\varepsilon\mathbf{E}$.[45]

[45] Cf. FitzGerald's unpublished manuscript discussed in Hunt 1991a: 17.

The magnetic force as a velocity

FitzGerald therefore chose the alternative electromagnetic interpretation of MacCullagh's Lagrangian, for which the magnetic force **H** is the time derivative of $\boldsymbol{\xi}$, and the electric displacement **D** its curl. Since **B** is divergenceless, the ether flow is no longer paradoxical. As for the continuity of $\boldsymbol{\xi}$ between two different dielectrics, it is no longer damaging since it requires only the continuity of **H**, which does not conflict with the divergenceless character of $\mathbf{B} = \mu\mathbf{H}$ as long as μ is very nearly the same in all non-magnetic dielectrics. As is discussed in Chapter 8, Larmor adopted FitzGerald's interpretation of MacCullagh's Lagrangian in his ether theory.[46]

The introduction of conduction currents in MacCullagh's medium is problematic, as Heaviside showed in 1891 and as Larmor verified in his unfortunate attempt of 1894.[47]

Lagrangians for the electron theory

Lorentz based the equations of his electron theory on d'Alembert's principle.[48] However, his reasoning may easily be recast in Lagrangian form. Call **d** and **b** the microscopic field strengths, and $\rho_{\rm m}$ the microscopic charge density in electrostatic units. The kinetic energy of the field is

$$T = \frac{1}{2}\int b^2 \mathrm{d}\tau, \tag{A.145}$$

b being the divergenceless vector such that

$$\nabla \times \mathbf{b} = \frac{1}{c}\left(\rho_{\rm m}\mathbf{v} + \frac{\partial \mathbf{d}}{\partial t}\right). \tag{A.146}$$

The potential energy is

$$U = \frac{1}{2}\int d^2 \mathrm{d}\tau. \tag{A.147}$$

Owing to the constraints $\nabla\cdot(\rho_{\rm m}\mathbf{v} + \partial\mathbf{d}/\partial t) = 0$ and $\rho_{\rm m} - \nabla\cdot\mathbf{d} = 0$, the effective Lagrangian is

$$L = T - U + \int \xi\nabla\cdot\left(\rho_{\rm m}\mathbf{v} + \frac{\partial \mathbf{d}}{\partial t}\right)\mathrm{d}\tau + \int \eta(\rho_{\rm m} - \nabla\cdot\mathbf{d})\mathrm{d}\tau. \tag{A.148}$$

Varying the action with respect to **d** yields

$$\mathbf{d} = -\frac{1}{c}\frac{\partial \mathbf{a}}{\partial t} + \nabla\left(\eta + \frac{\partial \xi}{\partial t}\right), \tag{A.149}$$

[46] FitzGerald 1879a; Larmor 1894.

[47] Heaviside 1891, 1893–1912, Vol. 1: 243–51; Larmor 1894. For Heaviside's argument, see Buchwald 1985a: 68–70.

[48] Lorentz 1892a: 230–8.

where $\mathbf{a}$ is such that $\mathbf{b} = \nabla \times \mathbf{a}$. Eliminating the Lagrange parameters, we get

$$\nabla \times \mathbf{d} = -\frac{1}{c}\frac{\partial \mathbf{b}}{\partial t}. \tag{A.150}$$

We now extract from the Lagrangian the part that depends on the position $\mathbf{r}$ and the velocity $\mathbf{v}$ of a given material volume element $\mathrm{d}\tau$, with the invariable charge $\rho\mathrm{d}\tau$. This is

$$L_{\mathrm{d}\tau} = (\rho\mathrm{d}\tau)\left(\frac{\mathbf{v}}{c}\cdot\mathbf{a} - \mathbf{v}\cdot\nabla\xi + \eta\right). \tag{A.151}$$

The electromagnetic force acting on this element is

$$\mathbf{f}\mathrm{d}\tau = -\frac{\mathrm{d}}{\mathrm{d}t}\frac{\partial L_{\mathrm{d}\tau}}{\partial \mathbf{v}} + \frac{\partial L_{\mathrm{d}\tau}}{\partial \mathbf{r}}, \tag{A.152}$$

or

$$\mathbf{f} = \rho\left[-\frac{1}{c}\frac{\partial \mathbf{a}}{\partial t} + \nabla\left(\eta + \frac{\partial \xi}{\partial t}\right)\right] + \rho\frac{\mathbf{v}}{c}\times\mathbf{b}. \tag{A.153}$$

Combining this result with eqn. (A.149), we get the Lorentz force

$$\mathbf{f} = \rho\left(\mathbf{d} + \frac{\mathbf{v}}{c}\times\mathbf{b}\right). \tag{A.154}$$

Another possibility, exploited by Schwarzschild in 1903 and now frequently used, is to use the potentials as the generalized coordinates.[49] The corresponding Lagrangian reads

$$L = \int\left(\frac{1}{2}(B^2 - E^2) + \rho\left(\phi - \frac{\mathbf{v}}{c}\cdot\mathbf{A}\right)\right)\mathrm{d}\tau, \tag{A.155}$$

where

$$\mathbf{E} = -\nabla\phi - \frac{1}{c}\frac{\partial \mathbf{A}}{\partial t}, \quad \mathbf{B} = \nabla\times\mathbf{A}. \tag{A.156}$$

The variation with respect to ϕ yields

$$\nabla\cdot\mathbf{E} = \rho. \tag{A.157}$$

That with respect to $\mathbf{A}$ yields

[49] Schwarzschild 1903.

$$\nabla \times \mathbf{B} = \frac{1}{c}\left(\rho\mathbf{v} + \frac{\partial \mathbf{E}}{\partial t}\right). \tag{A.158}$$

Lastly, the variation with respect of a deformation of the charge carriers can be done as above, by singling out the contribution of a material element $d\tau$. The result is the Lorentz force

$$\mathbf{f} = \rho\left(\mathbf{E} + \frac{\mathbf{v}}{c} \times \mathbf{B}\right) \tag{A.159}$$

Appendix 10
Electric convection

J. J. Thomson's guesses (1881)

A moving charged particle, Thomson reasoned, carries along a field of displacement, at least if the velocity is not too close to c. Consequently, the displacement at a given point in space changes with time, which implies magnetically active currents according to Maxwell. Thomson calculated these currents in the case of a uniformly electrified, spherical shell. Inside the shell, the displacement is zero; outside the shell it is given by

$$\mathbf{D} = \frac{q}{4\pi}\nabla\frac{1}{|\mathbf{r}-\mathbf{R}|}, \quad (|\mathbf{r}-\mathbf{R}| > a) \tag{A.160}$$

wherein $\mathbf{R}$ denotes the position of the center of the shell at a given time, and a the radius of the shell. For the corresponding current of displacement, Thomson gave zero inside the sphere, and

$$\frac{\partial \mathbf{D}}{\partial t} = -\left(\mathbf{v}\cdot\frac{\partial}{\partial \mathbf{R}}\right)\mathbf{D} \tag{A.161}$$

outside the sphere. The corresponding vector potential is[50]

$$\mathbf{A}(\mathbf{r}) = \frac{\mu}{4\pi}\int_{|\mathbf{r}'-\mathbf{R}|>a}\frac{1}{|\mathbf{r}-\mathbf{r}'|}\frac{\partial \mathbf{D}}{\partial t}(\mathbf{r}')\mathrm{d}\tau', \tag{A.162}$$

At that point, Thomson worried that this potential did not comply with Maxwell's condition $\nabla\cdot\mathbf{A} = 0$. Instead of trying to locate the origin of this difficulty, he added a term $\mu q\mathbf{v}/6\pi r$ to the potential that made it divergenceless. From this improved potential, he calculated the electrokinetic energy

$$T = \frac{1}{2}\int_{|\mathbf{r}-\mathbf{R}|>a}\mathbf{A}\cdot\frac{\partial \mathbf{D}}{\partial t}\mathrm{d}\tau, \tag{A.163}$$

and found

$$T = \frac{1}{2}(\delta m)v^2, \quad \text{with} \quad \delta m = \frac{1}{15\pi}\frac{\mu q^2}{a}. \tag{A.164}$$

Hence the moving particle acquires an additional mass δm of electromagnetic origin.

Next, Thomson calculated the magnetic induction $\mathbf{B} = \nabla\times\mathbf{A}$, and found

$$\mathbf{B} = \frac{\mu}{4\pi}q\mathbf{v}\times\frac{\mathbf{r}-\mathbf{R}}{|\mathbf{r}-\mathbf{R}|^3}, \tag{A.165}$$

[50] J. J. Thomson 1881a.

which is the field of a current element $q\mathbf{v}$ at the position of the particle.

Lastly, Thomson wrote the interaction energy between the moving charge and a permanent magnet as

$$T_l = \frac{1}{2\mu}\int \mathbf{B}\cdot\mathbf{B}_0 \mathrm{d}\tau = \frac{1}{2\mu}\int \mathbf{B}\cdot(\nabla\times\mathbf{A}_0)\mathrm{d}\tau, \tag{A.166}$$

where $\mathbf{B}$ is the magnetic induction produced by the particle and $\mathbf{B}_0$ that produced by the magnet. An integration by parts, and the consequence

$$\nabla\times\mathbf{B} = \mu q\mathbf{v}\delta(\mathbf{r}-\mathbf{R}) \tag{A.167}$$

of eqn. (A.165) yield

$$T_l = \frac{1}{2}q\mathbf{v}\cdot\mathbf{A}_0(\mathbf{R}). \tag{A.168}$$

According to Lagrange's equations, the force acting on the particle is

$$\mathbf{f} = \frac{\partial T_l}{\partial\mathbf{R}} - \frac{\mathrm{d}}{\mathrm{d}t}\frac{\partial T_l}{\partial\mathbf{v}} = \frac{1}{2}q\mathbf{v}\times\mathbf{B}_0. \tag{A.169}$$

Corrections

These reasonings involved several misconceptions and miscalculations. First, Thomson miscalculated the derivative $\partial\mathbf{D}/\partial t$ by ignoring the sudden variation of $\mathbf{D}$ that occurs at points of space through which the surface of the sphere passes. FitzGerald corrected this mistake and found that the curl of the properly computed derivative vanished. This was to be expected: the time derivation and the curl commute with one another, and the curl of the displacement is zero as long as self-induction is neglected. FitzGerald's concluded that Maxwell's formula for the total current was incomplete. He proposed a new contribution $\rho\mathbf{v}$ that accounted for Rowland's results with rotating electrified disks. Then the vector potential of the moving sphere has the form

$$\mathbf{A} = \frac{\mu}{4\pi}\frac{q\mathbf{v}}{r} + \nabla\psi, \tag{A.170}$$

which justifies Thomson's expression for the magnetic field. Thomson's calculation of the force acting on the particle involved one more mistake: the interaction energy (A.168) should be doubled. Then the force is $q\mathbf{v}\times\mathbf{B}_0$, in harmony with Lorentz's later formula.[51]

Heaviside corrected the latter mistake in 1889. In 1885 he had already corrected the expression of the electromagnetic mass, to obtain

$$\delta m = \frac{1}{6\pi}\frac{\mu q^2}{a}. \tag{A.171}$$

[51] FitzGerald 1881. Cf. Buchwald 1985a: 271.

He also offered a physical justification of the contribution $\rho\mathbf{v}$ to the total current:

We at once find [. . .] that to close the current requires us to regard the moving charge itself as a current-element, of moment [i.e.: intensity] equal to the charged multiplied by its velocity [. . .]. The necessity of regarding the moving charge as an element of the 'true current' [the curl of $\mathbf{H}$] may be also concluded by simply considering that when a charge q is conveyed *into* any region, an equal displacement simultaneously leaves it through its boundary.

In symbols, this gives

$$\nabla\cdot\frac{\partial\mathbf{D}}{\partial t}=\frac{\partial\rho}{\partial t}=-\nabla\cdot(\rho\mathbf{v}), \tag{A.172}$$

and therefore,[52]

$$\nabla\cdot\mathbf{J}=0 \quad \text{if} \quad \mathbf{J}=\frac{\partial\mathbf{D}}{\partial t}+\rho\mathbf{v}. \tag{A.173}$$

In another justification of the convection current $\rho\mathbf{v}$ offered by Hertz in 1890, the electric displacement is treated as a flux within the moving matter, in harmony with Maxwell's general treatment of ether and matter as a single medium with variable parameters ε, μ, σ, and $\mathbf{v}$.[53] Then the displacement current is the convective derivative

$$\frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t}=\frac{\partial\mathbf{D}}{\partial t}+\mathbf{v}(\nabla\cdot\mathbf{D})-\nabla\times(\mathbf{v}\times\mathbf{D}). \tag{A.174}$$

which contains the term $\rho\mathbf{v}$. Through the Ampère theorem, the third term yields the magnetic motional force $\mathbf{D}\times\mathbf{v}$. In the case of a superficially electrified sphere, this force vanishes everywhere except in the immediate vicinity of the surface. Consequently, it does not contribute to the magnetic field energy from which the electromagnetic mass and the Lorentz force derive.

Fast convection

When the velocity of the electrified particle approaches the velocity of light, the electric field of the particle is not longer equal to the Coulomb field. There is a non-negligible contribution from the electromagnetic induction by the varying displacement current. In 1888 Heaviside provided an exact formula for this effect in the case of a point charge. His second proof of this formula goes as follows.[54]

Heaviside first wrote the duplex equations in a moving medium without impressed forces:

[52] Heaviside 1885–1887: 446; 1889c: 504 (quotation).
[53] Hertz 1890b: 263–5, 274–5.
[54] Heaviside 1888–1889: 495 (formula), 1889c: 510 (first proof); 1888–1889: part 4 (second proof).

$$\begin{cases} \nabla\times(\mathbf{E}-\mathbf{v}\times\mathbf{B}) = -\dfrac{\partial\mathbf{B}}{\partial t} \\ \nabla\times(\mathbf{H}-\mathbf{D}\times\mathbf{v}) = \dfrac{\partial\mathbf{D}}{\partial t}+\mathbf{j}+\rho\mathbf{u} \end{cases} \tag{A.175}$$

Here **v** denotes the velocity of the medium, and **u** the velocity of electrification. As Heaviside soon admitted, from a Maxwellian point of view these two velocities should be the same, since electrification is a property of a single ether matter medium. Yet in 1889 he assumed, as Lorentz would later do, that *the motion of an electrified particle did not at all disturb the motion of the medium.*

In a reference frame bound to the charged particle, the ether moves with the velocity **–u**, and there is no convection current, so that the equations become

$$\begin{aligned} \nabla\times(\mathbf{E}+\mathbf{u}\times\mathbf{B}) &= \mathbf{0}, \\ \nabla\times(\mathbf{H}+\mathbf{D}\times\mathbf{u}) &= \mathbf{0}. \end{aligned} \tag{A.176}$$

Using $\nabla\cdot\mathbf{B}=0$ and the first equation, we get

$$\nabla\cdot(\mathbf{H}+\mathbf{D}\times\mathbf{u}) = \mathbf{u}\cdot(\nabla\times\mathbf{D}) = -\varepsilon\mathbf{u}\cdot(\mathbf{u}\times\mathbf{B}) = 0. \tag{A.177}$$

Together with the second field equation, this implies

$$\mathbf{H} = \mathbf{u}\times\mathbf{D}. \tag{A.178}$$

Inserting this expression of **H** in the first equation we reach

$$\nabla\times\left[\left(1-\frac{u^2}{c^2}\right)\mathbf{E}+\frac{1}{c^2}(\mathbf{u}\cdot\mathbf{E})\mathbf{u}\right] = 0. \tag{A.179}$$

If the x-axis is parallel to **u**, there exists a pseudo-potential P such that

$$E_x = -\gamma^2\frac{\partial P}{\partial x},\quad E_y = -\frac{\partial P}{\partial y},\quad E_z = -\frac{\partial P}{\partial z}, \tag{A.180}$$

with

$$\gamma = \sqrt{1-\frac{u^2}{c^2}}. \tag{A.181}$$

Then, the equation

$$\nabla\cdot\mathbf{D} = \rho = q\delta(\mathbf{r}) \tag{A.182}$$

gives

$$\gamma^2\frac{\partial^2 P}{\partial x^2}+\frac{\partial^2 P}{\partial y^2}+\frac{\partial^2 P}{\partial z^2}+\frac{q}{\varepsilon}\delta(\mathbf{r}) = 0. \tag{A.183}$$

Clearly, the standard Poisson equation is retrieved through the substitutions

$$x' = \gamma^{-1}x, \quad y' = y, \quad z' = z, \quad q' = \gamma^{-1}q. \tag{A.184}$$

Consequently, the pseudo-potential is

$$P = \frac{q'}{4\pi\varepsilon r'} = \frac{q}{4\pi\varepsilon\gamma}\left[x^2\gamma^{-2} + y^2 + z^2\right]^{-1/2}, \tag{A.185}$$

and the corresponding electric field is

$$\mathbf{E} = \frac{q}{4\pi\varepsilon\gamma}\frac{\mathbf{r}}{r'^3}. \tag{A.186}$$

It is still a radial field, but it is more intense in transverse directions than in the longitudinal direction, as if the medium had been compressed along the direction of motion.

Appendix 11
Fresnel's coefficient

Fresnel's assumptions

In Fresnel's optical theory, light is a transverse vibration of a subtle elastic medium, the ether. The elasticity of the ether is supposed to be the same in all bodies, but its density depends on the composition of the body. Hence the velocity of light in the ether is inversely proportional to the square root of its density, or the density of a transparent body is proportional to the square of its optical index.[55]

Fresnel further assumes that a body moving across the ether drags along the excess of ether that it contains.[56] Calling ρ the density of the ether in the transparent body, ρ_0 the density of the ether in a vacuum, v the velocity of the body, and αv the velocity of the center of gravity of the ether in the body, we have

$$\rho\alpha v = (\rho - \rho_0)v, \tag{A.187}$$

or

$$\alpha = 1 - \frac{\rho_0}{\rho} = 1 - \frac{1}{n^2}. \tag{A.188}$$

Consequences on the laws of refraction

Consider the propagation of a ray of light in a transparent material medium with the variable index n and the uniform velocity $\mathbf{v}$. In the approximation of geometrical optics, the path of the ray between two points of the medium is the path for which the traveling time is a minimum. Call $\mathrm{d}\mathbf{l}$ an element of an arbitrary continuous path, $\mathrm{d}s$ its length, and $\mathrm{d}t$ the time that light would take to travel along it. The velocity of light in this element with respect to the remote, undisturbed parts of the ether is

$$c' = \frac{c}{n} + \alpha\mathbf{v}\cdot\frac{\mathrm{d}\mathbf{l}}{\mathrm{d}s}, \tag{A.189}$$

where α is the dragging coefficient of the ether in moving matter. With respect to the moving medium this velocity is

$$\frac{\mathrm{d}s}{\mathrm{d}t} = c' - \mathbf{v}\cdot\frac{\mathrm{d}\mathbf{l}}{\mathrm{d}s} = \frac{c}{n} + (\alpha - 1)\mathbf{v}\cdot\frac{\mathrm{d}\mathbf{l}}{\mathrm{d}s}. \tag{A.190}$$

To first order in v/c, this gives

$$\mathrm{d}t = \frac{n}{c}\mathrm{d}s + \frac{n^2}{c^2}(1-\alpha)\mathbf{v}\cdot\mathrm{d}\mathbf{l}. \tag{A.191}$$

[55] Cf. Whittaker 1951: 109–10. [56] Fresnel 1818.

For complete ether drag ($\alpha = 1$) the condition of minimum time is of course identical to Fermat's principle, which yields the usual laws of refraction. For partial drag, the same laws remain valid if and only if the coeffient of the second term of dt does not depend on the index n, which requires Fresnel's value (A.188) of the drag. Indeed the integral of $\mathbf{v}\cdot d\mathbf{l}$ depends only on the extremities of the path.[57]

Lorentz's derivation (1892–1899)

For microscopic fields and forces, Lorentz equations are

$$\nabla\cdot\mathbf{d} = \rho_m, \quad \nabla\cdot\mathbf{b} = 0,$$
$$\nabla\times\mathbf{b} = \frac{1}{c}\left(\rho_m\mathbf{v} + \frac{\partial\mathbf{d}}{\partial t}\right), \quad \nabla\times\mathbf{d} = -\frac{1}{c}\frac{\partial\mathbf{b}}{\partial t},$$
$$\mathbf{f} = \rho_m\left(\mathbf{d} + \frac{1}{c}\mathbf{v}\times\mathbf{b}\right) \tag{A.192}$$

with respect to the stationary ether (see p. 327). The equations with respect to a system of axes moving at the constant velocity $\mathbf{u}$ along the x-axis follow from the Galilean transformation $x \to x - ut$, which implies the substitutions

$$\frac{\partial}{\partial t} \to \frac{\partial}{\partial t} - \mathbf{u}\cdot\nabla, \quad \mathbf{v} \to \mathbf{v} + \mathbf{u}. \tag{A.193}$$

These equations read

$$\nabla\cdot\mathbf{d} = \rho_m, \ \nabla\cdot\mathbf{b} = 0,$$
$$\nabla\times\left(\mathbf{b} - \frac{1}{c}\mathbf{u}\times\mathbf{d}\right) = \frac{1}{c}\left(\rho_m\mathbf{v} + \frac{\partial\mathbf{d}}{\partial t}\right), \quad \nabla\times\left(\mathbf{d} + \frac{1}{c}\mathbf{u}\times\mathbf{b}\right) = -\frac{1}{c}\frac{\partial\mathbf{b}}{\partial t},$$
$$\mathbf{f} = \rho_m\left(\mathbf{d} + \frac{1}{c}\mathbf{u}\times\mathbf{b} + \frac{1}{c}\mathbf{v}\times\mathbf{b}\right). \tag{A.194}$$

Lorentz then introduces the variables

$$x' = x, \quad y' = y, \quad z' = z, \quad t' = t - ux/c^2, \tag{A.195}$$

to which the differential operators

$$\nabla' = \nabla + \frac{\mathbf{u}}{c^2}\frac{\partial}{\partial t}, \quad \frac{\partial}{\partial t'} = \frac{\partial}{\partial t} \tag{A.196}$$

correspond, and he performs the field transformation

$$\mathbf{d}' = \mathbf{d} + \frac{1}{c}\mathbf{u}\times\mathbf{b}, \quad \mathbf{h}' = \mathbf{h} - \frac{1}{c}\mathbf{u}\times\mathbf{d}. \tag{A.197}$$

[57] Cf. Mascart 1893: Vol. 3, Ch. 15.

To first order in u/c, the resulting equations have exactly the same form as the equations (A.192) with respect to the ether.[58]

We now consider a transparent body moving through the ether at the velocity **u**, and we assume that the binding forces of the ions and electrons of the body are not modified by its motion (to first order in u/c). Then the equations ruling the accented fields are the same as the equations for the same body at rest. Consequently, they admit monochromatic plane wave solutions with the phase

$$\varphi = \omega t' - \mathbf{k}\cdot\mathbf{r}' = \omega t - \left(\mathbf{k} + \omega\mathbf{u}/c^2\right)\cdot\mathbf{r}, \tag{A.198}$$

with $\omega/k = c/n$. The corresponding true, non-accented, fields are also monochromatic plane waves, with the velocity

$$V = \frac{\omega}{\left|\mathbf{k} + \omega\mathbf{u}/c^2\right|} = \frac{c}{n} - \frac{1}{n^2}\mathbf{u}\cdot\frac{\mathbf{k}}{k} + O\left(u^2/c^2\right), \tag{A.199}$$

in agreement with eqns. (A.188) and (A.190) for the Fresnel drag.[59]

Poincaré's derivation (1900a)

According to Poincaré, the local time t' is the apparent time for observers moving with the transparent body. The relativity principle implies that the apparent velocity $\mathrm{d}\mathbf{r}'/\mathrm{d}t'$ of the waves is the same as if the body were at rest. This gives

$$\frac{c}{n} = \left|\frac{\mathrm{d}\mathbf{r}'}{\mathrm{d}t'}\right| = \left|\frac{\mathrm{d}\mathbf{r}}{\mathrm{d}t}\right| \frac{1}{\left|1 - \dfrac{\mathbf{u}}{c^2}\cdot\dfrac{\mathrm{d}\mathbf{r}}{\mathrm{d}t}\right|}, \tag{A.200}$$

which implies eqn. (A.199) and the Fresnel drag to first order in u/c.[60]

Laue's derivation (1907)

Laue's reasoning (Laue 1907) is formally identical to Poincaré's. However, it is based on Einstein's relativity. Hence the times t' and t now have comparable status. They refer to different Galilean systems of coordinates. There is no ether for which t would have a privileged meaning. The velocity $d\mathbf{r}/dt$ is no truer than the velocity $\mathrm{d}\mathbf{r}'/\mathrm{d}t'$.

[58] See Lorentz 1899. In previous reasonings Lorentz used the macroscopic field equations.

[59] Lorentz 1895: 95–7 for reasoning on phases; Lorentz 1899 for the microscopic approach.

[60] Poincaré 1900a: 278. Although Poincaré does not give the calculation, he notes that the result of Fizeau's experiment corresponds to the usual law of propagation in the running water *if this propagation is referred to the local time*.

Appendix 12
Cohn's electrodynamics

Cohn's circuital equations are (in Hertz's units):

$$\nabla\times\mathbf{E}=-\frac{1}{c}\frac{\mathrm{D}\mathbf{B}}{\mathrm{D}t},$$
$$\nabla\times\mathbf{H}=\frac{1}{c}\left(\frac{\mathrm{D}\mathbf{D}}{\mathrm{D}t}+\mathbf{j}\right), \tag{A.201}$$

with

$$\mathbf{D}=\varepsilon\mathbf{E}-\frac{1}{c}\mathbf{v}\times\mathbf{H},$$
$$\mathbf{B}=\mu\mathbf{H}+\frac{1}{c}\mathbf{v}\times\mathbf{E}, \tag{A.202}$$

where **E** and **H** are the forces acting on a unit electric or magnetic pole bound to the moving matter, D/Dt is the convective derivative for fluxes, and **v** is the velocity of matter with respect to the fixed stars. This velocity is supposed to be well defined even for very dilute matter. Cohn further assumes the conditions[61]

$$\nabla\cdot\mathbf{D}=\rho,\quad \nabla\cdot\mathbf{B}=0. \tag{A.203}$$

For bodies at rest on Earth, and in a system of axes bound to the Earth, these equations become

$$\nabla\times\mathbf{E}=-\frac{1}{c}\frac{\partial}{\partial t}\left(\varepsilon\mathbf{E}-\frac{1}{c}\mathbf{u}\times\mathbf{H}\right),$$
$$\nabla\times\mathbf{H}=\frac{1}{c}\frac{\partial}{\partial t}\left(\mu\mathbf{H}+\frac{1}{c}\mathbf{u}\times\mathbf{E}\right)+\frac{\mathbf{j}}{c}, \tag{A.204}$$

with the conditions

$$\nabla\cdot\left(\varepsilon\mathbf{E}-\frac{1}{c}\mathbf{u}\times\mathbf{H}\right)=\rho,$$
$$\nabla\cdot\left(\mu\mathbf{H}+\frac{1}{c}\mathbf{u}\times\mathbf{E}\right)=0. \tag{A.205}$$

Here **u** denotes the velocity of the Earth with respect to the fixed stars. Cohn introduces the local time $t'=t-\mathbf{u}\cdot\mathbf{r}/c^2$, noting, in 1904, that it is given by terrestrial clocks synchronized by optical means. The corresponding differential operators are

[61] Cohn 1902. Cf. Darrigol 1995b.

$$\frac{\partial}{\partial t'} = \frac{\partial}{\partial t}, \quad \nabla' = \nabla + \frac{\mathbf{u}}{c^2}\frac{\partial}{\partial t}. \tag{A.206}$$

In these terms, the circuital equations read

$$\nabla' \times \mathbf{E} = -\frac{1}{c}\frac{\partial \mu \mathbf{H}}{\partial t'},$$
$$\nabla' \times \mathbf{H} = \frac{1}{c}\left(\frac{\partial \varepsilon \mathbf{E}}{\partial t'} + \mathbf{j}\right), \tag{A.207}$$

and the conditions read

$$\nabla' \cdot (\varepsilon \mathbf{E}) = \rho',$$
$$\nabla' \cdot (\mu \mathbf{H}) = 0, \tag{A.208}$$

with

$$\rho' = \rho - \frac{1}{c^2}\mathbf{j} \cdot \mathbf{u}. \tag{A.209}$$

The latter density is that observed on Earth, for it complies with the relation

$$\partial \rho'/\partial t' + \nabla' \cdot \mathbf{j} = 0 \tag{A.210}$$

(this is easily seen by taking the divergence of the second of the circuital equations). Consequently, for bodies at rest with respect to the Earth, the equations governing the observable evolution of the field are exactly the same as if the Earth did not move. In other words, the observed field does not depend on the motion of the Earth, at any order in u/c.[62]

This result immediately implies the insensitivity of terrestrial optical experiments to the motion of the Earth. Cohn also derived the aberration of stars and the Fresnel drag from his theory. For this purpose it is sufficient to note that to first order in u/c his theory is equivalent to Lorentz's. The exact connection between the two theories is as follows.

Relation with Lorentz's theory

In this section, the units of space and time are chosen so that $c = 1$, and we assume that all matter is non-magnetic ($\mu = 1$). With respect to the stationary ether, the macroscopic equations of Lorentz's theory are

$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t},$$
$$\nabla \times \mathbf{H} = \mathbf{j} + \frac{\partial \mathbf{D}}{\partial t} + \mathbf{v}(\nabla \cdot \mathbf{D}) - \nabla \times (\mathbf{v} \times \mathbf{P}). \tag{A.211}$$

[62] Cohn 1900b, 1902, 1904.

Here **E** and **H** are the forces acting on a unit charge or pole at rest, **P** is the ionic polarization; **D** = **E** + **P** corresponds to Maxwell's electric displacement, and **B** = **H** to his magnetic induction. The substitutions

$$\frac{\partial}{\partial t}\rightarrow\frac{\partial}{\partial t}-\mathbf{u}\cdot\nabla,$$
$$\mathbf{v}\rightarrow\mathbf{u}+\mathbf{v} \tag{A.212}$$

lead to the equations with respect to the Earth

$$\nabla\times(\mathbf{E}+\mathbf{u}\times\mathbf{H})=-\frac{\partial\mathbf{B}}{\partial t},$$
$$\nabla\times(\mathbf{H}-\mathbf{u}\times\mathbf{E})=\mathbf{j}+\frac{\partial\mathbf{D}}{\partial t}. \tag{A.213}$$

if all bodies are at rest on Earth.[63]

Cohn observed that in Lorentz's theory the longitudinal dimensions of moving bodies were contracted by a factor γ^{-1} and the times of evolution in a moving system were dilated by a factor

$$\gamma=\frac{1}{\sqrt{1-u^2/c^2}}. \tag{A.214}$$

Then, Cohn went on, the measured space and time should be

$$\mathbf{r}'=(\gamma,1)\mathbf{r},\quad t'=\gamma^{-1}t, \tag{A.215}$$

where (α, β) denotes the multiplication by α of the component parallel to **u** and the multiplication by β of the component perpendicular to **u**. For the fluxes **j**′, **B**′, and **D**′ with respect to the surface elements d**S**′ = $(1, \gamma)$d**S**, we have

$$\mathbf{B}'=(1,\gamma^{-1})\mathbf{B},\quad \mathbf{D}'=(1,\gamma^{-1})\mathbf{D},\quad \mathbf{j}'=(1,\gamma^{-1})\mathbf{j},\quad \mathbf{P}'=(1,\gamma^{-1})\mathbf{P}. \tag{A.216}$$

For the forces **E**′ and **H**′ referred to the length–time elements d**l**′dt' = $(\gamma, 1)\gamma^{-1} = (1, \gamma^{-1})$ and to test charges and poles moving with the system (at the velocity **u**), we have

$$\mathbf{E}'=(1,\gamma)(\mathbf{E}+\mathbf{u}\times\mathbf{H}),$$
$$\mathbf{H}'=(1,\gamma)(\mathbf{H}-\mathbf{u}\times\mathbf{E}), \tag{A.217}$$

In terms of these observable quantities, the field equations are

$$\nabla'\times\mathbf{E}'=-\frac{\partial\mathbf{B}'}{\partial t'},$$
$$\nabla'\times\mathbf{H}'=\mathbf{j}'+\frac{\partial\mathbf{D}'}{\partial t'}. \tag{A.218}$$

[63] Lorentz 1895 (for the equations in a system of axes bound to the Earth).

with

$$\begin{aligned}\mathbf{D}' &= \mathbf{P}' + \mathbf{E}' - \mathbf{u}\times\mathbf{H}' \\ \mathbf{B}' &= \mathbf{H}' + \mathbf{u}\times\mathbf{E}',\end{aligned} \tag{A.219}$$

According to Lorentz's microscopic picture of polarization, $\mathbf{P}'$ is proportional to $\mathbf{E}'$ (when dispersion is negligible). Hence the equations ruling the fields observed on Earth are the same as in Cohn's theory.[64]

Three remarks are in order. First, Lorentz himself did not give direct physical significance to the accented coordinates (those for which Cohn's equations hold). For him, these coordinates referred to a fictitious system obtained by mentally bringing the moving system to rest. Cohn's interpretation of the accented coordinates was therefore a new contribution to Lorentz's theory. Second, in Cohn's theory there is no contraction of length and no dilation of times. Accordingly, Cohn's field equations with respect to the fixed stars differ from Lorentz's equations with respect to the stationary ether. As we just saw, for terrestrial observers this difference is exactly compensated by Lorentz's contraction–dilation effects. Third, Cohn's interpretation of the accented coordinates in Lorentz's theory does not take clock synchronization into account. If clocks are optically synchronized, the observed time is the local time $t'' = t' - \mathbf{u}\cdot\mathbf{r}'/c^2 = \gamma^{-1}t - \gamma\mathbf{u}\cdot\mathbf{r}/c^2$, which is the one given by Lorentz's transformation (a Galilean transformation being already included in $\mathbf{r}$).

Energy and mechanical forces

The circuital equations (A.201) and the expressions (A.202) of $\mathbf{D}$ and $\mathbf{B}$ imply the balance

$$\nabla\cdot\mathbf{\Pi} + \left(\frac{\partial w}{\partial t} + \nabla\cdot(w\mathbf{v})\right) + \mathbf{j}\cdot\mathbf{E} - \sigma_{ij}\,\partial_i v_j - \mathbf{v}\cdot\frac{\mathrm{D}}{\mathrm{D}t}\frac{\mathbf{\Pi}}{c^2} = 0, \tag{A.220}$$

with

$$\mathbf{\Pi} = \mathbf{E}\times\mathbf{H}, \tag{A.221}$$

$$w = \frac{1}{2}\left(\varepsilon E^2 + \mu H^2\right) + \frac{2}{c^2}\mathbf{v}\cdot(\mathbf{E}\times\mathbf{H}). \tag{A.222}$$

and

$$\sigma_{ij} = \varepsilon E_i E_j - \frac{1}{2}\varepsilon E^2\delta_{ij} + \mu H_i H_j - \frac{1}{2}\mu H^2\delta_{ij}. \tag{A.223}$$

For a *material* element of volume and in a unit of time, the successive terms of this equation give the energy flux across the surface of the element, the variation of the field energy in the element, the Joule heat dissipated in the element, the work of stresses at the surface of the element, and the work of a force density which is the convective derivative of $\mathbf{\Pi}/c^2$.[65]

The net force acting on a volume element is

[64] Cohn 1904: 1294–303. [65] Cohn 1904: 1404–16.

$$f_i = \partial_i \sigma_{ij} - \frac{1}{c^2}\frac{\mathrm{D}\Pi_j}{\mathrm{D}t}, \tag{A.224}$$

or

$$\mathbf{f} = \mathbf{E}(\nabla\cdot\varepsilon\mathbf{E}) + \mathbf{H}(\nabla\cdot\mu\mathbf{H}) + (\nabla\times\mathbf{E})\times\varepsilon\mathbf{E} + (\nabla\times\mathbf{H})\times\mu\mathbf{H}$$
$$-\frac{1}{2}(\nabla\varepsilon)E^2 - \frac{1}{2}(\nabla\mu)H^2 - \frac{1}{c^2}\frac{\mathrm{D}\mathbf{\Pi}}{\mathrm{D}t}. \tag{A.225}$$

When there is no absolute motion of matter, this force is identical to the one predicted by the Maxwell–Hertz theory except for the last term. Thanks to this term there is no force acting in a vacuum, as should be expected in a theory without ether.[66]

However, the equality of action and reaction does not hold. The total force acting on matter is

$$\int \mathbf{f}\mathrm{d}\tau = -\frac{1}{c^2}\frac{\partial}{\partial t}\int \mathbf{E}\times\mathbf{H}\mathrm{d}\tau, \tag{A.226}$$

which in general differs from zero. Cohn noted this without comment.[67] He knew that Newton's third law did not apply to Lorentz's theory either; and he had no reason to maintain this law in his theory, which purposely avoided mechanical reduction.

Consider now the implications of Cohn's theory for forces acting on bodies at rest with respect to the Earth. Introducing the new space and time coordinates

$$\mathbf{r}' = \mathbf{r} - \mathbf{u}t,$$
$$t' = t - \mathbf{u}\cdot\mathbf{r}/c^2 \tag{A.227}$$

in eqn. (A.224), we get

$$f_j = \partial_i' \sigma_{ij} - \frac{1}{c^2}\frac{\partial \Pi_j}{\partial t'} - \frac{1}{c^2}\frac{\partial}{\partial t'}(u_i\sigma_{ij}). \tag{A.228}$$

The two first terms reproduce the force that would exist if the Earth was not moving. The last one disappears for constant fields, so that ordinary electrostatic and electrodynamic forces do not depend at all on the Earth's motion. In the case of variable fields, this term does not necessarily vanish, but according to Cohn it is too small to be detected. In sum, only a negligible part of Cohn's electro-mechanical forces depend on the Earth's motion.[68]

66 Cohn 1902: 50. 67 Cohn 1902. 68 Cohn 1904: 1404–1416.

Abbreviations used in bibliographies

ACP: *Annales de Chimie et de Physique.*
AHES: *Archive for History of Exact Sciences.*
AHQP: Archive for the History of Quantum Physics. University of Berkeley, Office for history of science and technology; Paris, Médiathèque d'histoire des sciences de La Villette; and elsewhere.
AIHS: *Archives Internationales d'Histoire des Sciences.*
AJP: *American Journal of Physics.*
AJS: *American Journal of Science.*
AN: *Archives Néerlandaises.*
AP: *Annalen der Physik (und der Chemie).*
ASPN: *Supplément à la Bibliothèque Universelle de Genève: Archives des Sciences Physiques et Naturelles.*
BAR: British Association for the Advancement of Science, *Report.*
BB: Akademie der Wissenschaften zu Berlin, mathematisch-physikalische Klasse, *Sitzungsberichte.*
BJHS: *British Journal for the History of Science.*
BJPS: *British Journal for the Philosophy of Science.*
CA: *Correspondance du grand Ampère.* Ed. by L. de Launay. 3 Vols. Paris, 1936.
CDMJ: *The Cambridge and Dublin Mathematical Journal.*
CMF: *The correspondence of Michael Faraday.* Ed. F. James. 2 Vols. Exeter, 1991.
CMJ: *The Cambridge Mathematical Journal.*
CR: Académie des sciences, Paris, *Comptes-rendus Hebdomadaires des Séances.*
DSB: *Dictionary of scientific biography.* Ed. C. C. Gillispie. 16 Vols. New York, 1970–1980.
ECP: *The collected papers of Albert Einstein*, Vols. 1–2. Ed. John Stachel *et al.* Princeton, 1987–1989.
EE: *L'Éclairage Électrique.*
FD: Michael Faraday, *Diary.* 8 Vols. London, 1932–1936.
FER: Michael Faraday, *Experimental researches.* 3 Vols. London, 1839, 1844, 1855.
FSW: *The scientific writings of the late George Francis FitzGerald.* Ed. Joseph Larmor. Dublin, 1902.
GN: Königliche Gesellschaft der Wissenschaften und der Georg August Universität zu Göttingen. *Nachrichten.*
GW: Carl Friedrich Gauss, *Werke.* 15 Vols. Hildesheim, 1973.
HEP: Oliver Heaviside, *Electrical papers.* 2 Vols. New York, 1892.
HGW: Heinrich Hertz, *Gesammelte Werke.* 3 Vols. Leipzig, 1894–1895.
HSPS: *Historical Studies in the Physical (and Biological) Sciences.*

HWA: Hermann von Helmholtz, *Wissenschaftliche Abhandlungen*. 3 Vols. Leipzig, 1882, 1883, 1895.
JP: *Journal de Physique, de Chimie et d'Histoire Naturelle.*
JRAM: *Journal für die reine und angewandte Mathematik.*
JSHS: *Japanese Studies in the History of Science.*
KGA: Gustav Kirchhoff, *Gesammelte Abhandlungen*. Leipzig, 1882.
KSGA: Königliche Sächsische Gesellschaft der Wissenschaften zu Leipzig, Mathematisch-physikalische Classe, *Abhandlungen*.
KSGB: Königliche Sächsische Gesellschaft der Wissenschaften zu Leipzig, *Berichte über die Verhandlungen*.
LCP: Hendrik Antoon Lorentz, *Collected papers*. 9 Vols. The Hague, 1934–1936.
LMPP: Joseph Larmor, *Mathematical and physical papers*. 2 Vols. Cambridge, 1929.
MRP: *Collection de mémoires relatifs à la physique*. 2 Vols. Paris, 1884, 1885.
MSLP: *The scientific letters and papers of James Clerk Maxwell*. Ed. Peter Harman. Vols. 1–2. Cambridge, 1990–1995.
MSP: James Clerk Maxwell, *The scientific papers*. 2 Vols. Cambridge, 1890.
NRRS: Royal Society of London, *Notes and Records*.
NTM: *Schriftenreihe für Geschichte der Naturwissenschaften, Technik, und Medizin*.
PCPS: Cambridge Philosophical Society, *Proceedings*.
PLMS: London Mathematical Society, *Proceedings*.
PM: *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*.
PO: *Oeuvres de Henri Poincaré*. 11 Vols. Paris, 1954.
PRA: Royal Academy of Amsterdam, *Proceedings*.
PRDS: Royal Dublin Society, *Scientific Proceedings*.
PRI: Royal Institution, *Proceedings*.
PRS: Royal Society of London, *Proceedings*.
PT: Royal Society of London, *Philosophical Transactions*.
PZ: *Physikalische Zeitschrift*.
QJS: *Quarterly Journal of Science*.
RBMV: *Resultate aus den Beobachtungen des magnetischen Vereins*.
RHS: *Revue d'Histoire des Sciences*.
SCMF: *The selected correspondence of Michael Faraday*. Ed. L. P. Williams, 2 Vols. Cambridge, 1971.
SHPMP: *Studies in History and Philosophy of Modern Physics*.
SHPS: *Studies in History and Philosophy of Science*.
TCPS: Cambridge Philosophical Society, *Transactions*.
TMPP: William Thomson, *Mathematical and physical papers*. 6 Vols. Cambridge, 1882–1911.
TPEM: William Thomson, *Reprint of papers on electrostatics and magnetism*. London, 1872.
TRDS: Royal Dublin Society, *Scientific Transactions*.
VDNA: Gesellschaft Deutscher Naturforscher und Aertze, *Verhandlungen*.
VKA: Koninklijke Akademie van Wetenschappen, Amsterdam, *Verslagen*.
WW: Wilhelm Weber, *Werke*. 6 Vols. Berlin, 1892–1894.

Nota bene: References to Thomson and to Wiedemann, without mention of the first name, correspond to William Thomson and to Gustav Wiedemann.

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Olivier Darrigol is a Research Director at the Centre National de la Recherche Scientifique, Paris. His research focuses on the history of quantum theory and of electrodynamics.

Front cover image: Early electric lighting in San Jose, California (*La lumière électrique* **7** (1882) p. 277).

Back cover image: Exhibition of electric equipment by Sautter, Lemonnier & Co. (*La lumière électrique* **7** (1882) p. 227).

Spine image: Heinrich Dove's differential inductor (1841: from G. Wiedemann *Die Lehre der Elektricität* **4** (1885) p. 203).

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