Representing and intervening

Introductory topics in the philosophy of natural science

Ian Hacking

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Acknowledgements

What follows was written while Nancy Cartwright, of the Stanford University Philosophy Department, was working out the ideas for her book, *How the Laws of Physics Lie.* There are several parallels between her book and mine. Both play down the truthfulness of theories but favour some theoretical entities. She urges that only phenomenological laws of physics get at the truth, while in Part B, below, I emphasize that experimental science has a life more independent of theorizing than is usually allowed. I owe a good deal to her discussion of these topics. We have different anti-theoretical starting points, for she considers models and approximations while I emphasize experiment, but we converge on similar philosophies.

My interest in experiment was engaged in conversation with Francis Everitt of the Hanson Physical Laboratory, Stanford. We jointly wrote a very long paper, `Which comes first, theory or experiment?' In the course of that collaboration I learned an immense amount from a gifted experimenter with wide historical interests. (Everitt directs the gyro project which will soon test the general theory of relativity by studying a gyroscope in a satellite. He is also the author *of lames Clerk Maxwell,* and numerous essays in the *Dictionary of Scientific Biography.)* Debts to Everitt are especially evident in Chapter 9. Sections which are primarily due to Everitt are marked (E). I also thank him for reading the finished text with much deliberation.

Richard Skaer, of Peterhouse, Cambridge, introduced me to microscopes while he was doing research in the Haematological Laboratory, Cambridge University, and hence paved the way to Chapter ii. Melissa Franklin of the Stanford Linear Accelerator taught me about PEGGY II and so provided the core material for Chapter 16. Finally I thank the publisher's reader, Mary Hesse, for many thoughtful suggestions.

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(1982). Parts of Chapters 1o, 12 and 13 are adapted from *Versuchungen: Aufsatze zur Philosophy Paul Feyerabends* (ed. Peter Duerr), Suhrkamp: Frankfurt, 1981, Bd. 2, pp. 126—58. Chapter 9 draws on my joint paper with Everitt, and Chapter 8 develops my review of Lakatos, *British journal for the Philosophy of Science* 30 (1979), pp. 381—410. The book began in the middle, which I have called a "break'. That was a talk with which I was asked to open the April, 1979, Stanford—Berkeley Student Philosophy conference. It still shows signs of having been written in Delphi a couple of weeks earlier.

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explanation. They hold that theories are instruments for predicting phenomena, and for organizing our thoughts. A criticism of `inference to the best explanation' is developed.

4 Pragmatism 58 C.S. Peirce said that something is real if a community of inquirers will end up agreeing that it exists. He thought that truth is what scientific method finally settles upon, if only investigation continues long enough. W. James and J. Dewey place less emphasis on the long run, and more on what it feels comfortable to believe and talk about now. Of recent philosophers, H. Putnam goes along with Peirce while R. Rorty favours James and Dewey. These are two different kinds of anti-realism.

5 **Incommensurability** 65 T.S. Kuhn and P. Feyerabend once said that competing theories cannot be well compared to see which fits the facts best. This idea strongly reinforces one kind of anti-realism. There are at least three ideas here. Topic-incommensurability: rival theories may only partially overlap, so one cannot well compare their successes overall. Dissociation: after sufficient time and theory change, one world view may be almost unintelligible to a later epoch. Meaning-incommensurability: some ideas about language imply that rival theories are always mutually incomprehensible and never inter-translatable, so that reasonable comparison of theories is in principle impossible.

6 Reference 75 H. Putnam has an account of the meaning of `meaning' which avoids meaning-incommensurability. Successes and failures of this idea are illustrated by short histories of the reference of terms such as: glyptodon, electron, acid, caloric, muon, meson.

7 **Internal realism** 92 Putnam's account of meaning started from a kind of realism but has become increasingly pragmatic and anti-realist. These shifts are described and compared to Kant's philosophy. Both Putnam and Kuhn come close to what is best called transcendental nominalism.

I. Lakatos had a methodology of scientific research programmes intended as an antidote to Kuhn. It looks like an account of rationality, but is rather an explanation of how scientific objectivity need not depend on a correspondence theory of truth.

BREAK: Reals and representations 130 This chapter is an anthropological fantasy about ideas of reality and representation from cave-dwellers to H. Hertz. It is a parable to show why the realism/anti-realism debates at the level of representation are always inconclusive. Hence we turn from truth and representation to experimentation and manipulation.

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9 Experiment 149 Theory and experiment have different relationships in different sciences at different stages of development. There is no right answer to the question: Which comes first, experiment, theory, invention, technology, . . .? Illustrations are drawn from optics, thermodynamics, solid state physics, and radioastronomy.

10 Observation 167 N.R. Hanson suggested that all observation statements are theory-loaded. In fact observation is not a matter of language, and it is a skill. Some observations are entirely pre-theoretical. Work by C. Herschel in astronomy and by W. Herschel in radiant heat is used to illustrate platitudes about observation. Far from being unaided vision, we often speak of observing when we do not literally `see' but use information transmitted by theoretically postulated objects.

11 Microscopes 186 Do we see with a microscope? There are many kinds of light microscope, relying on different properties of light. We believe what we see largely because quite different physical systems provide the same picture. We even `see' with an acoustic microscope that uses sound rather than light. **12 speculation, calculation, models, approximations 210)** There is not one activity, theorizing. There are many kinds and levels of theory, which bear different relationships to experiment. The history of experiment and theory of the magneto-optical effect illustrates this fact. N. Cartwright's ideas about models and approximations further illustrate the varieties of theory.

13 The creation of phenomena 220 Many experiments create phenomena that did not hitherto exist in a pure state in the universe. Talk of repeating experiments is misleading. Experiments are not repeated but improved until phenomena can be elicited regularly. Some electromagnetic effects illustrate this creation of phenomena.

14 Measurement 233 Measurement has many different roles in sciences. There are measurements to test theories, but there are also pure determinations of the constants of nature. T.S. Kuhn also has an important account of an unexpected functional role of measurement in the growth of knowledge.

15 Baconian topics 246 F. Bacon wrote the first taxonomy of kinds of experiments. He predicted that science would be the collaboration of two different skills – rational and experimental. He thereby answered P. Feyerabend's question, `What's so great about science?' Bacon has a good account of crucial experiments, in which it is plain that they are not decisive. An example from chemistry shows that in practice we cannot in general go on introducing auxiliary hypotheses to save theories refuted by crucial experiments. I. Lakatos's misreports of the Michelson–Morley experiment are used to illustrate the way theory can warp the philosophy of experiment.

i6 Experimentation and scientific realism 262 Experimentation has a life of its own, interacting with speculation, calculation, model building, invention and technology in numerous ways. But whereas the speculator, the calculator, and the model-builder can be anti-realist, the experimenter must be a realist. This
thesis is illustrated by a detailed account of a device that produces concentrated beams of polarized electrons, used to demonstrate violations of parity in weak neutral current interactions. Electrons become tools whose reality is taken for granted. It is not thinking about the world but changing it that in the end must make us scientific realists.

Preface

This book is in two parts. You might like to start with the second half, *Intervening.* It is about experiments. They have been neglected for too long by philosophers of science, so writing about them has to be novel. Philosophers usually think about theories. *Representing is* about theories, and hence it is a partial account of work already in the field. The later chapters of Part A may mostly interest philosophers while some of Part B will be more to a scientific taste. Pick and choose: the analytical table of contents tells what is in each chapter. The arrangement of the chapters is deliberate, but you need not begin by reading them in my order.

I call them introductory topics. They are, for me, literally that. They were the topics of my annual introductory course in the philosophy of science at Stanford University. By `introductory' I do not mean simplified. Introductory topics should be clear enough and serious enough to engage a mind to whom they are new, and also abrasive enough to strike sparks off those who have been thinking about these things for years.

Introduction: rationality

You ask me, which of the philosophers' traits are idiosyncrasies? For example: their lack of historical sense, their hatred of becoming, their Egypticism.
They think that they show their *respect* for a subject when they dehistoricize it — when they turn it into a mummy.

(F. Nietzsche, *The Twilight of the* ***Idols,*** **`Reason in Philosophy', Chapter 1**)

Philosophers long made a mummy of science. When they finally unwrapped the cadaver and saw the remnants of an historical process of becoming and discovering, they created for themselves a crisis of rationality. That happened around 1960.

It was a crisis because it upset our old tradition of thinking that scientific knowledge is the crowning achievement of human reason. Sceptics have always challenged the complacent panorama of cumulative and accumulating human knowledge, but now they took ammunition from the details of history. After looking at many of the sordid incidents in past scientific research, some philosophers began to worry whether reason has much of a role in intellectual confrontation. Is it reason that settles which theory is getting at the truth, or what research to pursue? It became less than clear that reason *ought* to determine such decisions. A few people, perhaps those who already held that morality is culture-bound and relative, suggested that `scientific truth' is a social product with no claim to absolute validity or even relevance.

Ever since this crisis of confidence, rationality has been one of the two issues to obsess philosophers of science. We ask: What do we really know? What should we believe? What is evidence? What are good reasons? Is science as rational as people used to think? Is all this talk of reason only a smokescreen for technocrats? Such questions about ratiocination and belief are traditionally called logic and epistemology. They are *not* what this book is about.

Scientific realism is the other major issue. We ask: What is the world? What kinds of things are in it? What is true of them? What is truth? Are the entities postulated by theoretical physics real, or only

constructs of the human mind for organizing our experiments? These are questions about reality. They are metaphysical. In this book I choose them to organize my introductory topics in the philosophy of science.

Disputes about both reason and reality have long polarized philosophers of science. The arguments are up-to-the-minute, for most philosophical debate about natural science now swirls around one or the other or both. But neither is novel. You will find them in Ancient Greece where philosophizing about science began. I've chosen realism, but rationality would have done as well. The two are intertwined. To fix on one is not to exclude the other.

Is either kind of question important? I doubt it. We do want to know what is really real and what is truly rational. Yet you will find that I dismiss most questions about rationality and am a realist on only the most pragmatic of grounds. This attitude does not diminish my respect for the depths of our need for reason and reality, nor the value of either idea as a place from which to start.

I shall be talking about what's real, but before going on, we should try to see how a `crisis of rationality' arose in recent philosophy of science. This could be `the history of an error'. It is the story of how slightly off-key inferences were drawn from work of the first rank.

Qualms about reason affect many currents in contemporary life, but so far as concerns the philosophy of science, they began in earnest with a famous sentence published twenty years ago:

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed.

Decisive transformation – anecdote or chronology – image of science – possessed – those are the opening words of the famous book by Thomas Kuhn, *The Structure of Scientific Revolutions.* The book itself produced a decisive transformation and unintentionally inspired a crisis of rationality.

A divided image

How could history produce a crisis? In part because of the previous image of mummified science. At first it looks as if there was not exactly one image. Let us take a couple of leading philosophers for

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illustration. Rudolf Carnap and Karl Popper both began their careers in Vienna and fled in the 1930s. Carnap, in Chicago and Los Angeles, and Popper, in London, set the stage for many later debates.

They disagreed about much, but only because they agreed on basics. They thought that the natural sciences are terrific and that physics is the best. It exemplifies human rationality. It would be nice to have a criterion to distinguish such good science from bad nonsense or ill-formed speculation.

Here comes the first disagreement: Carnap thought it is import-ant to make the distinction in terms of language, while Popper thought that the study of meanings is irrelevant to the understanding of science. Carnap said scientific discourse is meaningful; metaphysical talk is not. Meaningful propositions must be *verifiable* in principle, or else they tell nothing about the world. Popper thought that verification was wrong-headed, because powerful scientific theories can never be verified. Their scope is too broad for that. They can, however, be tested, and possibly shown to be false. A proposition is scientific if it is *falsifiable.* In Popper's opinion it is not all that bad to be pre-scientifically metaphysical, for un-falsifiable metaphysics is often the speculative parent of falsifiable science.

The difference here betrays a deeper one. Carnap's verification is from the bottom up: make observations and see how they add up to confirm or verify a more general statement. Popper's falsification is from the top down. First form a theoretical conjecture, and then deduce consequences and test to see if they are true.

Carnap writes in a tradition that has been common since the seventeenth century, a tradition that speaks of the ` inductive sciences'. Originally that meant that the investigator should make precise observations, conduct experiments with care, and honestly record results; then make generalizations and draw analogies and gradually work up to hypotheses and theories, all the time developing new concepts to make sense of and organize the facts. If the theories stand up to subsequent testing, then we know something about the world. We may even be led to the underlying laws of nature. Carnap's philosophy is a twentieth-century version of this attitude. He thought of our observations as the foundations for our knowledge, and he spent his later years trying to invent an

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inductive logic that would explain how observational evidence could support hypotheses of wide application.

There is an earlier tradition. The old rationalist Plato admired geometry and thought less well of the high quality metallurgy, medicine or astronomy of his day. This respect for deduction became enshrined in Aristotle's teaching that real knowledge — science — is a matter of deriving consequences from first principles by means of demonstrations. Popper properly abhors the idea of first principles but he is often called a deductivist. This is because he thinks there is only one logic — deductive logic. Popper agreed with David Hume, who, in 1739, urged that we have at most a psychological propensity to generalize from experience. That gives no reason or basis for our inductive generalizations, no more than a young man's propensity to disbelieve his father is a reason for trusting the youngster rather than the old man. According to Popper, the rationality of science has nothing to do with how well our evidence `supports' our hypotheses. Rationality is a matter of method; that method is conjecture and refutation. Form far-reaching guesses about the world, deduce some observable con-sequences from them. Test to see if these are true. If so, conduct other tests. If not, revise the conjecture or better, invent a new one.

According to Popper, we may say that an hypothesis that has passed many tests is `corroborated'. But this does not mean that it is well supported by the evidence we have acquired. It means only that this hypothesis has stayed afloat in the choppy seas of critical testing. Carnap, on the other hand, tried to produce a theory of confirmation, analysing the way in which evidence makes hypo-theses more probable. Popperians jeer at Carnapians because they have provided no viable theory of confirmation. Carnapians in revenge say that Popper's talk of corroboration is either empty or is a concealed way of discussing confirmation.

Battlefields

Carnap thought that *meanings* and a theory of *language* matter to the philosophy of science. Popper despised them as scholastic. Carnap favoured *verification* to distinguish science from non-science. Popper urged *falsification.* Carnap tried to explicate good reason in terms of a theory of *confirmation;* Popper held that rationality

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consists in *method.* Carnap thought that knowledge has *foundations;* Popper urged that there are no foundations and that all our knowledge is *fallible.* Carnap believed in *induction;* Popper held that there is no logic except *deduction.*

All this makes it look as if there were no standard `image' of science in the decade before Kuhn wrote. On the contrary: whenever we find two philosophers who line up exactly opposite on a series of half a dozen points, we know that in fact they agree about almost everything. They share an image of science, an image rejected by Kuhn. If two people genuinely disagreed about great issues, they would not find enough common ground to dispute specifics one by one.

Common ground

Popper and Carnap assume that natural science is our best example of rational thought. Now let us add some more shared beliefs. What they do with these beliefs differs; the point is that they are shared.

Both think there is a pretty sharp distinction between *observation* and *theory.* Both think that the growth of knowledge is by and large *cumulative.* Popper may be on the lookout for refutations, but he thinks of science as evolutionary and as tending towards the one true theory of the universe. Both think that science has a pretty tight *deductive structure.* Both held that scientific terminology is or ought to be rather *precise.* Both believed in the *unity of science.* That means several things. All the sciences should employ the same methods, so that the human sciences have the same methodology as physics. Moreover, at least the natural sciences are part of one science, and we expect that biology reduces to chemistry, as chemistry reduces to physics. Popper came to think that at least part of psychology and the social world did not strictly reduce to the physical world, but Carnap had no such qualms. He was a founder of a series of volumes under the general title, *The Encyclopedia of Unified Science.*

Both agreed that there is a fundamental difference between the *context of justification* and the *context of discovery.* The terms are due to Hans Reichenbach, a third distinguished philosophical emigre of that generation. In the case of a discovery, historians, economists, sociologists, or psychologists will ask a battery of questions: Who made the discovery? When? Was it a lucky guess, an idea filched

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from a rival, or the pay-off for 20 years of ceaseless toil? Who paid for the research? What religious or social milieu helped or hindered this development? Those are all questions about the context of *discovery.*

Now consider the intellectual end-product: an hypothesis, theory, or belief. Is it reasonable, supported by the evidence, confirmed by experiment, corroborated by stringent testing? These are questions about *justification* or soundness. Philosophers care about justification, logic, reason, soundness, methodology. The historical circumstances of discovery, the psychological quirks, the social interactions, the economic milieux are no professional concern of Popper or Carnap. They use history only for purposes of chronology or anecdotal illustration, just as Kuhn said. Since Popper's account of science is more dynamic and dialectical, it is more congenial to the historicist Kuhn than the flat formalities of Carnap's work on confirmation, but in an essential way, the philosophies of Carnap and Popper are timeless: outside time, outside history.

Blurring an image

Before explaining why Kuhn dissents from his predecessors, we can easily generate a list of contrasts simply by running across the Popper/Carnap common ground and denying everything. Kuhn holds:

There is no sharp distinction between observation and theory. Science is not cumulative.

A live science does not have a tight deductive structure. Living scientific concepts are not particularly precise. Methodological unity of science is false: there are lots of disconnected tools used for various kinds of inquiry.

The sciences themselves are disunified. They are composed of a large number of only loosely overlapping little disciplines many of which in the course of time cannot even comprehend each other. (Ironically Kuhn's best-seller appeared in the moribund series, *The Encyclopedia of Unified Science.)*

The context of justification cannot be separated from the context of discovery.

Science is in time, and is essentially historical.

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Is reason in question?

I have so far ignored the first point on which Popper and Carnap agree, namely that natural science is the paragon of rationality, the gemstone of human reason. Did Kuhn think that science is irrational? Not exactly. That is not to say he took it to be `rational' either. I doubt that he had much interest in the question.

We now must run through some main Kuhnian themes, both to understand the above list of denials, and to see how it all bears on rationality. Do not expect him to be quite as alien to his predecessors as might be suggested. Point-by-point opposition between philosophers indicates underlying agreement on basics, and in some respects Kuhn is point-by-point opposed to Carnap-Popper.

Normal science

Kuhn's most famous word was *paradigm,* of which more anon. First we should think about Kuhn's tidy structure of revolution: *normal science, crisis, revolution, new normal science.*

The normal science thesis says that an established branch of science is mostly engaged in relatively minor tinkering with current theory. Normal science is *puzzle-solving.* Almost any well-workedout theory about anything will somewhere fail to mesh with facts about the world – ` Every theory is born refuted'. Such failures in an otherwise attractive and useful theory are *anomalies.* One hopes that by rather minor modifications the theory may be mended so as to explain and remove these small counterexamples. Some normal science occupies itself with mathematical articulation of theory, so that the theory becomes more intelligible, its consequences more apparent, and its mesh with natural phenomena more intricate. Much normal science is technological application. Some normal science is the experimental elaboration and clarification of facts implied in the theory. Some normal science is refined measurement of quantities that the theory says are important. Often the aim is simply to get a precise number by ingenious means. This is done neither to test nor confirm the theory. Normal science, sad to say, is not in the confirmation, verification, falsification or conjecture-andrefutation business at all. It does, on the other hand, constructively accumulate a body of knowledge and concepts in some domain.

Crisis and revolution

Sometimes anomalies do not go away. They pile up. A few may come to seem especially pressing. They focus the energies of the livelier members of the research community. Yet the more people work on the failures of the theory, the worse things get. Counter-examples accumulate. An entire theoretical perspective becomes clouded. The discipline is in *crisis.* One possible outcome is an entirely new approach, employing novel concepts. The problematic phenomena are all of a sudden intelligible in the light of these new ideas. Many workers, perhaps most often the younger ones, are converted to the new hypotheses, even though there may be a few hold-outs who may not even understand the radical changes going on in their field. As the new theory makes rapid progress, the older ideas are put aside. *A revolution* has occurred.

The new theory, like any other, is born refuted. A new generation of workers gets down to the anomalies. There is a new normal science. Off we go again, puzzle-solving, making applications, articulating mathematics, elaborating experimental phenomena, measuring.

The new normal science may have interests quite different from the body of knowledge that it displaced. Take the least contentious example, namely measurement. The new normal science may single out different things to measure, and be indifferent to the precise measurements of its predecessor. In the nineteenth century analytical chemists worked hard to determine atomic weights. Every element was measured to at least three places of decimals. Then around 1920 new physics made it clear that naturally occurring elements are mixtures of isotopes. In many practical affairs it is still useful to know that earthly chlorine has atomic weight 35.453. But this is a largely fortuitous fact about our planet. The deep fact is that chlorine has two stable isotopes, 35 and 37. (Those are not the exact numbers, because of a further factor called binding energy.) These isotopes are mixed here on earth in the ratios 75.53% and 24.47%.

`Revolution' is not novel

The thought of a scientific revolution is not Kuhn's. We have long had with us the idea of the Copernican revolution, or of the `scientific revolution' that transformed intellectual life in the

seventeenth century. In the second edition of his *Critique of Pure Reason (1787),* Kant speaks of the 'intellectual revolution' by which Thales or some other ancient transformed empirical mathematics into demonstrative proof. Indeed the idea of revolution in the scientific sphere is almost coeval with that of political revolution. Both became entrenched with the French Revolution (1789) and the revolution in chemistry (1785, say). That was not the beginning, of course. The English had had their `glorious revolution' (a bloodless one) in 1688 just as it became realized that a scientific revolution was also occurring in the minds of men and women.'

Under the guidance of Lavoisier the phlogiston theory of combustion was replaced by the theory of oxidation. Around this time there was, as Kuhn has emphasized, a total transformation in many chemical concepts, such as mixture, compound, element, substance and the like. To understand Kuhn properly we should not fixate on grand revolutions like that. It is better to think of smaller revolutions in chemistry. Lavoisier taught that oxygen is the principle of acidity, that is, that every acid is a compound of oxygen. One of the most powerful of acids (then or now) was called muriatic acid. In 1774 it was shown how to liberate a gas from this. The gas was called dephlogisticated muriatic acid. After 1785 this very gas was inevitably renamed oxygenized muriatic acid. By 1811

Humphry Davy showed this gas is an element, namely chlorine. Muriatic acid is our hydrochloric acid, HCL It contains no oxygen. The Lavoisier conception of acidity was thereby overthrown. This event was, in its day, quite rightly called a revolution. It even had the Kuhnian feature that there were hold-outs from the old school. The greatest analytical chemist of Europe, J.J. Berzelius *0779-^{18}4^{8}),* never publicly acknowledged that chlorine was an element, and not a compound of oxygen.

The idea of scientific revolution does not in itself call in question scientific rationality. We have had the idea of revolution for a long time, yet still been good rationalists. But Kuhn invites the idea that every normal science has the seeds of its own destruction. Here is an idea of perpetual revolution. Even that need not be irrational. Could Kuhn's idea of a revolution as switching `paradigms' be the challenge to rationality?

((footnote:))

1 I. B. Cohen, `The eighteenth century origins of the concept of scientific revolution', *journal for the History of Ideas 37 (1976),* Pp. *257-88.*

Paradigm-as-achievement

`Paradigm' has been a vogue word of the past twenty years, all thanks to Kuhn. It is a perfectly good old word, imported directly from Greek into English 500 years ago. It means a pattern, exemplar, or model. The word had a technical usage. When you learn a foreign language by rote you learn for example how to conjugate *amare* (to love) as *amo, amas, amat ...,* and then conjugate verbs of this class following this modcl, called the paradigm. A saint, on whom we might pattern our lives, was also called a paradigm. This is the word that Kuhn rescued from obscurity.

It has been said that in *Structure* Kuhn used the word `paradigm' in 22 different ways. He later focussed on two meanings. One is the paradigm-as-achievement. At the time of a revolution there is usually some exemplary success in solving an old problem in a completely new way, using new concepts. This success serves as a model for the next generation of workers, who try to tackle other problems in the same way. There is an element of rote here, as in the conjugation of Latin verbs ending in *-are.* There is also a more liberal element of modelling, as when one takes one's favourite saint for one's paradigm, or role-model. The paradigm-as-achievement is the role-model of a normal science.

Nothing in the idea of paradigm-as-achievement speaks against scientific rationality — quite the contrary.

Paradigm-as-set-of-shared-values

When kuhn writes of science he does not usually mean the vast engine of modern science but rather small groups of research workers who carry forward one line of inquiry. He has called this a disciplinary matrix, composed of interacting research groups with common problems and goals. It might number a hundred or so people in the forefront, plus students and assistants. Such a group can often be identified by an ignoramus, or a sociologist, knowing nothing of the science. The know-nothing simply notes who corresponds with whom, who telephones, who is on the preprint lists, who is invited to the innumerable specialist disciplinary gatherings where front-line information is exchanged years before

it is published. Shared clumps of citations at the ends of published papers are a good clue. Requests for money are refereed by `peer reviewers'. Those peers are a rough guide to the disciplinary matrix within one country, but such matrixes are often international.

Within such a group there is a shared set of methods, standards, and basic assumptions. These are passed on to students, inculcated in textbooks, used in deciding what research is supported, what problems matter, what solutions are admissible, who is promoted, who referees papers, who publishes, who perishes. This is a paradigm-as-set-of-shared-values.

The paradigm-as-set-of-shared-values is so intimately linked to paradigm-as-achievement that the single word 'paradigm' remains a natural one to use. One of the shared values is the achievement. The achievement sets a standard of excellence, a model of research, and a class of anomalies about which it is rewarding to puzzle. Here `rewarding' is ambiguous. It means that within the conceptual constraints set by the original achievement, this kind of work is intellectually rewarding. It also means that this is the kind of work that the discipline rewards with promotion, finance, research students and so forth.

Do we finally scent a whiff of irrationality? Are these values merely social constructs? Are the rites of initiation and passage just the kind studied by social anthropologists in parts of our own and other cultures that make no grand claims to reason? Perhaps, but so what? The pursuit of truth and reason will doubtless be organized according to the same social formulae as other pursuits such as happiness or genocide. The fact that scientists are people, and that scientific societies are societies, does not cast doubt, yet, upon scientific rationality.

Conversion

The threat to rationality comes chiefly from Kuhn's conception of revolutionary shift in paradigms. He compares it to religious conversion, and to the phenomenon of a gestalt-switch. If you draw a perspective figure of a cube on a piece of paper, you can see it as now facing one way, now as facing another way. Wittgenstein used a figure that can be seen now as a rabbit, now as a duck. Religious conversion is said to be a momentous version of a similar pheno-

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menon, bringing with it a radical change in the way in which one feels about life.

Gestalt-switches involve no reasoning. There can be reasoned religious conversion — a fact perhaps more emphasized in a catholic tradition than a protestant one. Kuhn seems to have the `born-again' view instead. He could also have recalled Pascal, who thought that a good way to become a believer was to live among believers, mindlessly engaging in ritual until it is true.

Such reflections do not show that a non-rational change of belief might not also be a switch from the less reasonable to the more reasonable doctrine. Kuhn is himself inciting us to make a gestalt-switch, to stop looking at development in science as subject solely to the old canons of rationality and logic. Most importantly he suggests a new picture: after a paradigm shift, members of the new disciplinary matrix `live in a different world' from their predecessors.

Incommensurability

Living in a different world seems to imply an important con-sequence. We might like to compare the merits of an old paradigm with those of a successor. The revolution was reasonable only if the new theory fits the known facts better than the old one. Kuhn suggests instead that you may not even be able to express the ideas of the old theory in the language of the new one. A new theory is a new language. There is literally no way of finding a theory-neutral language in which to express, and then compare the two.

Complacently, we used to assume that a successor theory would take under its wing the discoveries of its predecessor. In Kuhn's view it may not even be able to express those discoveries. Our old picture of the growth of knowledge was one of accumulation of knowledge, despite the occasional setback. Kuhn says that although any one normal science may be cumulative, science is not in general that way. Typically after a revolution a big chunk of some chemistry or biology or whatever will be forgotten, accessible only to the historian who painfully acquires a discarded world-view. Critics will of course disagree about how 'typical' this is. They will hold — with some justice — that the more typical case is the one where, for

example, quantum theory of relativity takes classical relativity under its wing.

Objectivity

Kuhn was taken aback by the way in which his work (and that of others) produced a crisis of rationality. He subsequently wrote that he never intended to deny the customary virtues of scientific theories. Theories should be accurate, that is, by and large fit existing experimental data. They should be both internally consistent and consistent with other accepted theories. They should be broad in scope and rich in consequences. They should be simple in structure, organizing facts in an intelligible way. They should be fruitful, disclosing new events, new techniques, new relationships. Within a normal science, crucial experiments deciding between rival hypotheses using the same concepts may be rare, but they are not impossible.

Such remarks seem a long way from the popularized Kuhn of *Structure.* But he goes on to make two fundamental points. First, his five values and others of the same sort are never sufficient to make a decisive choice among competing theories. Other qualities of judgement come into play, qualities for which there could, in principle, be no formal algorithm. Secondly:

> Proponents of different theories are, I have claimed, native speakers of different languages.... I simply assert the existence of significant limits to what the proponents of different theories can communicate to each other ....Nevertheless, despite the incompleteness of their communication, proponents of different theories can exhibit to each other, not always easily, the concrete technical results available by those who practice within each theory.[2]

When you do buy into a theory, Kuhn continues, you `begin to speak the language like a native. No process quite like choice has occurred', but you end up speaking the language like a native nonetheless. You don't have two theories in mind and compare them point by point — they are too different for that. You gradually convert, and that shows itself by moving into a new language community.

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2 `Objectivity, value judgment, and theory choice', in T.S. Kuhn, *The Essential Tension,* Chicago, 1977, PP. 320—39.

sometimes irrational (as well as being idle, reckless, confused, unreliable). Aristotle taught that humans are rational animals, which meant that they are able to reason. We can assent to that without thinking that 'rational' is an evaluative word. Only `irrational', in our present language, is evaluative, and it may mean nutty, unsound, vacillating, unsure, lacking self-knowledge, and much else. The `rationality' studied by philosophers of science holds as little charm for me as it does for Feyerabend. Reality is more fun, not that `reality' is any better word. Reality ... what a concept.

Be that as it may, see how historicist we have become. Laudan draws his conclusions `from the existing historical evidence'. The discourse of the philosophy of science has been transformed since the time that Kuhn wrote. No longer shall we, as Nietzsche put it, show our respect for science by dehistoricizing it.

Rationality and scientific realism

So much for standard introductory topics in the philosophy of science that will *not* be discussed in what follows. But of course reason and rationality are not so separable. When I do take up matters mentioned in this introduction, the emphasis is always on realism. Chapter 5 is about incommensurability, but only because it contains the germs of irrealism. Chapter 8 is about Lakatos, often regarded as a champion of rationality, but he occurs here because I think he is showing one way to be a realist without a correspondence theory of truth.

Other philosophers bring reason and reality closer together. Laudan, for example, is a rationalist who attacks realist theories. This is because many wish to use realism as the basis of a theory of rationality, and Laudan holds that to be a terrible mistake. In the end I come out for a sort of realism, but this is not at odds with Laudan, for I would never use realism as a foundation for ` rationality'.

Conversely Hilary Putnam begins a 1982 book, *Reason, Truth and History,* by urging `that there is an extremely close connection between the notions of *truth* and *rationality'.* (Truth is one heading under which to discuss scientific realism.) He continues, `to put it even more crudely, the only criterion for what is a fact is what it is *rational* to accept' (p. x). Whether Putnam is right or wrong,

Nietzsche once again seems vindicated. Philosophy books in English once had titles such as A.J. Ayer's 1936 *Language, Truth and Logic.* In 1982 we have *Reason, Truth and History.*

It is not, however, history that we are now about to engage in. I shall use historical examples to teach lessons, and shall assume that knowledge itself is an historically evolving entity. So much might be part of a history of ideas, or intellectual history. There is a simpler, more old-fashioned concept of history, as history not of what we think but of what we do. That is not the history of ideas but history (without qualification). I separate reason and reality more sharply than do Laudan and Putnam, because I think that reality has more to do with what we do in the world than with what we think about it.

long chains of molecules are really there to be spliced. Biologists may think more clearly about an amino acid if they build a molecular model out of wire and coloured balls. The model may help us arrange the phenomena in our minds. It may suggest new microtechnology, but it is not a literal picture of how things really are. I could make a model of the economy out of pulleys and levers and ball bearings and weights. Every decrease in weight M (the `money supply') produces a decrease in angle I (the `rate of inflation') and an increase in the number N of ball bearings in this pan (the number of unemployed workers). We get the right inputs and outputs, but no one suggests that this is what the economy is.

If you can spray them, then they are real

For my part I never thought twice about scientific realism until a friend told me about an ongoing experiment to detect the existence of fractional electric charges. These are called quarks. Now it is not the quarks that made me a realist, but rather electrons. Allow me to tell the story. It ought not to be a simple story, but a realistic one, one that connects with day to day scientific research. Let us start with an old experiment on electrons.

The fundamental unit of electric charge was long thought to be the electron. In 1908 J.A. Millikan devised a beautiful experiment to measure this quantity. A tiny negatively charged oil droplet is suspended between electrically charged plates. First it is allowed to fall with the electric field switched off. Then the field is applied to hasten the rate of fall. The two observed terminal velocities of the droplet are combined with the coefficient of viscosity of the air and the densities of air and oil. These, together with the known value of gravity, and of the electric field, enable one to compute the charge on the drop. In repeated experiments the charges on these drops are small integral multiples of a definite quantity. This is taken to be the minimum charge, that is, the charge on the electrons. Like all experiments, this one makes assumptions that are only roughly correct: that the drops are spherical, for instance. Millikan at first ignored the fact that the drops are not large compared to the mean free path of air molecules so they get bumped about a bit. But the idea of the experiment is definitive.

The electron was long held to be the unit of charge. We use e as the name of that charge. Small particle physics, however, increasingly suggests an entity, called a quark, that has a charge of 113 *e*. Nothing in theory suggests that quarks have independent existence; if they do come into being, theory implies, then they react immediately and are gobbled up at once. This has not deterred an ingenious experiment started by LaRue, Fairbank and Hebard at Stanford. They are hunting for `free' quarks using Millikan's basic idea.

Since quarks may be rare or short-lived, it helps to have a big ball rather than a tiny drop, for then there is a better chance of having a quark stuck to it. The drop used, although weighing less than 10^{-4} grams, is times bigger than Millikan's drops. If it were made of oil it would fall like a stone, almost. Instead it is made of a substance called niobium, which is cooled below its superconducting transition temperature of 9°K. Once an electric charge is set going round this very cold ball, it stays going, forever. Hence the drop can be kept afloat in a magnetic field, and indeed driven back and forth by varying the field. One can also use a magnetometer to tell exactly where the drop is and how fast it is moving.

The initial charge placed on the ball is gradually changed, and, applying our present technology in a Millikan-like way, one determines whether the passage from positive to negative charge occurs at zero or at ± 113 *e*. If the latter, there must surely be one loose quark on the ball. In their most recent preprint, Fairbank and his associates report four fractional charges consistent with + 113 *e*, four with *-113 e*, and 13 with zero.

Now how does one alter the charge on the niobium ball? `Well, at that stage,' said my friend, `we spray it with positrons to increase the charge or with electrons to decrease the charge.' From that day forth I've been a scientific realist. *So far as I'm concerned, if you can spray them then they are real.*

Long-lived fractional charges are a matter of controversy. It is not quarks that convince me of realism. Nor, perhaps, would I have been convinced about electrons in 1908. There were ever so many more things for the sceptic to find out: There was that nagging worry about inter-molecular forces acting on the oil drops. Could that be what Millikan was actually measuring? So that his numbers showed nothing at all about so-called electrons? If so, Millikan goes no way towards showing the reality of electrons. Might there be minimum electric charges, but no electrons? In our quark example

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we have the same sorts of worry. Marinelli and Morpurgo, in a recent preprint, suggest that Fairbank's people are measuring a new electromagnetic force, not quarks. What convinced me of realism has nothing to do with quarks. It was the fact that by now there are standard emitters with which we can spray positrons and electrons – and that is precisely what we do with them. We understand the effects, we understand the causes, and we use these to find out something else. The same of course goes for all sorts of other tools of the trade, the devices for getting the circuit on the supercooled niobium ball and other almost endless manipulations of the `theoretical'.

What is the argument about?

The practical person says: consider what you use to do what you do. If you spray electrons then they are real. That is a healthy reaction but unfortunately the issues cannot be so glibly dismissed. Anti-realism may sound daft to the experimentalist, but questions about realism recur again and again in the history of knowledge. In addition to serious verbal difficulties over the meanings of `true' and `real', there are substantive questions. Some arise from an intertwining of realism and other philosophies. For example, realism has, historically, been mixed up with materialism, which, in one version, says everything that exists is built up out of tiny material building blocks. Such a materialism will be realistic about atoms, but may then be anti-realistic about `immaterial' fields of force. The dialectical materialism of some orthodox Marxists gave many modern theoretical entities a very hard time. Lysenko rejected Mendelian genetics partly because he doubted the reality of postulated `genes'.

Realism also runs counter to some philosophies about causation. Theoretical entities are often supposed to have causal powers: electrons neutralize positive charges on niobium balls. The original nineteenth-century positivists wanted to do science without ever speaking of' causes', so they tended to reject theoretical entities too. This kind of anti-realism is in full spate today.

Anti-realism also feeds on ideas about knowledge. Sometimes it arises from the doctrine that we can know for real only the subjects of sensory experience. Even fundamental problems of logic get

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involved; there is an anti-realism that puts in question what it is for theories to be true or false.

Questions from the special sciences have also fuelled controversy. Old-fashioned astronomers did not want to adopt a realist attitude to Copernicus. The idea of a solar system might help calculation, but it does not say how the world really is, for the earth, not the sun, they insisted, is the centre of the universe. Again, should we be realists about quantum mechanics? Should we realistically say that particles do have a definite although unknowable position and momentum? Or at the opposite extreme should we say that the `collapse of the wave packet' that occurs during microphysical measurement is an interaction with the human mind?

Nor shall we find realist problems only in the specialist natural sciences. The human sciences give even more scope for debate. There can be problems about the libido, the super ego, and the transference of which Freud teaches. Might one use psychoanalysis to understand oneself or another, yet cynically think that nothing answers to the network of terms that occurs in the theory? What should we say of Durkheim's supposition that there are real, though by no means distinctly discernible, social processes that act upon us as inexorably as the laws of gravity, and yet which exist in their own right, over and above the properties of the individuals that constitute society? Could one coherently be a realist about sociology and an anti-realist about physics, or vice versa?

Then there are meta-issues. Perhaps realism is as pretty an example as we could wish for, of the futile triviality of basic philosophical reflections. The questions, which first came to mind in antiquity, are serious enough. There was nothing wrong with asking, once, Are atoms real? But to go on discussing such a question may be only a feeble surrogate for serious thought about the physical world.

That worry is anti-philosophical cynicism. There is also philosophical anti-philosophy. It suggests that the whole family of issues about realism and anti-realism is mickey-mouse, founded upon a prototype that has dogged our civilization, a picture of knowledge `representing' reality. When the idea of correspondence between thought and the world is cast into its rightful place – namely, the grave – will not, it is asked, realism and anti-realism quickly follow?

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Movements, not doctrines

Definitions of `scientific realism' merely point the way. It is more an attitude than a clearly stated doctrine. It is a way to think about the content of natural science. Art and literature furnish good comparisons, for not only has the word `realism' picked up a lot of philosophical connotations: it also denotes several artistic movements. During the nineteenth century many painters tried to escape the conventions that bound them to portray ideal, romantic, historical or religious topics on vast and energetic canvases. They chose to paint scenes from everyday life. They refused to ` aestheticize' a scene. They accepted material that was trivial or banal. They refused to idealize it, refused to elevate it: they would not even make their pictures picturesque. Novelists adopted this realist stance, and in consequence we have the great tradition in French literature that passes through Flaubert and which issues in Zola's harrowing descriptions of industrial Europe. To quote an un-sympathetic definition of long ago, `a realist is one who deliberately declines to select his subjects from the beautiful or harmonious, and, more especially, describes ugly things and brings out details of the unsavoury sort'.

Such movements do not lack doctrines. Many issued manifestos. All were imbued with and contributed to the philosophical sensibilities of the day. In literature some latterday realism was called positivism. But we speak of movements rather than doctrine, of creative work sharing a family of motivations, and in part defining itself in opposition to other ways of thinking. Scientific realism and anti-realism are like that: they too are movements. We can enter their discussions armed with a pair of one-paragraph definitions, but once inside we shall encounter any number of competing and divergent opinions that comprise the philosophy of science in its present excited state.

Truth and real existence

With misleading brevity I shall use the term `theoretical entity' as a portmanteau word for all that ragbag of stuff postulated by theories but which we cannot observe. That means, among other things, particles, fields, processes, structures, states and the like. There are two kinds of scientific realism, one for theories, and one for entities.

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The question about theories is whether they are true, or are trueor-false, or are candidates for truth, or aim at the truth. The question about entities is whether they exist.

A majority of recent philosophers worries most about theories and truth. It might seem that if you believe a theory is true, then you automatically believe that the entities of the theory exist. For what is it to think that a theory about quarks is true, and yet deny that there are any quarks? Long ago Bertrand Russell showed how to do that. He was not, then, troubled by the truth of theories, but was worried about unobservable entities. He thought we should use logic to rewrite the theory so that the supposed entities turn out to be logical constructions. The term `quark' would not denote quarks, but would be shorthand, via logic, for a complex expression which makes reference only to observed phenomena. Russell was then a realist about theories but an anti-realist about entities.

It is also possible to be a realist about entities but an anti-realist about theories. Many Fathers of the Church exemplify this. They believed that God exists, but they believed that it was in principle impossible to form any true positive intelligible theory about God. One could at best run off a list of what God is not – not finite, not limited, and so forth. The scientific-entities version of this says we have good reason to suppose that electrons exist, although no full-fledged description of electrons has any likelihood of being true. Our theories are constantly revised; for different purposes we use different and incompatible models of electrons which one does not think are literally true, but there are electrons, nonetheless.

Two realisms

Realism about entities says that a good many theoretical entities really do exist. Anti-realism denies that, and says that they are fictions, logical constructions, or parts of an intellectual instrument for reasoning about the world. Or, less dogmatically, it may say that we have not and cannot have any reason to suppose they are not fictions. They may exist, but we need not assume that in order to understand the world.

Realism about theories says that scientific theories are either true or false independent of what we know: science at least aims at the truth, and the truth is how the world is. Anti-realism says that

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theories are at best warranted, adequate, good to work on, acceptable but incredible, or what-not.

Subdivisions

I have just run together claims about reality and claims about what we know. My realism about entities implies both that a satisfactory theoretical entity would be one that existed (and was not merely a handy intellectual tool). That is a claim about entities and reality. It also implies that we actually know, or have good reason to believe in, at least some such entities in present science. That is a claim about knowledge.

I run knowledge and reality together because the whole issue would be idle if we did not *now* have some entities that some of us think really do exist. If we were talking about some future scientific utopia I would withdraw from the discussion. The two strands that I run together can be readily unscrambled, as in the following scheme of W. Newton-Smith's.' He notes three ingredients in scientific realism:

i An *ontological* ingredient: scientific theories are either true or false, and that which a given theory is, is in virtue of how the world is.

2 A *causal* ingredient: if a theory is true, the theoretical terms of the theory denote theoretical entities which are causally responsible for the observable phenomena.

3 An *epistemological* ingredient: we can have warranted belief in theories or in entities (at least in principle).

Roughly speaking, Newton-Smith's causal and epistemological ingredients add up to my realism about entities. Since there are two ingredients, there can be two kinds of anti-realism. One rejects (1); the other rejects (3).

You might deny the ontological ingredient. You deny that theories are to be taken literally; they are not either true or false; they are intellectual tools for predicting phenomena; they are rules for working out what will happen in particular cases. There are many versions of this. Often an idea of this sort is called *instrumentalism* because it says that theories are only instruments.

Instrumentalism denies (i). You might instead deny (3). An

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W. Newton-Smith, The underdetermination of theory by data', *Proceedings of the Aristotelian Society,* Supplementary Volume 52 (1978), p. 72.

example is Bas van Fraassen in his book *The Scientific Image (1980).* He thinks theories are to be taken literally – there is no other way to take them. They are either true or false, and which they are depends on the world – there is no alternative semantics. But we have no warrant or need to believe any theories about the unobservable in order to make sense of science. Thus he denies the epistemological ingredient.

My realism about theories is, then, roughly (1) and (3), but my realism about entities is not exactly (2) and (3). Newton-Smith's causal ingredient says that if a theory is true, then the theoretical terms denote entities that are causally responsible for what we can observe. He implies that belief in such entities depends on belief in a theory in which they are embedded. But one can believe in some entities without believing in any particular theory in which they are embedded. One can even hold that no general deep theory about the entities could possibly be true, for there is no such truth. Nancy Cartwright explains this idea in her book *How the Laws of Physics Lie* (1983). She means the title literally. The laws are deceitful. Only phenomenological laws are possibly true, but we may well know of causally effective theoretical entities all the same.

Naturally all these complicated ideas will have an airing in what follows. Van Fraassen is mentioned in numerous places, especially Chapter *3.* Cartwright comes up in Chapter 2 and Chapter 12. The overall drift of this book is away from realism about theories and towards realism about those entities we can use in experimental work. That is, it is a drift away from representing, and towards intervening.

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Metaphysics and the special sciences

We should also distinguish realism-in-general from realism-inparticular.

To use an example from Nancy Cartwright, ever since Einstein's work on the photoelectric effect the photon has been an integral part of our understanding of light. Yet there are serious students of optics, such as Willis Lamb and his associates, who challenge the reality of photons, supposing that a deeper theory would show that the photon is chiefly an artifact of our present theories. Lamb is not saying that the extant theory of light is plain false. A more profound theory would preserve most of what is now believed about light, but

would show that the effects we associate with photons yield, on analysis, to a different aspect of nature. Such a scientist could well be a realist in general, but an anti-realist about photons in particular.

Such localized anti-realism is a matter for optics, not philosophy. Yet N.R. Hanson noticed a curious characteristic of new departures in the natural sciences. At first an idea is proposed chiefly as a calculating device rather than a literal representation of how the world is. Later generations come to treat the theory and its entities in an increasingly realistic way. (Lamb is a sceptic proceeding in the opposite direction.) Often the first authors are ambivalent about their entities. Thus James Clerk Maxwell, one of the creators of statistical mechanics, was at first loth to say whether a gas really is made up of little bouncy balls producing effects of temperature pressure. He began by regarding this account as a `mere' model, which happily organizes more and more macroscopic phenomena. He became increasingly realist. Later generations apparently regard kinetic theory as a good sketch of how things really are. It is quite common in science for anti-realism about a particular theory or its entities to give way to realism.

Maxwell's caution about the molecules of a gas was part of a general distrust of atomism. The community of physicists and chemists became fully persuaded of the reality of atoms only in our century. Michael Gardner has well summarized some of the strands that enter into this story.[2] It ends, perhaps, when Brownian motion was fully analysed in terms of molecular trajectories. This feat was important not just because it suggested in detail how molecules were bumping into pollen grains, creating the observable move-ment. The real achievement was a new way to determine Avogadro's number, using Einstein's analysis of Brownian motion and Jean Perrin's experimental techniques.

That was of course a `scientific', not a `philosophical', discovery. Yet realism about atoms and molecules was once the central issue for philosophy of science. Far from being a local problem about one kind of entity, atoms and molecules were the chief candidates for real (or merely fictional) theoretical entities. Many of our present positions on scientific realism were worked out then, in connection

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2 M. Gardner, `Realism and instrumentalism in 19th century atomism', *Philosophy of Science 46* (1979), pp. 1-34.

with that debate. The very name ` scientific realism' came into use at that time.

Realism-in-general is thus to be distinguished from realism-inparticular, with the proviso that a realism-in-particular can so dominate discussion that it determines the course of realism-ingeneral. A question of realism-in-particular is to be settled by research and development of a particular science. In the end the sceptic about photons or black holes has to put up or shut up. Realism-in-general reverberates with old metaphysics and recent philosophy of language. It is vastly less contingent on facts of nature than any realism-in-particular. Yet the two are not fully separable and often, in formative stages of our past, have been intimately combined.

Representation and intervention

Science is said to have two aims: theory and experiment. Theories try to say how the world is. Experiment and subsequent technology change the world. We represent and we intervene. We represent in order to intervene, and we intervene in the light of representations. Most of today's debate about scientific realism is couched in terms of theory, representation, and truth. The discussions are illuminating but not decisive. This is partly because they are so infected with intractable metaphysics. I suspect there can be no final argument for or against realism at the level of representation. When we turn from representation to intervention, to spraying niobium balls with positrons, anti-realism has less of a grip. In what follows I start with a somewhat old-fashioned concern with realism about entities. This soon leads to the chief modern studies of truth and representation, of realism and anti-realism about theories. Towards the end I shall come back to intervention, experiment, and entities.
The final arbitrator in philosophy is not how we think but what we do.

2 ***Building and causing***

Does the word `real' have any use in natural science? Certainly. Some experimental conversations are full of it. Here are two real examples. The cell biologist points to a fibrous network that regularly is found on micrographs of cells prepared in a certain way. It looks like chromatin, namely the stuff in the cell nucleus full of fundamental proteins. It stains like chromatin. But it is not real. It is only an artifact that results from the fixation of nucleic sap by glutaraldehyde. We do get a distinctive reproduction pattern, but it has nothing to do with the cell. It is an artifact of the preparation.'

To turn from biology to physics, some critics of quark-hunting don't believe that Fairbank and his colleagues have isolated long-lived fractional charges. The results may be important but the free quarks aren't real. In fact one has discovered something quite different; a hitherto unknown new electromagnetic force.

What does `real' mean, anyway? The best brief thoughts about the word are those of J.L. Austin, once the most powerful philosophical figure in Oxford, where he died in 1960 at the age of 49. He cared deeply about common speech, and thought we often prance off into airy-fairy philosophical theories without recollect-ing what we are saying. In Chapter 7 of his lectures, *Sense and Sensibilia,* he writes about reality: `We must not dismiss as beneath contempt such humble but familiar phrases as "not real cream ".' That was his first methodological rule. His second was not to look for ` one single specifiable always-the-same *meaning'*. He is warning us against looking for synonyms, while at the same time urging systematic searches for regularities in the usage of a word.

He makes four chief observations about the word `real'. Two of these seem to me to be important even though they are expressed somewhat puckishly. The two right remarks are that the word ` real'

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1 For example, R.J. Skaer and S. Whytock, `Chromatin-like artifacts from nuclear *sap*', *Journal of Cell Science 26* (1977), pp. 301-5.

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is substantive-hungry: hungry for nouns. The word is also what Austin, in a genially sexist way, calls a trouser-word.

The word is hungry for nouns because `that's real' demands a noun to be properly understood: real cream, a real constable, a real Constable.

`Real' is called a trouser-word because of negative uses of the words `wear the trousers'. Pink cream is pink, the same colour as a pink flamingo. But to call some stuff real cream is not to make the same sort of positive assertion. Real cream is, perhaps, not a non-dairy coffee product. Real leather is hide, not naugehyde, real diamonds are not paste, real ducks are not decoys, and so forth. The force of `real S' derives from the negative `not (a) real S'. Being hungry for nouns and being a trouser-word are connected. To know what wears the trousers we have to know the noun, in order that we can tell what is being denied in a negative usage. Real telephones are, in a certain context, not toys, in another context, not imitations, or not purely decorative. This is not because the word is ambiguous, but because whether or not something is a real N depends upon the N in question. The word ` real' is regularly doing the same work, but you have to look at the $N$ to see what work is being done. The word ` real' is like a migrant farm worker whose work is clear: to pick the present crop. But what is being picked? Where is it being picked? How is it being picked? That depends on the crop, be it lettuce, hops, cherries or grass.

In this view the word `real' is not ambiguous between `real chromatin', `real charge', and `real cream'. One important reason for urging this grammatical point is to discourage the common idea that there *must* be different kinds of reality, just because the word is used in so many ways. Well, perhaps there are different kinds of reality. I don't know, but let not a hasty grammar force us to conclude there are different kinds of reality. Moreover we now must force the philosopher to make plain what contrast is being made by the word `real' in some specialized debate. If theoretical entities are, or are not, real entities, what contrast is being made?

Materialism

J.J.C. Smart meets the challenge in his book, *Philosophy and Scientific Realism* (1963). Yes, says Smart, `real' should mark a contrast. Not all theoretical entities are real. `Lines of force, unlike

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electrons, *are* theoretical fictions. I wish to say that this table is composed of electrons, etc., just as this wall is composed of bricks' (p. 36). A swarm of bees is made up of bees, but nothing is made up of lines of force. There is a definite number of bees in a swarm and of electrons in a bottle, but there is no definite number of lines of magnetic force in a given volume; only a convention allows us to count them.

With the physicist Max Born in mind, Smart say that the anti-realist holds that electrons do not occur in the series: `stars, planets, mountains, houses, tables, grains of wood, microscopic crystals, microbes'. On the contrary, says Smart, crystals *are* made up of molecules, molecules of atoms, and atoms are made up of electrons, among other things. So, infers Smart, the anti-realist is wrong. There are at least some real theoretical entities. On the other hand, the word `real' marks a significant distinction. In Smart's account, lines of magnetic force are not real.

Michael Faraday, who first taught us about lines of force, did not agree with Smart. At first he thought that lines of force are indeed a mere intellectual tool, a geometrical device without any physical significance. In 1852, when he was over 6o, Faraday changed his mind. ` I cannot conceive curved lines of force without the condition of physical existence in that intermediate space.'[2] He had come to realize that it is possible to exert a stress on the lines of force, so they had, in his mind, to have real existence. `There can be no doubt,' writes his biographer, ` that Faraday was firmly convinced that lines of force were real.' This does not show that Smart is mistaken. It does however remind us that some physical conceptions of reality pass beyond the rather simplistic level of building blocks.

Smart is *a materialist* – he himself now prefers the term physicalist. I do not mean that he insists that electrons are brute matter. By now the older ideas of matter have been replaced by more subtle notions. His thought remains, however, based on the idea that material things like stars and tables are built up out of electrons and so forth. The anti-materialist, Berkeley, objecting to the corpuscles of Robert Boyle and Isaac Newton, was rejecting just such a picture. Indeed Smart sees himself as opposed to phenomenalism, a modern version of Berkeley's immaterialism. It is perhaps

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2 All quotations from and remarks about Faraday are from L. Pearce Williams, *Michael Faraday, A biography,* London and New York, 1965.

significant that Faraday was no materialist. He is part of that tradition in physics that downplays matter and emphasizes fields of force and energy. One may even wonder if Smart's materialism is an empirical thesis. Suppose that the model of the physical world, due to Leibniz, to Boscovic, to the young Kant, to Faraday, to nineteenth-century energeticists, is in fact far more successful than atomism. Suppose that the story of building blocks gives out after a while. Would Smart then conclude that the fundamental entities of physics are theoretical fictions?

La Realite Physique, the most recent book by the philosophical quantum theorist, Bernard d'Espagnat, is an argument that we can continue to be scientific realists without being materialists. Hence ` real' must be able to mark other contrasts than the one chosen by Smart. Note also that Smart's distinction does not help us say whether the theoretical entities of social or psychological science are real. Of course one can to some extent proceed in a materialistic way. Thus we find the linguist Noam Chomsky, in his book *Rules and Representations* (198o), urging realism in cognitive psychology. One part of his claim is that structured material found in the brain, and passed down from generation to generation, helps explain language acquisition. But Chomsky is not asserting only that the brain is made up of organized matter. He thinks the structures are responsible for some of the phenomenon of thought. Flesh and blood structures in our heads cause us to think in certain ways. This word `cause' prompts another version of scientific realism.

Causalism

Smart is a materialist. By analogy say that someone who emphasizes the causal powers of real stuff is *a causalist.* David Hume may have wanted to analyse causality in terms of regular association between cause and effect. But good Humeians know there must be more than mere correlation. Every day we read this sort of thing:

While the American College of Obstetricians and Gynecologists recognizes that an association has been established between toxic-shock-syndrome and menstruation-tampon use, we should not assume that this means there is a definite cause-and-effect relationship until we better understand the mechanism that creates this condition. (Press release, October 7, 1980.)

A few young women employing a new brand (` Everything you've ever wanted in a tampon . . . or napkin') vomit, have diarrhoea and

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a high fever, some skin rash, and die. It is not just fear of libel suits that makes the College want a better understanding of mechanisms before it speaks of causes. Sometimes an interested party does deny that an association shows anything. For example, on September 19, 1980, a missile containing a nuclear warhead blew up after someone had dropped a pipe wrench down the silo. The warhead did not go off, but soon after the chemical explosion the nearby village of Guy, Arkansas, was covered in reddish-brown fog. Within an hour of the explosion the citizens of Guy had burning lips, shortness of breath, chest pains, and nausea. The symptoms continued for weeks and no one anywhere else in the world had the same problem. Cause and Effect? `The United States Air Force has contended that no such correlation has been determined.' (Press release, October 198o).

The College of Obstetricians and Gynecologists insists that we cannot talk of causes until we find out how the causes of toxic-shock syndrome actually work. The Air Force, in contrast, is lying through its teeth. It is important to the causalist that such distinctions arise in a natural way. We distinguish ludicrous denials of any correlation, from assertions of correlations. We also distinguish correlations from causes. The philosopher C.D. Broad once made this anti-Humeian point in the following way. We may observe that every day a factory hooter in Manchester blows at noon, and exactly at noon the workers in a factory in Leeds lay down their tools for an hour. There is a perfect regularity, but the hooter in Manchester is not the cause of the lunch break in Leeds.

Nancy Cartwright advocates causalism. In her opinion one makes a very strong claim in calling something a cause. We must understand why a certain type of event regularly produces an effect. Perhaps the clearest proof of such understanding is that we can actually use events of one kind to produce events of another kind. Positrons and electrons are thus to be called real, in her vocabulary, since we can for example spray them, separately, on the niobium droplet and thereby change its charge. It is well understood why this effect follows the spraying. One made the experimental device because one knew it would produce these effects. A vast number of very different causal chains are understood and employed. We are entitled to speak of the reality of electrons not because they are building blocks but because we know that they have quite specific causal powers.

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This version of realism makes sense of Faraday. As his biographer put it:

The magnetic lines of force are visible if and when iron filings are spread around a magnet, and the lines are supposedly denser where the filings are thicker. But no one had assumed that the lines of force are there, in reality, even when the iron filings are removed. Faraday now did: we can cut these lines and get a real effect (for example with the electric motor that Faraday invented) — hence they are real.

The true story of Faraday is a little more complicated. Only long after he had invented the motor did he set out his line of force realism in print. He began by saying ` I am now about to leave the strict line of reasoning for a time, and enter upon a few speculations respecting the physical character of lines of force'. But what-ever the precise structure of Faraday's thought, we have a manifest distinction between a tool for calculation and a conception of cause and effect. No materialist who follows Smart will regard lines of force as real. Faraday, tinged with immaterialism, and something of a causalist, made just that step. It was a fundamental move in the history of science. Next came Maxwell's electro-dynamics that still envelops us.

Entities not theories

I distinguished *realism about entities* and *realism about theories*. Both causalists and materialists care more for entities than theories. Neither has to imagine that there is a best true theory about electrons. Cartwright goes further; she denies that the laws of physics state the facts. She denies that the models that play such a central role in applied physics are literal representations of how things are. She is an anti-realist about theories and a realist about entities. Smart could, if he chose, take a similar stance. We may have no true theory about how electrons go into the build-up of atoms, then of molecules, then of cells. We will have models and theory sketches. Cartwright emphasizes that in several branches of quantum mechanics the investigator regularly uses a whole battery of models of the same phenomena. No one thinks that one of these is the whole truth, and they may be mutually inconsistent. They are intellectual tools that help us understand phenomena and build bits and pieces of experimental technology. They enable us to intervene in processes and to create new and hitherto unimagined phen-

omena. But what is actually `making things happen' is not the set of laws, or true laws. There are no exactly true laws to make anything happen. It is the electron and its ilk that is producing the effects. The electrons are real, they produce the effects.

This is a striking reversal of the empiricist tradition going back to Hume. In that doctrine it is only the regularities that are real. Cartwright is saying that in nature there are no deep and completely uniform regularities. The regularities are features of the ways in which we construct theories in order to think about things. Such a radical doctrine can only be assessed in the light of her detailed treatment in *How the Laws of Physics Lie*. One aspect of her approach is described in Chapter 12 below.

The possibility of such a reversal owes a good deal to Hilary Putnam. As we shall find in Chapters 6 and 7, he had readily modified his views. What is important here is that he rejects the plausible notion that theoretical terms, such as `electron', get their sense from within a particular theory. He suggests instead that we can name kinds of things that the phenomena suggest to an inquiring and inventive mind. Sometimes we shall be naming nothing, but often one succeeds in formulating the idea of a kind of thing that is retained in successive elaborations of theory. More importantly one begins to be able to do things with the theoretical entity. Early in the day one may start to measure it; much later, one may spray with it. We shall have all sorts of incompatible accounts of it, all of which agree in describing various causal powers which we are actually able to employ while intervening in nature. (Putnam's ideas are often run together with ideas about essence and necessity more attributable to Saul Kripke: I attend only to the practical and pragmatic part of Putnam's account of naming.)

Beyond physics

Unlike the materialist, the causalist can consider whether the superego or late capitalism is real. Each case has to stand on its own: one might conclude that Jung's collective unconscious is not real while Durkheim's collective consciousness is real. Do we sufficiently understand what these objects or processes do? Can we intervene and redeploy them? Measurement is not enough. We can measure IQ and boast that a dozen different techniques give the same stable array of numbers, but we have not the slightest causal understanding. In a recent polemic Stephen Jay Gould speaks of the `fallacy of reification' in the history of IQ: I agree.

Causalism is not unknown in the social sciences. Take Max Weber (1864—1920), one of the founding fathers. He has a famous doctrine of ideal types. He was using the word `ideal' fully aware of its philosophical history. In his usage it contrasts with `real'. The ideal is a conception of the human mind, an instrument of thought (and none the worse for that). Just like Cartwright in our own day, he was `quite opposed to the naturalistic prejudice that the goal of the social sciences must be the reduction of reality to "laws"'. In a cautious observation about Marx, Weber writes,

> All specifically Marxian `laws' and developmental constructs, in so far as they are theoretically sound, are ideal types. The eminent, indeed *heuristic* significance *of* these ideal-types when they are used for the *assessment of* reality is known to everyone who has ever employed Marxian concepts and hypotheses. Similarly their perniciousness, as soon as they are thought *of* as empirically valid or real (i.e. truly metaphysical) `effective forces', 'tendencies', etc., is likewise known to those who have used them.[3]

One can hardly invite more controversy than by citing Marx and Weber in one breath. The point of the illustration is, however, a modest one. We may enumerate the lessons:

1 The materialist, such as Smart, can attach no direct sense to the reality of social science entities.

2 The causalist can.

3 The causalist may in fact reject the reality of any entities yet proposed in theoretical social science; materialist and causalist may be equally sceptical — although no more so than the founding fathers.

4 Weber's doctrine of ideal types displays a causalist attitude to social science laws. He uses it in a negative way. He holds that for example Marx's ideal types are not real precisely because they do not have causal powers.

5 The causalist may distinguish some social science from some physical science on the ground that the latter has found some entities whose causal properties are well understood, while the former has not.

((footnote:))

'Objectivity in social science and social policy', German original 1904, in Max Weber, *The Methodology of the Social Sciences (E.A.* Shils and H.A. Finch, eds. and trans.), New York, 1949, P. 103.

My chief lesson here is that at least some scientific realism can use the word `real' very much the same way that Austin claims is standard. The word is not notably ambiguous. It is not particularly deep. It is a substantive-hungry trouser-word. It marks a contrast. What contrast it marks depends upon the noun or noun phrase *N* that it modifies or is taken to modify. Then it depends upon the way that various candidates for being *N* may fail to be *N*. If the philosopher is suggesting a new doctrine, or a new context, then one will have to specify why lines of force, or the id, fail to be real entities. Smart says entities are for building. Cartwright says they are for causing. Both authors will deny, although for different reasons, that various candidates for being real entities are, in fact, real. Both are scientific realists about some entities, but since they are using the word ` real' to effect different contrasts, the contents of their `realisms' are different. We shall now see that the same thing can happen for anti-realists.

3 Positivism

one anti-realist tradition has been around for a long time. At first might it does not seem to worry about what the word ` real' means. It says simply: there *are* no electrons, nor any other theoretical entities. In a less dogmatic mood it says we have no good reason to suppose that any such things exist; nor have we any expectation of showing that they do exist. Nothing can be known to be real except what might be observed.

The tradition may include David Hume's *A Treatise of Human Nature* (1739). Its most recent distinguished example is Bas van Fraassen's *The Scientific Image* (198o). We find precursors of Hume even in ancient times, and we shall find the tradition continuing long into the future. I shall call it *positivism.* There is nothing in the name, except that it rings a few bells. The name had not even been invented in Hume's day. Hume is usually classed as an empiricist. Van Fraassen calls himself a constructive empiricist. Certainly each generation of philosophers with a positivist frame of mind gives a new form to the underlying ideas and often chooses a new label. I want only a handy way to refer to those ideas, and none serves me better than `positivism'.

Six positivist instincts

The key ideas are as follows: (i) An emphasis upon *verification* (or some variant such as *falsification):* Significant propositions are hose whose truth or falsehood can be settled in some way. *(2) Pro-observation:* What we can see, feel, touch, and the like, provides the best content or foundation for all the rest of our non-mathematical knowledge. *(3) Anti-cause:* There is no causality in nature, over and above the constancy with which events of one kind are followed by events of another kind. (4) *Downplaying explanations:* Explanations may help organize phenomena, but do not provide any deeper answer to *Why* questions except to say that the phenomena regularly occur in such and such a way.

(5) Anti-theoretical entities:

Positivists tend to be non-realists, not only because they restrict reality to the observable but also because they are against causes and are dubious about explanations. They won't infer the existence of electrons from their causal effects because they reject causes, holding that there are only constant regularities between phenomena. (6) Positivists sum up items (i) to (5) by being *against metaphysics.* Untestable propositions, unobservable entities, causes, deep explanation – these, says the positivist, are the stuff of metaphysics and must be put behind us.

I shall illustrate versions of these six themes by four epochs: Hume (1739), Comte (1830-42), logical positivism (1920–40) and van Fraassen (1980).

Self-avowed positivists

The name `positivism' was invented by the French philosopher Auguste Comte. His *Course of Positive Philosophy* was published in thick installments between 1830 and 1842. Later he said that he had chosen the word `positive' to capture a lot of values that needed emphasis at the time. He had, he tells us, chosen the word ` positive' because of its happy connotations. In the major West European languages `positive' had overtones of reality, utility, certainty, precision, and other qualities that Comte held in esteem.

Nowadays when philosophers talk of `the positivists' they usually mean not Comte's school but rather the group of logical positivists who formed a famous philosophy discussion group in Vienna in the 1920s. Moritz Schlick, Rudolf Carnap, and Otto Neurath were among the most famous members. Karl Popper, Kurt Godel, and Ludwig Wittgenstein also came to some of the meetings. The Vienna Circle had close ties to a group in Berlin of whom Hans Reichenbach was a central figure. During the Nazi regime these workers went to America or England and formed a whole new philosophical tradition there. In addition to the figures that I have already mentioned, we have Herbert Feigl and C.G. Hempel. Also the young Englishman A. J. Ayer went to Vienna in the early 19305 and returned to write his marvellous tract of English logical positivism, *Language, Truth and Logic* (1936). At the same time Willard V.O. Quine made a visit to Vienna which sowed the seeds of his doubt about some logical positivist theses, seeds which blossomed into Quine's famous denials of the analytic–synthetic

distinction and the doctrine of the indeterminancy of translation.

Such widespread influence makes it natural to call the logical positivists simply positivists. Who remembers poor old Comte, longwinded, stuffy, and not a success in life? But when I am speaking strictly, I shall use the full label `logical positivism', keeping `positivism' for its older sense. Among the distinctive traits of logical positivism, in addition to items (i) to (6), is an emphasis on logic, meaning, and the analysis of language. These interests are foreign to the original positivists. Indeed for the philosophy of science I prefer the old positivism just because it is not obsessed by a theory of meaning.

The usual Oedipal reaction has set in. Despite the impact of logical positivism on English-speaking philosophy, no one today wants to be called a positivist. Even logical positivists came to avour the label of `logical empiricist.' In Germany and France ' positivism' is, in many circles, a term of opprobrium, denoting an obsession with natural science and a dismissal of alternative routes to understanding in the social sciences. It is often wrongly associated with a conservative or reactionary ideology.

In *The Positivist Dispute in German Sociology,* edited by Theodore Adorno, we see German sociology professors and their philosophical peers — Adorno, Jurgen Habermas and so forth — lining up against Karl Popper, whom they call a positivist. He himself rejects that label because he has always dissociated himself from logical positivism. Popper does not share enough of my f|eatures (i) to (6) for me to call him a positivist. He is a realist about theoretical entities, and he holds that science tries to discover explanations and causes. He lacks the positivist obsession with observation and the raw data of sense. Unlike the logical positivists he thought that the theory of meaning is a disaster for the philosophy of science. True, he does define science as the class of testable propositions, but far from decrying metaphysics, he thinks that untestable metaphysical speculation is a first stage in the formation of more testable bold conjectures.

Why then did the anti-positivist sociology professors call Popper a positivist? *Because he believes in the unity of scientific method.* Make hypotheses, deduce consequences, test them: that is Popper's method of conjecture and refutation. He denies that there is any peculiar technique for the social sciences, any *Verstehen* that is

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different from what is best for natural science. In this he is at one with the logical positivists. But I shall keep `positivism' for the name of an anti-metaphysical collection of ideas (i) to (6), rather than dogma about the unity of scientific methodology. At the same time I grant that anyone who dreads an enthusiasm for scientific rigour will see little difference between Popper and the members of the Vienna Circle.

Anti-metaphysics

Positivists have been good at slogans. Hume set the tone with the ringing phrases with which he concludes his *An Enquiry Concerning Human Understanding:*

When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divine or school metaphysics, for instance; let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames: for it can contain nothing but sophistry and illusion.

In the introduction to his anthology, *Logical Positivism,* A.J. Ayer says that this `is an excellent statement of the positivists' position. In the case of the logical positivists the epithet "logical" was added because they wished to annex the discoveries of modern logic.' Hume, then, is the beginning of the criterion of verifiability intended to distinguish nonsense (metaphysics) from sensible discourse (chiefly science). Ayer began his *Language, Truth and Logic* with a powerful chapter, called `The elimination of metaphysics'. The logical positivists, with their passion for language and meanings, combined their scorn for idle metaphysics with a meaning-oriented doctrine called `the verification principle'. Schlick announced that the meaning of a statement is its method of verification. Roughly speaking, a statement was to be meaningful, or to have `cognitive meaning', if and only if it was verifiable. Surprisingly, no one was ever able to define verifiability so as to exclude all bad metaphysical conversation and include all good scientific talk.

Anti-metaphysical prejudices and a verification theory of mean-ings are linked largely by historical accident. Certainly Comte was a great anti-metaphysician with no interest in the study of 'meanings'. Equally in our day van Fraassen is as opposed to metaphysics.

He is of my opinion that, whatever be the interest in the philosophy of language, it has very little value for understanding science. At the start of *The Scientific Image,* he writes: `My own view is that empiricism is correct, but could not live in the linguistic form the logical] positivists gave it.' (p. 3)

Comte

Auguste Comte was very much a child of the first half of the nineteenth century. Far from casting empiricism into a linguistic form, he was an historicist: that is, he firmly believed in human progress and in the near-inevitability of historical laws. It is sometimes thought that positivism and historicism are at odds with each other: quite the contrary, they are, for Comte, complementary parts of the same ideas. Certainly historicism and positivism are no more necessarily separated than positivism and the theory of meaning are necessarily connected.

Comte's model was a passionate *Essay on the Development of the Human Mind,* left as a legacy to progressive mankind by the radical aristocrat, Condorcet (1743–94). This document was written just before Condorcet killed himself in the cell from which, the following morning, he was to be taken to the guillotine. Not even the Terror of the French Revolution, 1794, could vanquish faith in progress. Comte inherited from Condorcet a structure of the evolution of the human spirit. It is defined by The Law of Three Stages. First we went through a theological stage, characterized by the search for first causes and the fiction of divinities. Then we went through a somewhat equivocal metaphysical stage, in which we gradually replaced divinities by the theoretical entities of half-completed science. Finally we now progress to the stage of positive science.

Positive science allows propositions to count as true-or-false if and only if there is some way of settling their truth values. Comte's *Course of Positive Philosophy* is a grand epistemological history of the development of the sciences. As more and more styles of scientific reasoning come into being, they thereby constitute more and more domains of positive knowledge. Propositions cannot have ' positivity' – be candidates for truth-or-falsehood – unless there is some style of reasoning which bears on their truth value and can at least in principle determine that truth value. Comte, who invented

the very word ` sociology', tried to devise a new methodology, a new style of reasoning, for the study of society and `moral science'. He was wrong in his own vision of sociology, but correct in his meta-conception of what he was doing: creating a new style of reasoning to bring positivity – truth-or-falsehood – to a new domain of discourse.

Theology and metaphysics, said Comte, were earlier stages in human development, and must be put behind us, like childish things. This is not to say that we must inhabit a world denuded of values. In the latter part of his life Comte founded a Positivist Church that would establish humanistic virtues. This Church is not quite extinct; some buildings still stand, a little tatty, in Paris, and I am told that Brazil still possesses strongholds of the institution. Long ago it did flourish in collaboration with other humanistic societies, in many parts of the would. Thus positivism was not only a philosophy of scientism but a new, humanistic, religion.

Anti-cause

Hume notoriously taught that cause is only constant conjunction. To say that *A* caused *B* is not to say that *A,* from some power or character within itself, brought about *B.* It is only to say that things of type *A* are regularly followed by things of type *B.* The details of Hume's argument are analysed in hundreds of philosophy books. We may, however, miss a good deal if we read Hume out of his historical context.

Hume is in fact not responsible for the widespread philosophical acceptance of a constant-conjunction attitude to causation. Isaac Newton did it, unintentionally. The greatest triumph of the human spirit in Hume's day was held to be the Newtonian theory of gravitation. Newton was so canny about the metaphysics of gravity that scholars will debate to the end of time what he really thought. Immediately before Newton, all progressive scientists thought that the world must be understood in terms of mechanical pushes and pulls. But gravity did not seem `mechanical', for its was action at a distance. For that very reason, Newton's only peer, Leibniz, quite rejected Newtonian gravitation: it was a reactionary reversion to inexplicable occult powers. A positivist spirit triumphed over Leibniz. We learned to think that the laws of gravity are regularities that describe what happens in the world. Then we decided that all causal laws are mere regularities!

For empirically minded people the post-Newtonian attitude was, then, this: we should not seek for causes in nature, but only regularities. We should not think of laws of nature revealing what must happen in the universe, but only what does happen. The natural scientist tries to find universal statements – theories and laws – which cover all phenomena as special cases. To say that we have found the explanation of an event is only to say that the event can be deduced from a general regularity.

There are many classic statements of this idea. Here is one from 'Thomas Reid's *Essays on the Active Powers of the Human Mind* of 1788. Reid was,the founder of what is often called the Scottish School of Common Sense Philosophy, which was imported to form he main American philosophy until the advent of pragmatism at the end of the nineteenth century.

Natural philosophers, who think accurately, have a precise meaning to the terms they use in the science; and, when they pretend to show the cause of any phenomenon of nature, they mean by the cause, a law of nature of which that phenomenon is a necessary consequence.

The whole object of natural philosophy, as Newton expressly teaches, is seducible to these two heads: first, by just induction from experiment and observation, to discover the laws of nature; and then to apply those laws to the solution of the phenomena of nature. This was all that this great philosopher attempted, and all that he thought attainable. (I. vii. 6.)

Comte tells a similar story in his *Cours de philosophie positive:*

The first characteristic of the positive philosophy is that it regards all phenomena as subjected to invariable natural *laws*. Our business is –seeing how vain is any research into what are called *causes,* whether first or final –

to pursue an accurate discovery of these laws, with a view to reducing them to the smallest possible number. By speculating upon causes, we could solve no difficulty about origin and purpose. Our real business is to analyze accurately the circumstances of phenomena, and to connect them by the natural relations of succession and resemblance. The best illustration of this is in the case of the doctrine of gravitation. We say that the general phenomena of the universe are *explained* by it, because it connects under one head the whole immense variety of astronomical facts; exhibiting the

Instant tendency of atoms towards each other in direct proportion to their masses, and in inverse proportion to the squares of their distances; while the lie general fact itself is a mere extension of one that is perfectly familiar to

and that we therefore say that we know – the weight of bodies on the surface of the earth. As to what weight and attraction are, these are questions that we regard as insoluble, which are not part of positive philosophy and which we rightly abandon to the imagination of the theologians or the subtlety of the metaphysicians. (Paris, 1830, pp. 14–16.)

Logical positivism was also to accept Hume's constant conjunction account of causes. Laws of Nature, in Mortitz Schlick's maxim, *describe* what happens, but do not *prescribe* it. They are accounts of regularities only. The logical positivist account of explanation was finally summed up in C.G. Hempel's `deductive-nomological' model of explanation. To explain an event whose occurrence is described by the sentence S is to present some laws of nature (i.e. regularities) *L,* and some particular facts *F* and to show that the sentence S is deducible from sentences stating *L* and *F.* Van Fraassen, who has an interestingly more sophisticated account of explanation, shares the traditional positivist hostility to causes. ` Flights of fancy' he dismissively calls them in his book (for causes are even worse, in his book, than explanation).

Anti-theoretical-entities

Opposition to unobservable entities goes hand in hand with an opposition to causes. Hume's scorn for the entity-postulating sciences of his day is, as always, stated in an ironic prose. He admires the seventeenth-century chemist Robert Boyle for his experiments and his reasoning, but not for his corpuscular and mechanical philosophy that imagines the world to be made up of little bouncy balls or springlike tops. In Chapter LXII of his great *History of England* he tells us that, `Boyle was a great partisan of the mechanical philosophy, a theory which, by discovering some of the secrets of nature and allowing us to imagine the rest, is so agreeable to the natural vanity and curiosity of men.' Isaac Newton, `the greatest and rarest genius that ever arose for the ornament and instruction of the species', is a better master than Boyle: `While Newton seemed to draw off the veil from some of the mysteries of nature, he showed at the same time the imperfections of the mechanical philosophy, and thereby restored her ultimate secrets to that obscurity in which they ever did and ever will remain.'

Hume seldom denies that the world is run by hidden and secret causes. He denies that they are any of our business. The natural vanity and curiosity of our species may let us seek fundamental particles, but physics will not succeed. Fundamental causes ever did and ever will remain cloaked in obscurity.

Opposition to theoretical entities runs through all positivism. Comte admitted that we cannot merely generalize from observations, but must proceed through hypotheses. These must, how-

ever, be regarded only as hypotheses, and the more that they postulate, the further they are from positive science. In practical terms, Comte was opposed to the Newtonian aether, soon to be electromagnetic aether, filling all space. He was equally opposed to t lie atomic hypothesis. You win one, you lose one.

The logical positivists distrusted theoretical entities in varying degrees. The general strategy was to employ logic and language. I 'hey took a leaf from Bertrand Russell's notebook. Russell thought hat whenever possible, inferred entities should be replaced by logical constructions. That is, a statement involving an entity whose existence is merely inferred from data is to be replaced by a logically equivalent statement about the data. In general these data are closely connected with observation. Thereby arose a great pro-gramme of reductionism for the logical positivists, who hoped that all statements involving theoretical entities would by means of logic be `reduced' to statements that did not make reference to such entities. The failure of this project was greater even than the failure to state the verification principle.

Van Fraassen continues the positivist antipathy to theoretical entities. Indeed he will not even let us speak of theoretical entities: we mean, he writes, simply unobservable entities. These, not being seen, must be inferred. It is van Fraassen's strategy to block every inference to the truth of our theories or the existence of their entities.

believing

hue did not believe in the invisible bouncy balls or atoms of Robert Boyle's mechanical philosophy. Newton had showed us that we ought only to seek natural laws that connect the phenomena. We should not allow our natural vanity to imagine that we can successfully seek out causes.

Comte equally disbelieved in the atoms and aether of the science of his time. We need to make hypotheses in order to tell us where to investigate nature, but positive knowledge must lie at the level of the he phenomena whose laws we may determine with precision. This is not to say that Comte was ignorant of science. He was trained by the great French theoretical physicists and applied mathematicians. I le believed in their laws of phenomena and distrusted any drive towards postulating new entities.

Logical positivism had no such simplistic opportunities.

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Members of the Vienna Circle believed the physics of their day: some had made contributions to it. Atomism and electromagnetism had long been established, relativity was a proven success and the quantum theories were advancing by leaps and bounds. Hence arose, in the extreme version of logical positivism, a doctrine of reductionism. It was proposed that in principle there are logical and linguistic transformations in the sentences of theories that will re-duce them to sentences about phenomena. Perhaps when we speak of atoms and currents and electric charges we are not to be under-stood quite literally, for the sentences we use are reducible to sentences about phenomena. Logicians did to some extent oblige. F.P. Ramsey showed how to leave out the names of theoretical entities in the theories, using instead a system of quantifiers. William Craig proved that for any axiomatizable theory involving both observational and theoretical terms, there exists an axiomatizable theory involving only the observational terms. But these results did not do quite what logical positivism wanted, nor was there any linguistic reduction for any genuine science. This was in terrible contrast to the remarkable partial successes by which more superficial scientific theories have been reduced to deeper ones, for example, the ways in which analytic chemistry is founded upon quantum chemistry, or the theory of the gene has been transformed into molecular biology. Attempts at scientific reduction – reducing one empirical theory to a deeper one – have scored innumerable partial successes, but attempts at linguistic reduction have got nowhere.

Accepting

Hume and Comte took all that stuff about fundamental particles and said: We don't believe it. Logical positivism believed it, but said in a sense that it must not be taken literally; our theories are really talking about phenomena. Neither option is open to a present-day positivist, for the programmes of linguistic reduction failed, while on the other hand one can hardly reject the whole body of modern theoretical science. Yet van Fraassen finds a way through this impasse by distinguishing belief from acceptance.

Against the logical positivists, van Fraassen says that theories are to be taken literally. There is no other way to take them! Against the realist he says that we need not believe theories to be true. He invites us instead to use two further concepts: *acceptance* and *empirical*

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adequacy. He defines scientific realism as the philosophy that maintains that, `Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true' (p. 8). His own *constructive empiricism* asserts instead that, `Science aims to give us theories w which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate' (p. 12).

"There is,' he writes, `no need to believe good theories to be true, nor to believe *ipso facto* that the entities they postulate are real.' The ' *ipso facto'* reminds us that van Fraassen does not much distinguish realism about theories from realism about entities. I say that one could believe entities to be real, not `in virtue of the fact' that one believes some theory to be true, but for other reasons.

A little later van Fraassen explains as follows: `to accept a theory is (for us) to believe that it is empirically adequate – that what the theory says *about what is observable* (by us) is true' (p. 18). Theories are intellectual instruments for prediction, control, research and sheer enjoyment. Acceptance means commitment, among other things. To accept a theory in your field of research is to be

committed to developing the programme of inquiry that it suggests. You may even accept that it provides explanations. But you must reject what has been called inference to the best explanation: to accept a theory because it makes something plain is not thereby to kink that what the theory says is literally true.

Van Fraassen's is the most coherent present-day positivism. It has all six features by which I define positivism, and which are shared by Hume, Comte and the logical positivists. Naturally it lacks Hume's psychology, Comte's historicism, and logical positivism's theories of meaning, for those have nothing essential to to with the positivist spirit. Van Fraassen shares with his predecessors the *anti-metaphysics:* `The assertion of empirical adequacy is a great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics' (p. 69). He is *pro-observation,* and *anti-cause.* He *downplays explanation;* he does not think explanation leads to truth. Indeed, just like Hume and :unte, he cites the classic case of Newton's inability to explain gravity as proof that science is not essentially a matter of explalint ion (p. 94). Certainly he is *anti-theoretical-entities.* So he holds live of our six positivist doctrines. The only one left is the emphasis

on *verification* or some variant. Van Fraassen does not subscribe to the logical positivist verifiability theory of meaning. Nor did Comte. Nor, I think, did Hume, although Hume did have an unverifiability maxim for burning books. The positivist enthusiasm for verifiability was only temporarily connected with meaning, in the days of logical positivism. More generally it represents a desire for positive science, for knowledge that can be settled as true, and whose facts are determined with precision. Van Fraassen's constructive empiricism shares this enthusiasm.

Anti-explanation

Many positivist theses were more attractive in Comte's day than our own. In 1840, theoretical entities were thoroughly hypothetical, and distaste for the merely postulated is the starting point for some sound philosophy. But increasingly we have come even to see what was once merely postulated: microbes, genes, even molecules. We have also learned how to use many theoretical entities in order to manipulate other parts of the world. These grounds for realism about entities are discussed in Chapters 10 and 16 below. However one positivist theme stands up rather well: caution about explanation.

The idea of `inference to the best explanation' is quite old. C.S. Peirce (1839–1914) called it the method of hypothesis, or abduction. The idea is that if, confronted by some phenomenon, you find one explanation (perhaps with some initial plausibility) that makes sense of what is otherwise inexplicable, then you should conclude that the explanation is probably right. At the start of his career Peirce thought that there are three fundamental modes of scientific inference: deduction, induction and hypothesis. The older he got the more sceptical he became of the third category, and by the end of his life he attached no weight at all to ` inference to the best explanation'.

Was Peirce right to recant so thoroughly? I think so, but we need not decide that now. We are concerned only with inference to the best explanation as an argument for realism. The basic idea was enunciated by H. Helmholtz (1821–94), the great nineteenth-century contributor to physiology, optics, electrodynamics and other sciences. Helmholtz was also a philosopher who called realism
positivism

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`an admirably useful and precise hypothesis '.[1] By now there appear to be three distinct arguments in circulation. I shall call them the simple inference argument, the cosmic accident argument, and the success of science argument.

I am sceptical of all three. I should begin by saying that explanation may play a less central a role in scientific reasoning than some philosophers imagine. Nor is *the* explanation of a phenomenon one of the ingredients of the universe, as if the Author of Nature had written down various things in the Book of the World – the entities, the phenomena, the quantities, the qualities, the laws, the numerical constants, and also the explanations of events. Explanations are relative to human interests. I do not deny that explaining – ` feeling the key turn in the lock' as Peirce put it – does happen in our intellectual life. But that is largely a feature of the historical or psychological circumstances of a moment. There are times when we feel a great gain in understanding by the organization of new explanatory hypotheses. But that feeling is not a ground for supposing that the hypothesis is true. Van Fraassen and Cartwright urge that being an explanation is never a ground for belief. I am less stringent than they: it seems to me like Peirce to be merely a feeble ground. In 1905 Einstein explained the photo-electric effect with a theory of photons. He thereby made attractive the notion of quantized bundles of light. But the ground for believing the theory is its predictive success, and so forth, not its explanatory power. Feeling the key turn in the lock makes you feel t hat you have an exciting new idea to work with. It is not a ground for the truth of the idea: that comes later.

Simple inference

The simple inference argument says it would be an absolute miracle if for example the photoelectric effect went on working while there were no photons. The explanation of the persistence of this phenomenon – the one by which television information is converted from pictures into electrical impulses to be turned into electromagnetic waves in turn to be picked up on the home receiver – is

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1 `On the aim and progress of physical science' (German original 1871) in H. von Helmholtz, *Popular Lectures and Addresses on Scientific Subjects* (D. Atkinson trans.), London, 1873, p. 247.

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that photons do exist. As J.J.C. Smart expresses the idea: `One would have to suppose that there were innumerable lucky accidents about the behavior mentioned in the observational vocabulary, so that they behaved miraculously as *if* they were brought about by the non-existent things ostensibly talked about in the theoretical vocabulary.'[2] The realist then infers that photons are real because otherwise we could not understand how scenes are turned into electronic messages.

Even if, contrary to what I have said, explanation were a ground for belief, this seems not to be an inference to the best explanation at all. That is because the *reality* of photons is no part of the explanation. There is not, after Einstein, some further explanation, namely `and photons are real', or `there exist photons'. I am inclined to echo Kant, and say that existence is a merely logical predicate that adds nothing to the subject. To add `and photons are real', after Einstein has finished, is to add nothing to the under-standing. It is not in any way to increase or enhance the explanation.

If the explainer protests, saying that Einstein himself asserted the existence of photons, then he is begging the question. For the debate between realist and anti-realist is whether the adequacy of Einstein's theory of the photon does require that photons be real.

Cosmic accidents

The simple inference argument considers just one theory, one phenomenon and one kind of entity. The cosmic accident argument notes that often in the growth of knowledge a good theory will explain diverse phenomena which had not hitherto been thought of as connected. Conversely, we often come at the same brute entities by quite different modes of reasoning. Hans Reichenbach called this the common cause argument, and it has been revived by Wesley Salmon.[3] His favoured example is not the photoelectric effect but another of Einstein's triumphs. In 1905 Einstein also explained the Brownian movement – the way in which, as we now say, pollen particles are bounced around in a random way by being hit by molecules in motion. When Einstein's calculations are combined

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2 J.J.C. Smart, 'Difficulties for realism in the philosophy of science', in *Logic, Methodology and Philosophy of Science VI,* Proceedings of the 6th International Congress of Logic, Methodology and Philosophy of Science, Hannover, 1979, pp. 363-75.
3 Wesley Salmon, 'Why ask, "Why?" An Inquiry Concerning Scientific Explanation', *Proceedings and Addresses of the American Philosophical Association* 51 (1978), pp. 683-705.

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with the results of careful experimenters, we are able, for example, lit compute Avogadro's number, the number of molecules of an arbitrary gas contained in a given volume at a set temperature and pressure. This number had been computed from numerous quite different sources ever since 1815. What is remarkable is that we always get essentially the same number, coming at it from different routes. The only explanation must be that there *are* molecules, indeed, some 6.023 x 10 (to**23** molecules per gram-mole of any gas.

Once again, this seems to me to beg that realist/anti-realist issue. The anti-realist agrees that the account, due to Einstein and others, of the mean free path of molecules is a triumph. It is empirically adequate – wonderfully so. The realist asks why is it empirically adequate – is that not because there just are molecules? The anti-realist retorts that explanation is no hall-mark of truth, and that all you evidence points only to empirical adequacy. In short the argument goes around in circles (as, I contend, do all arguments conducted at this level of discussion of theories).

The success story

The previous considerations bear more on the existence of entities; now we consider the truth of theories. We reflect not on one bit of science but on ` Science' which, Hilary Putnam tells us, is a Success. This is connected with the claim that Science is converging on the truth, as urged by many, including W. Newton-Smith in his book *Rationality* (1982). Why is Science Successful? It must be because we are converging on the truth. This issue has now been well aired, and I refer you to a number of recent discussions.' The claim that here we have an `argument' drives me to the following additional expostulations:

1 The phenomenon of growth is at most a monotonic increase in knowledge, not convergence. This trivial observation is important, for `convergence' implies somewhat that there is *one* thing being converged on, but `increase' has no such implication. There can be heapings up of knowledge without there being any unity of

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Among many arguments in favour of this idea of convergence, see R.N. Boyd, `Scientific realism and naturalistic epistemology', in P.D. Asquith and R. Giere (eds.), *PSA 1980,* Volume **2,** Philosophy of Science Assn., East Lansing, Mich., pp. 613-62, and W.H. Newton-
The Rationality of Science, London, 1981. For a very powerful statement of the opposite point of view, see L. Laudan, `A confutation of convergent realism', *Philosophy of .Sience* 48 (1981), PP· 19-49.

science to which they all add up. There can also be an increasing depth of understanding, and breadth of generalization, without anything properly called convergence. Twentieth-century physics is a witness to this.

2 There are numerous merely sociological explanations of the growth of knowledge, free of realist implications. Some of these deliberately turn the `growth of knowledge' into a pretence. On Kuhn's analysis in *Structure,* when normal science is ticking over nicely, it is solving the puzzles that it creates as solvable, and so growth is built in. After revolutionary transition, the histories are rewritten so that early successes are sometimes ignored as uninteresting, while the `interesting' is precisely what the post-cataclysmic science is good at. So the miraculously uniform growth is an artifact of instruction and textbooks.

3 What grows is not particularly the strictly increasing body of (nearly true) *theory.* Theory-minded philosophers fixate on ac-cumulation of theoretical knowledge – a highly dubious claim. Several things do accumulate. (a) Phenomena accumulate. For example, Willis Lamb is trying to do optics without photons. Lamb may kill off the photons but the photoelectric effect will still be there. (b) Manipulative and technological skills accumulate – the photoelectric effect will still be opening the doors of supermarkets. (c) More interestingly to the philosopher, styles of scientific reasoning tend to accumulate. We have gradually accumulated a horde of methods, including the geometrical, the postulational, the model-building, the statistical, the hypothetico-deductive, the genetic, the evolutionary, and perhaps even the historicist. Certainly there is growth of types (a), (b), and (c), but in none of them is there any implication about the reality of theoretical entities or the truth of theories.

4 Perhaps there is a good idea, which I attribute to Imre Lakatos, and which is foreshadowed by Peirce and the pragmatism soon to be described. It is a route open to the post-Kantian, post-Hegelian, who has abandoned a correspondence theory of truth. One takes the growth of knowledge to be a given fact, and tries to characterize truth in terms of it. This is not explanation by assuming a reality, but a definition of reality as `what we grow to'. That may be a mistake, but at least it has an initial cogency. I describe it in Chapter 8 below.

5 Moreover, there are genuine conjectural inferences to be drawn from the growth of knowledge. To cite Peirce again, our talents at forming roughly the right expectations about the humanized world may be accounted for by the theory of evolution. If we regularly formed the wrong expectations, we would all be dead. But we seem to have an uncanny ability to formulate structures that explain and predict both the inner constitution of nature, and the most distant realms of cosmology. What can it have benefited us, in terms of survival, that we have a brain so tooled for the lesser and t he larger universe? Perhaps we should guess that people are indeed rational animals that live in a rational universe. Peirce made a more instructive if implausible proposal. He asserted that strict materialism and necessitarianism are false. The whole world is what he called `effete mind', which is forming

habits. The habits of inference that we form about the world are formed according to the Name habits that the world used as it acquired its increased spectrum of regularities. That is a bizarre and fascinating metaphysical conjecture that might be turned into an explanation of `the success of science'.

How Peirce's imagination contrasts with the banal emptiness of t he Success Story or convergence argument for realism! Popper, I ı hink, is a wiser self-professed realist than most when he writes that it never makes sense to ask for the explanation of our success. We can only have the faith to hope that it will continue. If you must have tin explanation of the success of science, then say what Aristotle did, that we are rational animals that live in a rational universe.

4 Pragmatism

Pragmatism is the American philosophy founded by Charles Sanders Peirce (1839—1914), and made popular by William James (1842—1910). Peirce was a cantankerous genius who obtained some employment in the Harvard Observatory and the US Coast and Geodesic survey, both thanks to his father, then one of the few distinguished mathematicians in America. In an era when philosophers were turning into professors, James got him a job at Johns Hopkins University. He created a stir there by public misbehaviour
(such as throwing a brick at a ladyfriend in the street), so the President of the University abolished the whole Philosophy Department, then created a new department and hired everyone back — except Peirce. Peirce did not like James's popularization of pragmatism, so he invented a new name for his ideas — pragmaticism — a name ugly enough, he would say, that no one would steal it. The relationship of pragmaticism to reality is well stated in his widely reprinted essay, `Some consequences of four incapacities' (1868).

And what do we mean by the real? It is a conception which we must first have had when we discovered that there was an unreal, an illusion; that is, when we first corrected ourselves. . . . *The real, then, is that which, sooner or later, information and reasoning would finally result in,* and which is therefore independent of the vagaries of me and you. Thus, the very origin of the conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of a definite increase of knowledge. And so those two series of cognition — the real and the unreal — consist of those which, at a time sufficiently future, the community will always continue to reaffirm; and of those which, under the same conditions, will ever after be denied. Now, a proposition whose falsity can never be discovered, and the error of which therefore is absolutely incognizable, contains, upon our principle, absolutely no error. Consequently, that which is thought in these cognitions is the real, as it really is. There is nothing, then, to prevent our knowing outward things as they really are, and it is most likely that we do thus know them in numberless cases, although we can never be absolutely certain of doing so in any special case. ***(The Philosophy of** Peirce,* J. Buchler (ed.), pp. **247f.)**

Precisely this notion is revived in our day by Hilary Putnam, whose 'internal realism' is the topic of Chapter 7.

The road to Peirce

Peirce and Nietzsche are the two most memorable philosophers writing a century ago. Both are the heirs of Kant and Hegel. They represent alternative ways to respond to those philosophers. Both took for granted that Kant had shown that truth cannot consist in some correspondence to external reality. Both took for granted that process and possibly progress are essential characteristics of the nature of human knowledge. They had learned that from Hegel.

Nietzsche wonderfully recalls how the true world became a fable. An aphorism in his book, *The Twilight of the Idols,* starts from Plato's `true world – attainable for the sage, the virtuous man'. We arrive, with Kant, at something `elusive, pale, Nordic, Konigsbergian'. Then comes Zarathrustra's strange semblance of subjectivism. That is not the only post-Kantian route. Peirce tried to replace truth by method. Truth is whatever is in the end delivered to the community of inquirers who pursue a certain end in a certain way.

Thus Peirce is finding an objective substitute for the idea that truth is correspondence to a mind-independent reality. He sometimes called his philosophy objective idealism. He is much impressed with the need for people to attain a stable set of beliefs. In a famous essay on the fixation of belief, he considers with genuine seriousness the notion that we might fix our beliefs by following authority, or by believing whatever first comes into our heads and sticking to it. Modern readers often have trouble with this essay, because they do not for a moment take seriously that Peirce held an Established (and powerful) Church to be a very good way to fix beliefs. If there is nothing to which true belief has to correspond, why not have a Church fix your beliefs? It can be very comforting to know that your Party has the truth. Peirce rejects this possibility because he holds as a fact of human nature (not of pre-human truth) that there will in the end always be dissidents. So you want a way to lix beliefs that will fit in with this human trait. If you can have a method which is internally self-stabilizing, which acknowledges permanent fallibility and yet at the same time tends to settle down, t hen you will have found a better way to fix belief.

Repeated measurements as the model of reasoning

Peirce is perhaps the only philosopher of modern times who was quite a good experimenter. He made many measurements, including a determination of the gravitational constant. He wrote extensively on the theory of error. Thus he was familiar with the way in which a sequence of measurements can settle down to one basic value. Measurement, in his experience, converges, and what it converges on is by definition correct. He thought that all human beliefs would be like that too. Inquiry continued long enough would lead to a stable opinion about any issue we could address. Peirce did not think that truth is correspondence to the facts: the truths are the stable conclusions reached by that unending COMMUNITY of inquirers.

This proposal to substitute method for truth — which would still warrant scientific objectivity — has all of a sudden become popular again. I think that it is the core of the methodology of research programmes of Imre Lakatos, and explained in Chapter 8. Unlike Peirce, Lakatos attends to the motley of scientific practices and so does not have the simplistic picture of knowledge settling down by a repeated and slightly mindless process of trial and error. More recently Hilary Putnam has become Peircian. Putnam does not think that Peirce's account of the method of inquiry is the last word, nor does he propose that there is a last word. He does think that there is an evolving notion of rational investigating, and that the truth is what would result from the results to which such investigation tends. In Putnam there is a double limiting process. For Peirce, there was one method of inquiry, based on deduction, induction, and, to some small degree, inference to the best explanation. Truth was, roughly, whatever hypothesizing, inducing, and testing settled down upon. That is one limiting process. For Putnam the methods of inquiry can themselves grow, and new styles of reasoning can build on old ones. But he hopes that there will be some sort of accumulation here, rather than abrupt displacement of one style of reasoning just replacing another one. There can then be two limiting processes: the long term settling into a ` rationality' of accumulated modes of thinking, and the long term settling into facts that are agreed to by these evolving kinds of reason.

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V ision

Peirce wrote on the whole gamut of philosophical topics. He has gathered about him a number of coteries who hardly speak to each other. Some regard him as a predecessor of Karl Popper, for nowhere else do we find so trenchant a view of the self-correcting method of science. Logicians find that he had many premonitions of how modern logic would develop. Students of probability and induction rightly see that Peirce had as deep an understanding of probabilistic reasoning as was possible in his day. Pierce wrote a great deal of rather obscure but fascinating material on signs, and a whole discipline that calls itself semiotics reveres him as a founding father. I think him important because of his bizarre proposal that one just is one's language, a proposal that has become a centrepiece i modern philosophy. I think him important because he was the first person to articulate the idea that we live in a universe of chance, chance that is both indeterministic, but which because of the laws of probability accounts for our false conviction that nature is governed by regular laws. A glance at the index at the end of this book will refer you to other things that we can learn from Peirce. Peirce has uttuffered from readers of narrow vision, so he is praised for having had this precise thought in logic, or that inscrutable idea about signs. We should instead see him as a wild man, one of the handful who understood the philosophical events of his century and set out to cast his stamp upon them. He did not succeed. He finished almost nothing, but he began almost everything.

The branching of the ways

Peirce emphasized rational method and the community of inquirers who would gradually settle down to a form of belief. Truth is whatever in the end results. The two other great pragmatists, William James and John Dewey, had very different instincts. They lived, if not for the now, at least for the near future. They scarcely addressed the question of what might come out in the end, if there is one. Truth is whatever answers to our present needs, or at least those needs that lie to hand. The needs may be deep and various, as attested in James's fine lectures, *The Varieties of Religious Experience.* Dewey gave us the idea that truth is warranted acceptability. He thought of language as an instrument that we use to

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mould our experiences to suit our ends. Thus the world, and our representation of it, seems to become at the hands of Dewey very much of a social construct. Dewey despised all dualisms – mind/matter, theory/practice, thought/action, fact/value. He made fun of what he called the spectator theory of knowledge. He said it resulted from the existence of a leisure class, who thought and wrote philosophy, as opposed to a class of entrepreneurs and workers, who had not the time for just looking. *My* own view, that realism is more a matter of intervention in the world, than of representing it in words and thought, surely owes much to Dewey.

There is, however, in James and Dewey, an indifference to the Peircian vision of inquiry. They did not care what beliefs we settle on in the long run. The final human fixation of belief seemed to them a chimaera. That is partly why James's rewriting of pragmatism was resisted by Peirce. This same disagreement is enacted at the very moment. Hilary Putnam is today's Peircian. Richard Rorty, in his book ***Philosophy** and **the Mirror** of Nature* (1979), plays some of the parts acted by James and Dewey. He explicitly says that recent history of American philosophy has got its emphases wrong. Where Peirce has been praised, it has been only for small things. (*My* section above on Peirce's vision, obviously disagrees.) Dewey and James are the true teachers, and Dewey ranks with Heidegger and Wittgenstein as the three greats of the twentieth century. However Rorty does not write only to admire. He has no Peirce/Putnam interest in the long run nor in growing canons of rationality. Nothing is more reasonable than anything else, in the long run. James was right. Reason is whatever goes in the conversation of our days, and that is good enough. It may be sublime, because of what it inspires within us and among us. There is nothing that makes one conversation intrinsically more rational than another. Rationality is extrinsic: it is whatever we agree on. If there is less persistence among fashionable literary theories than among fashionable chemical theories, that is a matter of sociology. It is not a sign that chemistry has a better method, nor that it is nearer to the truth.

Thus pragmatism branches: there are Peirce and Putnam on the one hand, and James, Dewey and Rorty on the other. Both are anti-realist, but in somewhat different ways. Peirce and Putnam optimistically hope that there is something that sooner or later,

Information and reasoning would finally result in. That, for them, is t he real and the true. It is interesting for Peirce and Putnam both to define the real and to know what, within our scheme of things, will pan out as real. This is not of much interest to the other sort of pragmatism. How to live and talk is what matters, in those quarters. ' There is not only no external truth, but there are no external or even evolving canons of rationality. Rorty's version of pragmatism is yet another language-based philosophy, which regards all our life as a matter of conversation. Dewey rightly despised the spectator theory of knowledge. What might he have thought of science as conversation? In my opinion, the right track in Dewey is the attempt to destroy the conception of knowledge and reality as a matter of thought and of representation. He should have turned the minds of philosophers to experimental science, but instead his new followers praise talk.

Dewey distinguished his philosophy from that of earlier philosophical pragmatists by calling it *instrumentalism.* This partly Indicated the way in which, in his opinion, things we make (including all tools, including language as a tool) are instruments that intervene when we turn our experiences into thoughts and deeds that serve our purposes. But soon `instrumentalism' came to denote a philosophy of science. An instrumentalist, in the parlance of most modern philosophers, is a particular kind of anti-realist about science – one who holds that theories are tools or calculating devices for organizing descriptions of phenomena, and for drawing inferences from past to future. Theories and laws have no truth in themselves. They are only instruments, not to be understood as literal assertions. Terms that seemingly denote invisible entities do not function as referential terms at all. Thus instrumentalism is to he contrasted with van Fraassen's view, that theoretical expressions are to be taken literally – but not believed, merely `accepted' and used.

how do positivism and pragmatism differ?

The differences arise from the roots. Pragmatism is an Hegelian doctrine which puts all its faith in the process of knowledge. Positivism results from the conception that seeing is believing. The p pragmatist claims no quarrel with common sense: surely chairs and electrons are equally real, if indeed we shall never again come to

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doubt their value to us. The positivist says electrons cannot be believed in, because they can never be seen. So it goes through all the positivist litany. Where the positivist denies causation and explanation, the pragmatist, at least in the Peircian tradition, gladly accepts them — so long as they turn out to be both useful and enduring for future inquirers.

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8 A surrogate for truth

`Mob psychology' – that is how Imre Lakatos (1922–74) caricatured Kuhn's account of science. `Scientific method (or "logic of discovery"), conceived as the discipline of rational appraisal of scientific theories – and of criteria of *progress* – vanishes. We may of course still try to explain changes in " paradigms " in terms of social psychology. This is . . . Kuhn's way' (I, p. 31).[1] Lakatos utterly opposed what he claimed to be Kuhn's reduction of the philosophy of science to sociology. He thought that it left no place for the sacrosanct scientific values of truth, objectivity, rationality and reason.

Although this is a travesty of Kuhn the resulting ideas are important. The two current issues of philosophy of science are epistemological (rationality) and metaphysical (truth and reality). Lakatos *seems* to be talking about the former. Indeed he is universally held to present a new theory of method and reason, and he is admired by some and criticized by others on that score. If that is what Lakatos is up to, his theory of rationality is bizarre. It does not help us at all in deciding what it is reasonable to believe or do now. It is entirely backward-looking. It can tell us what decisions in past science were rational, but cannot help us with the future. In so far as Lakatos's essays bear on the future they are a bustling blend of platitudes and prejudices. Yet the essays remain compelling. Hence I urge that they are about something other than method and rationality. He is important precisely because he is addressing, not an epistemological issue, but a metaphysical one. He is concerned with truth or its absence. He thought science is our model of objectivity. We might try to explain that, by holding that a scientific proposition must say how things are. It must correspond to the truth. That is what makes science objective. Lakatos, educated in Hungary in an Hegelian and Marxist tradition, took for granted the

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i All references to Imre Lakatos in this chapter are to his *Philosophical Papers*, 2 Volumes (J. Worrall and G. Currie, eds.), Cambridge, 1978.

post-Kantian, Hegelian, demolition of correspondence theories. He was thus like Peirce, also formed in an Hegelian matrix, and who, with other pragmatists, had no use for what William James called the copy theory of truth.

At the beginning of the twentieth century philosophers in England and then in America denounced Hegel and revived correspondence theories of truth and referential accounts of meaning. These are still central topics of Anglophone philosophy. Hilary Putnam is instructive here. In *Reason, Truth and History* he makes his own attempt to terminate correspondence theories. Putnam sees himself as entirely radical, and writes `what we have here is the demise of a theory that lasted for over two thousand years' (p. 74). Lakatos and Peirce thought the death in the family occurred about two hundred years earlier. Yet both men wanted an account of the objective values of Western science. So they tried to find a substitute for truth. In the Hegelian tradition, they said it lies in process, in the nature of the growth of knowledge itself.

A history of methodologies

Lakatos presented his philosophy of science as the upshot of an historical sequence of philosophies.

This sequence will include the familiar facts about Popper, Carnap, Kuhn, about revolution and rationality, that I have already described in the Introduction. But it is broader in scope and far more stylized. I shall now run through this story. A good many of its peripheral assertions were fashionable among philosophers of science in 1965. These are simplistic opinions such as: there is no distinction in principle between statement of theory and reports of observation; there are no crucial experiments, for only with hindsight do we call an experiment crucial; you can always go on inventing plausible auxiliary hypo-theses that will preserve a theory; it is never sensible to abandon a theory without a better theory to replace it. Lakatos never gives a good or even a detailed argument for any of these propositions. Most of them are a consequence of a theory-bound philosophy and they are best revised or refuted by serious reflection on experimentation. I assess them in Part B, on Intervening. On crucial experiments and auxiliary hypotheses, see Chapter 15. On the distinctions between observation and theory, see Chapter to.

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Euclidean model and inductivism

In the beginning, says Lakatos, mathematical proof was the model of true science. Conclusions had to be demonstrated and made absolutely certain. Anything less than complete certainty was defective. Science was by definition infallible.

The seventeenth century and the experimental method of reasoning made this seem an impossible goal. Yet the tale is only modified as we pass from deduction to induction. If we cannot have secure knowledge let us at least have probable knowledge based on sure foundations. Observations rightly made shall serve as the basis. We shall generalize upon sound experiments, draw analogies, and build up to scientific conclusions. The greater the variety and quantity of observations that confirm a conclusion, the more probable it is. We may no longer have certainty, but we have high probability.

Here then are two stages on the high road to methodology: proof and probability. Hume, knowing the failure of the first, already cast doubts on the second by 1739. In no way can particular facts provide `good reason' for more general statements or claims about the future. Popper agreed, and so in turn does Lakatos.

Falsificationisms

Lakatos truncates some history of methodology but expands others. He even had a Popper 1, Popper 2, and a Popper 3, denoting increasingly sophisticated versions of what Lakatos had learned from Popper. All three emphasize the testing and falsifying of conjectures rather than verifying or confirming them. The simplest view would be, ` people propose, nature disposes'. That is, we think up theories, and nature junks them if they are wrong. That implies a pretty sharp distinction between fallible theories and basic observations of nature. The latter, once checked out, are a final and indubitable court of appeal. A theory inconsistent with an observation must be rejected.

This story of conjecture and refutation makes us think of a pleasingly objective and honest science. But it won't do: for one thing ` all theories are born refuted', or at least it is very common for a theory to be proposed even when it is known not to square with all the known facts. That was Kuhn's point about puzzle-solving normal science. Secondly (according to Lakatos), there is no firm theory–observation distinction. Thirdly there is a claim made by the great French historian of science, Pierre Duhem. He remarked that theories are tested via auxiliary hypotheses. In his example, if an astronomer predicts that a heavenly body is to be found in a certain location, but it turns up somewhere else, he need not revise his astronomy. He could perhaps revise the theory of the telescope (or produce a suitable account of how phenomena differ from reality (Kepler), or invent a theory of astronomical aberration (G.G. Stokes), or suggest that the Doppler effect works differently in outer space). Hence a recalcitrant observation does not necessarily refute a theory. Duhem probably thought that it is a matter of choice or convention whether a theory or one of its auxiliary hypotheses is to be revised. Duhem was an outstanding anti-realist, so such a conclusion was attractive. It is repugnant to the staunch instincts for scientific realism found in Popper or Lakatos.

So the falsificationist adds two further props. First, no theory is rejected or abandoned unless there is a better rival theory in existence. Secondly, one theory is better than another if it makes more novel predictions. Traditionally theories had to be consistent with the evidence. The falsificationist, says Lakatos, demands not that the theory should be consistent with the evidence, but that it should actually outpace it.

Note that this last item has a long history of controversy. By and large inductivists think that evidence consistent with a theory supports it, no matter whether the theory preceded the evidence or the evidence preceded the theory. More rationalistic and deductively oriented thinkers will insist on what Lakatos calls `the Leibniz–Whewell–Popper requirement that *the – well planned – building of pigeon holes must proceed much faster than the recording of facts which are to be housed in them'* (I, p. 100).

Research programmes

We might take advantage of the two spellings of the word, and use the American spelling `research program' to denote what investigators normally call a research program, namely a specific attack on a problem using some well-defined combination of theoretical and experimental ideas. A research program is a program of research which a person or group can undertake, seek funding for, obtain help with, and so on. What Lakatos spells as `research programme' is not much like that. It is more abstract, more historical. It is a sequence of developing theories that might last for centuries, and which might sink into oblivion for 8o years and then be revived by an entirely fresh infusion of facts or ideas.

In particular cases it is often easy to recognize a continuum of developing theories. It is less easy to produce a general characterization. Lakatos introduces the word `heuristic' to help. Now `heuristic' is an adjective describing a method or process that guides discovery or investigation. From the very beginnings of Artificial Intelligence in the 1950s, people spoke of heuristic procedures that would help machines solve problems. In *How to solve it* and other wonderful books, Lakatos's countryman and mentor, the mathematician Georg Polya, provided classic modern works on mathematical heuristics. Lakatos's work on the philosophy of mathematics owed much to Polya. He then adapted the idea of heuristics as a key to identifying research programmes. He says a research programme is defined by its positive and negative heuristic. The negative heuristic says: Hands off – don't meddle here. The positive heuristic says: Here is a set of problem areas ranked in order of importance – worry only about questions at the top of the list.

Hard cores and protective belts

The negative heuristic is the ` hard core' of a programme, a body of central principles which are never to be challenged. They are regarded as irrefutable. Thus in the Newtonian programme, we have at the core the three laws of dynamics and the law of gravitation. If planets misbehave, a Newtonian will not revise the gravitational law, but try to explain the anomaly by postulating a possibly invisible planet, a planet which, if need be, can be detected only by its perturbations on the solar system.

The positive heuristic is an agenda determining which problems are to be worked on. Lakatos imagines a healthy research pro-gramme positively wallowing in a sea of anomalies, but being none the less exuberant. According to him Kuhn's vision of normal science makes it almost a chance affair which anomalies are made

the object of puzzle-solving activity. Lakatos says on the contrary that there is a ranking of problems. A few are systematically chosen for research. This choice generates a ` protective belt' around the theory, for one attends only to a set of problems ordained in advance. Other seeming refutations are simply ignored. Lakatos uses this to explain, why, *pace* Popper, verification seems so important in science. People choose a few problems to work on, and feel vindicated by a solution; refutations, on the other hand, may be of no interest.

Progress and degeneration

What makes a research programme good or bad? The good ones are progressive, the bad ones are degenerating. A programme will be a sequence of theories T_1, T_2, T_3. . . . Each theory must be at least as consistent with known facts as its predecessor. The sequence is theoretically progressive if each theory in turn predicts some novel facts not foreseen by its predecessors. It is empirically progressive if some of these predictions pan out. A programme is simply *progressive,* if it is both theoretically and empirically progressive. Otherwise it is *degenerating.*

The degenerating programme is one that gradually becomes closed in on itself. Here is an example.' One of the famous success stories is that of Pasteur, whose work on microbes enabled him to save the French beer, wine and silk industries that were threatened by various small hostile organisms. Later we began to pasteurize milk. Pasteur also identified the micro-organisms that enabled him to vaccinate against anthrax and rabies. There evolved a research programme whose hard core held that every hitherto organic harm not explicable in terms of parasites or injured organs was to be explained in terms of micro-organisms. When many diseases failed to be caused by bacteria, the positive heuristic directed a search for something smaller, the virus. This progressive research programme had degenerating subprogrammes. Such was the enthusiasm for microbes that what we now call deficiency diseases *had* to be caused by bugs. In the early years of this century the leading professor of tropical disease, Patrick Manson, insisted that beriberi and some other deficiency diseases are caused by bacterial contagion. An

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2 K. Codell Carter, 'The germ theory, Beriberi, and the deficiency theory of disease', *Medical History* 21 (1977), pp. 119-36.

epidemic of beriberi was in fact caused by the new processes of steam-polishing rice, processes imported from Europe which killed off millions of Chinese and Indonesians whose staple food was rice. Vitamin B, in the hull of the rice was destroyed by polishing. Thanks largely to dietary experiments in the Japanese Navy, people gradually came to realize that not presence of microbes, but absence of something in polished rice was the problem. When all else failed, Manson insisted that there are bacteria that live and die in the polished but not in the unpolished rice, and they are the cause of the new scourge. This move was theoretically degenerating because each modification in Manson's theory came only after some novel observations, not before, and it was empirically degenerating because no polished-rice-organisms are to be found.

Hindsight

We cannot tell whether a research programme is progressive until after the fact. Consider the splendid problem shift of the Pasteur programme, in which viruses replace bacteria as the roots of most evils that persist in the developed world. In the 1960s arose the speculation that cancers – carcinomas and lymphomas – are caused by viruses. A few extremely rare successes have been recorded. For example, a strange and horrible tropical lymphoma (Burrito's lymphoma) that causes grotesque swellings in the limbs of people who live above 5000 feet near the equator, has almost certainly been traced to a virus. But what of the general cancer-virus programme? Lakatos tells us, 'We must take budding programmes leniently; programmes may take decades before they get off the ground and become empirically progressive' (I, p. 6). Very well, but even if they have been progressive in the past – what more so than Pasteur's programme – that tells us exactly nothing except ` Be open-minded, and embark on numerous different kinds of research if you are stymied.' It does not merely fail to help choose new programmes with no track record. We know of few more progressive programmes than that of Pasteur, even if some of its failures have been hived off, for example into the theory of deficiency diseases. Is the attempt to find cancer viruses progressive or degenerating? We shall know only later. If we were trying to decide what proportion of the `War on Cancer' to spend on molecular biology and what on viruses (not necessarily mutually exclusive, of course) Lakatos could tell us nothing.

Objectivity and subjectivism

What then was Lakatos doing? My guess is indicated by the title of this chapter. He wanted to find a substitute for the idea of truth. This is a little like Putnam's subsequent suggestion, that the correspondence theory of truth is mistaken, and truth is whatever it is rational to believe. But Lakatos is more radical than Putnam. Lakatos is no born-again pragmatist. He is down on truth, not just a particular theory of truth. He does not want a replacement for the correspondence theory, but a replacement for truth itself. Putnam has to fight himself away from a correspondence theory of truth because, in English-speaking philosophy, correspondence theories, despite the pragmatist assault of long ago, are still popular. Lakatos, growing up in an Hegelian tradition, almost never gives the correspondence theory a thought. However, like Peirce, he values an objectivity in science that plays little role in Hegelian discourse. Putnam honours this value by hoping, like Peirce, that there is a scientific method upon which we shall come to agree, and which in turn will lead us all to agreement, to rational, warranted, belief. Putnam is a simple Peircian, even if he is less confident than Peirce that we are already on the final track. Rationality looks forward. Lakatos went one step further. There is no forward-looking rationality, but we can comprehend the objectivity of our present beliefs by reconstructing the way we got here. Where do we start? With the growth of knowledge itself.

The growth of knowledge

The one fixed point in Lakatos's endeavour is the simple fact that knowledge does grow. Upon this he tries to build his philosophy without representation, starting from the fact that one can see that knowledge grows whatever we think about `truth' or `reality'. Three related aspects of this fact are to be noticed.

First, one can see by direct inspection that knowledge has grown. This is not a lesson to be taught by general philosophy or history but by detailed reading of specific sequences of texts. There is no doubt that more is known now than was grasped by past genius. To take an

example of his own, it is manifest that after the work of Rutherford and Soddy and the discovery of isotopes, vastly more was known about atomic weights than had been dreamt of by a century of toilers after Prout had hypothesized in 1815 that hydrogen is the stuff of the universe, and that atomic weights are integral multiples of that of hydrogen. I state this to remind ourselves that Lakatos starts from a profound but elementary point. The point is not that there is knowledge but that there is growth; we know more about atomic weights than we once did, even if future times plunge us into quite new, expanded, reconceptualizations of those domains.

Secondly, there is no *arguing* that some historical events do exhibit the growth of knowledge. What is needed is an *analysis* that will say in what this growth consists, and tell us what is the growth that we call science and what is not. Perhaps there are fools who think that the discovery of isotopes is no growth in real knowledge. Lakatos's attitude is that they are not to be contested – they are likely idle and have never read the texts or engaged in the experimental results of such growth. We should not argue with such ignoramuses. When they have learned how to use isotopes or simply read the texts, they will find out that knowledge does grow.

This thought leads to the third point. The growth of scientific knowledge, given an intelligent analysis, might provide a demarcation between rational activity and irrationalism. Although Lakatos expressed matters in that way, it is not the right form of words to use. Nothing has grown more consistently and persistently over the years than the commentaries on the Talmud. Is that a rational activity? We see at once how hollow is that word `rational' if used for positive evaluation. The commentaries are the most reasoned great bodies of texts that we know, vastly more reasoned than the scientific literature. Philosophers often pose the tedious question of why twentieth-century Western astrology, such as it is, is no science. That is not where the thorny issues of demarcation lie. Popper took on more serious game in challenging the right of psychoanalysis or Marxist historiography to the claim of `science'. The machinery of research programmes, hard cores and protective belts, progress and degeneration, must, if it is of worth, effect a distinction not between the rational and reasoning, and the irrational and unreasoning, but between those reasonings which lead to what Popper and Lakatos call objective knowledge and those which pursue different aims and have different intellectual trajectories.

Appraising scientific theories

Hence Lakatos provides no forward-looking assessments of present competing scientific theories. He can at best look back and say why, on his criteria, this research programme was progressive, why another was not. As for the future, there are few pointers to be derived from his `methodology'. He says that we should be modest in our hopes for our own projects because rival programmes may turn out to have the last word. There is a place for pig-headedness when one's programme is going through a bad patch. The mottos are to be proliferation of theories, leniency in evaluation, and honest `score-keeping' to see which programme is producing results and meeting new challenges. These are not so much real methodology as a list of the supposed values of a science allegedly free of ideology.

If Lakatos were in the business of theory appraisal, then I should have to agree with his most colourful critic, Paul Feyerabend. The main thrust of the often perceptive assaults on Lakatos to be found in Chapter 17 of *Against Method is* that Lakatos's `methodology' is not a good device for advising on current scientific work. I agree, but suppose that was never the point of the analysis which, I claim, has a more radical object. Lakatos had a sharp tongue, strong opinions and little difference. He made many entertaining observations about this or that current research project, but these acerbic asides were incidental to and independent of the philosophy I attribute to him.

Is it a defect in Lakatos's methodology that it is only retroactive? I think not. There are no significant general laws about what, in a current bit of research, bodes well for the future. There are only truisms. A group of workers who have just had a good idea often spends at least a few more years fruitfully applying it. Such groups properly get lots of money from corporations, governments, and foundations. There are other mild sociological inductions, for example that when a group is increasingly concerned to defend itself against criticism, and won't dare go out on a new limb, then it seldom produces interesting new research. Perhaps the chief practical problem is quite ignored by philosophers of rationality. How do you stop funding a program you have supported for five or

fifteen years – a program to which many young people have dedicated their careers – and which is finding out very little? That real-life crisis has little to do with philosophy.

There is a current vogue among some philosophers of science, that Lakatos might have called `the new justifications'. It produces whole books trying to show that a system of appraising theories can be built up out of rules of thumb. It is even suggested that governments should fund work in the philosophy of science, in order to learn how to fund projects in real science. We should not confuse such creatures of bureaucracy with Lakatos's attempt to understand the content of objective judgement.

Internal and external history

Lakatos's tool for understanding objectivity was something he called history. Historians of science, even those given to considerable flights of speculative imagination, find in Lakatos only ` an historical parody that makes one's hair stand on end'. That is Gerald Holton's characterization in *The Scientific Imagination* (p. 106); many colleagues agree.

Lakatos begins with an `unorthodox, new demarcation between " internal " and " external" history' (I, p. 102), but is not very clear what is going on. External history commonly deals in economic, social and technological factors that are not directly involved in the content of a science, but which are deemed to influence or explain some events in the history of knowledge. External history might include an event like the first Soviet satellite to orbit the .earth – Sputnik – which was followed by the instant investment of vast sums of American money in science education. Internal history is usually the history of ideas germane to the science, and attends to the motivations of research workers, their patterns of communication and lines of intellectual filiation – who learned what from whom.

Lakatos's internal history is to be one extreme on this spectrum. It is to exclude anything in the subjective or personal domain. What people believed is irrelevant: it is to be a history of some sort of abstraction. It is, in short, to be a history of Hegelian alienated knowledge, the history of anonymous and autonomous research programmes.

This idea about the growth of knowledge into something objective and non-human was foreshadowed in his first major philosophical work, *Proofs and Refutations.* On p. 146 of this wonderful dialogue on the nature of mathematics, we find:

> Mathematical activity is human activity. Certain aspects of this activity — as of any human activity — can be studied by psychology, others by history. Heuristic is not primarily interested in these aspects. But mathematical activity produces mathematics. Mathematics, this product of human activity, `alienates itself' from the human activity which has been producing it. It becomes a living growing organism that acquires a certain autonomy from the activity which has produced it.

Here then are the seeds of Lakatos's redefinition of `internal history', the doctrine underlying his `rational reconstructions'. One of the lessons of *Proofs and Refutations* is that mathematics might be both the product of human activity and autonomous, with its own internal characterization of objectivity which can be analysed in terms of how mathematical knowledge has grown. Popper has suggested that such objective knowledge could be a `third world' of reality, and Lakatos toyed with this idea.

Popper's metaphor of a third world is puzzling. In Lakatos's definition, `the "first world" is the physical world; the "second world" is the world of consciousness, of mental states and, in particular, of beliefs; the "third world" is the Platonic world of objective spirit, the world of ideas' (II, p. 108). I myself prefer those texts of Popper's where he says that the third world is a world of books and journals stored in libraries, of diagrams, tables and computer memories. Those extra-human things, uttered sentences, are more real than any talk of Plato would suggest.

Stated as a list of three worlds we have a mystery. Stated as a sequence of three emerging kinds of entity with corresponding laws it is less baffling. First there was the physical world. Then when sentient and reflective beings emerged out of that physical world there was also a second world whose descriptions could not be in any general way reduced to physical world descriptions. Popper's third world is more conjectural. His idea is that there is a domain of human knowledge (sentences, print-outs, tapes) which is subject to its own descriptions and laws and which cannot be reduced to second-world events (type by type) any more than second-world events can be reduced to first-world ones. Lakatos persists in the metaphorical expression of this idea: `The *products* of human

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knowledge; propositions, theories, systems of theories, problems, problemshifts, research programmes live and grow in the "third world"; the producers of knowledge live in the first and second worlds' (II, p. l o8). One need not be so metaphorical. It is a difficult but straightforward question whether there is an extensive and coherent body of description of ` alienated' and autonomous human knowledge that cannot be reduced to histories and psychologies of subjective beliefs. A substantiated version of a `third world' theory can provide just the domain for the content of mathematics. It admits that mathematics is a product of the human mind, and yet is also autonomous of anything peculiar to psychology. An extension of this theme is provided by Lakatos's conception of `unpsychological' internal history.

Internal history will be a rational construction of what actually happened, one which displays why what happened in many of the best incidents of the history of science are worthy of designations such as `rational' and `objective'. Lakatos had a fine sounding maxim, a parody of one of Kant's noble turns of phrase: 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind.' That sounds good, but Kant had been speaking of something else. All we need to say about rather unreflective history of science was said straightforwardly by Kant himself in his lectures on *Logic:* `Mere polyhistory is *a cyclopean* erudition that lacks one eye, the eye of philosophy.' Lakatos wants to rewrite the history of science so that the `best' incidents in the history of science are cases of progressive research programmes.

Rational reconstruction

Lakatos has a problem, to characterize the growth of knowledge internally by analysing examples of growth. There is a conjecture, that the unit of growth is the research programme (defined by hard core, protective belt, heuristic) and that research programmes are progressive or degenerating and, finally, that knowledge grows by the triumph of progressive programmes over degenerating ones. To test this supposition we select an example which must prima facie illustrate something that scientists have found out. Hence the example should be currently admired by scientists, or people who think about the appropriate branch of knowledge, not because we

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kow-tow to orthodoxy, but because workers in a given domain tend to have a better sense of what matters than laymen. Feyerabend calls this attitude elitism. Is it? The next Lakatosian injunction is for all of us to read all the texts we can lay hands on, covering a complete epoch spanned by the research programme, and the entire array of practitioners. Yes, that is elitism because few can afford the time to read. But it has an anti-elite intellectual premise (as opposed to an elite economic premise) that if texts are available, anyone is able to read them.

Within what we read we must select the class of sentences that express what the workers of the day were trying to find out, and how they were trying to find it out. Discard what people felt about it, the moments of creative hype, even their motivation or their role models. Having settled on such an ` internal' part of the data we can now attempt to organize the result into a story of Lakatosian research programmes.

As in most inquiries, an immediate fit between conjecture and articulated data is not to be expected. Three kinds of revision may improve the mesh between conjecture and selected data. First, we may fiddle with the data analysis, secondly, we may revise the conjecture, and thirdly, we may conclude that our chosen case study does not, after all, exemplify the growth of knowledge. I shall discuss these three kinds of revision in order.

By improving the analysis of data I do not mean lying. Lakatos made a couple of silly remarks in his `falsification' paper, where he asserts something as historical fact in the text, but retracts it in the footnotes, urging that we take his text with tons of salt (I, p. 55)· The historical reader is properly irritated by having his nose tweaked in this way. No point was being served. Lakatos's little joke was not made in the course of a rational reconstruction despite the fact that he said it was. Just as in any other inquiry, there is nothing wrong with trying to re-analyse the data. That does not mean lying. It may mean simply reconsidering or selecting and arranging the facts, or it may be a case of imposing a new research programme on the known historical facts.

If the data and the Lakatosian conjecture cannot be reconciled, two options remain. First, the case history may itself be regarded as something other than the growth of knowledge. Such a gambit could easily become monster-barring, but that is where the

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constraint of external history enters. Lakatos can always say that a particular incident in the history of science fails to fit his model because it is ` irrational', but he imposes on himself the demand that one should allow this only if one can say what the irrational element is. External elements may be political pressure, corrupted values or, perhaps, sheer stupidity. Lakatos's histories are normative in that he can conclude that a given chunk of research `ought not to have' gone the way it did, and that it went that way through the interference of external factors not germane to the programme. In concluding that a chosen case was not `rational' it is permissible to go against current scientific wisdom. But although in principle Lakatos can countenance this, he is properly moved by respect for the implicit appraisals of working scientists. I cannot see Lakatos willingly conceding that Einstein, Bohr, Lavoisier or even Copernicus was participating in an irrational programme. `Too much of the actual history of science' would then become `irrational' (I, p. 172). We have no standards to appeal to, in Lakatos's programme, other than the history of knowledge as it stands. To declare it to be globally irrational is to abandon rationality. We see why Feyerabend spoke of Lakatos's elitism. Rationality will simply be defined by what a present community calls good, and nothing shall counterbalance the extraterrestrial weight of an Einstein.

Lakatos then defines objectivity and rationality in terms of progressive research programmes, and allows an incident in the history of science to be objective and rational if its internal history can be written as a sequence of progressive problem shifts.

Cataclysms in reasoning

Peirce defined truth as what is reached by an ideal end to scientific inquiry. He thought that it is the task of methodology to characterize the principles of inquiry. There is an obvious problem: what if inquiry should not converge on anything? Peirce, who was as familiar in his day with talk of scientific revolutions as we are in ours, was determined that `cataclysms' in knowledge (as he called them) have not been replaced by others, but this is all part of the self-correcting character of inquiry. Lakatos has an attitude similar to Peirce's. He was determined to refute the doctrine that he attributed to Kuhn, that knowledge changes by irrational 'conversions' from one paradigm to another.

As I said in the Introduction, I do not think that a correct reading of Kuhn gives quite the apocalyptic air of cultural relativism that Lakatos found there. But there is a really deep worry underlying Lakatos's antipathy to Kuhn's work, and it must not be glossed over. It is connected with an important side remark of Feyerabend's, that Lakatos's accounts of scientific rationality at hest fit the major achievements `of the last couple of hundred years'.

A body of knowledge may break with the past in two distinguishable ways/ By now we are all familiar with the possibility that new theories may completely replace the conceptual organization of their predecessors. Lakatos's story of progressive and degenerating programmes is a good stab at deciding when such replacements are ` rational'. But all of Lakatos's reasoning takes for granted what we may call the hypothetico-deductive model of reasoning. For all his revisions of Popper, he takes for granted that conjectures are made and tested against some problems chosen by the protective belt. A much more radical break in knowledge occurs when an entirely new style of reasoning surfaces. The force of Feyerabend's gibe about `the last couple of hundred years' is that Lakatos's analysis is relevant not to timeless knowledge and timeless reason, but to a particular kind of knowledge produced by a particular style of reasoning/ That knowledge and that style have specific beginnings. So the Peircian fear of cataclysm becomes: Might there not be further styles of reasoning which will produce yet a new kind of knowledge? Is not Lakatos's surrogate for truth a local and recent phenomenon?

I am stating a worry, not an argument. Feyerabend makes sensational but implausible claims about different modes of reason-ing and even seeing in the archaic past. In a more pedestrian way my own book, *The Emergence of Probability* (1975), contends that part of our present conception of inductive evidence came into being only at the end of the Renaissance. In his book, *Styles of Scientific Thinking in the European Tradition* (1983), the historian A/C. Crombie, from whom I take the word `style', writes of six distinguishable styles/ I have elaborated Crombie's idea elsewhere/ Now it does not follow that the emergence of a new style is a cataclysm. Indeed we may add style to style, with a cumulative body of conceptual tools. That is what Crombie teaches. Clearly both

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nd Laudan expect this to happen. But these are matters only recently broached, and are utterly ill-understood. uld make us chary of an account of reality and objectivity rts from the growth of knowledge, when the kind of scribed turns out to concern chiefly a particular knowieved by a particular style of reasoning.

e matters worse, I suspect that a style of reasoning may the very nature of the knowledge that it produces/ The anal method of the Greeks gave a geometry which long the philosopher's model of knowledge. Lakatos inveighs e domination of the Euclidean mode. What future Lakatos h against the hypothetico-deductive mode and the theory h programmes to which it has given birth? One of the most eatures of this mode is the postulation of theoretical which occur in high-level laws, and yet which have atal consequences. This feature of successful science endemic only at the end of the eighteenth century/ Is it ible that the questions of objectivity, asked for our times are precisely the questions posed by this new knowledge? n it is entirely fitting that Lakatos should try to answer stions in terms of the knowledge of the past two centuries. zld be wrong to suppose that we can get from this specific owth to a theory of truth and reality. To take seriously the ook that Lakatos proposed, but never lived to write, `The logic of scientific discovery' is to take seriously the y that Lakatos has, like the Greeks, made the eternal lepend on a mere episode in the history of human

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remains an optimistic version of this worry/ Lakatos was characterize certain objective values of Western science n appeal to copy theories of truth. Maybe those objective recent enough that his limitation to the past two or three is exactly right. We are left with no external way to >ur own tradition, but why should we want that?

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BREAK

Reals and representations

Incommensurability, transcendental nominalism, surrogates for truth, and styles of reasoning are the jargon of philosophers. They arise from contemplating the connection between theory and the world. All lead to an idealist cul-de-sac. None invites a healthy sense of reality. Indeed much recent philosophy of science parallels seventeenth-century epistemology. By attending only to knowledge as representation of nature, we wonder how we can ever escape from representations and hook-up with the world. That way lies an idealism of which Berkeley is the spokesman/ In our century John Dewey has spoken sardonically of a spectator theory of knowledge that has obsessed Western philosophy. If we are mere spectators at the theatre of life, how shall we ever know, on grounds internal to the passing show, what is mere representation by the actors, and what is the real thing? If there were a sharp distinction between theory and observation, then perhaps we could count on what is observed as real, while theories, which merely represent, are ideal/ But when philosophers begin to teach that all observation is loaded with theory, we seem completely locked into representation, and hence into some version of idealism.

Pity poor Hilary Putnam, for example/ Once the most realist of philosophers, he tried to get out of representation by tacking `reference' on at the end of the list of elements that constitute the meaning of a word. It was as if some mighty referential sky-hook could enable our language to embed within it a bit of the very stuff to which it refers. Yet Putnam could not rest there, and ended up as an ` internal realist' only, beset by transcendental doubts, and given to some kind of idealism or nominalism.

I agree with Dewey. I follow him in rejecting the false dichotomy between acting and thinking from which such idealism arises. Perhaps all the philosophies of science that I have described are part of a larger spectator theory of knowledge. Yet I do not think that the idea of knowledge as representation of the world is in itself the

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source of that evil. The harm comes from a single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment. That is why in the next part of this book I study experimental science, and find in it the sure basis of an uncontentious realism. But before abandoning theory for experiment, let us think a little more about the very notions of representation and reality.

The origin of ideas

What are the origins of these two ideas, *representation* and *reality?* Locke might have asked that question as part of a psychological inquiry, seeking to show how the human mind forms, frames, or constitutes its ideas. There is a legitimate science that studies the maturation of human intellectual abilities, but philosophers often play a different game when they examine the origin of ideas. They tell fables in order to teach philosophical lessons. Locke himself was fashioning a parable when he pretended to practice the natural history of the mind. Our modern psychologies have learned how to trick themselves out in more of the paraphernalia of empirical research, but they are less distant from fantastical Locke than they assume. Let us, as philosophers, welcome fantasies. There may be more truth in the average *a priori* fantasy about the human mind than in the supposedly disinterested observations and mathematical model-building of cognitive science.

Philosophical anthropology

Imagine a philosophical text of about 1850: `Reality is as much an anthropomorphic creation as God Himself.' This is not to be uttered in a solemn tone of voice that says, `God is dead and so is reality.' It is to be a more specific and practical claim: *Reality is just a byproduct of an anthropological fact.* More modestly, the concept of reality is a byproduct of a fact about human beings.

By anthropology I do not mean ethnography or ethnology, the studies practised in present-day departments of anthropology, and which involve lots of field work. By anthropology I mean the bogus nineteenth-century science of `Man'. Kant once had three philosophical questions. What must be the case? What should we do? For what may we hope? Late in life he added a fourth question: *What is **Man?*** **With this he inaugurated *(philosophische) Anthropologie* and**

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even wrote a book **called *Anthropology.*** Realism is not to be considered part of pure reason, nor judgement, nor the metaphysics of morals, nor even the metaphysics of natural science. If we are to give it classification according to the titles of Kant's great books, realism shall be studied as part of *Anthropologie* itself.

A Pure Science of Human Beings is a bit risky. When Aristotle proposed that Man is an animal that lives in cities, so that the *polis* is a part of Man's nature to which He strives, his pupil Alexander refuted him by re-inventing the Empire. We have been told that Man is a tool-maker, or a creature that has a thumb, or that stands erect. We have been told that these fortuitous features are noticed only by attending to half of the species wrongly called Man, and that tools, thumbs and erectness are scarcely what define the race. It is seldom clear what the grounds might be for any such statements, pro or con. Suppose one person defines humans as rational, and another person defines them as the makers of tools. Why on earth should we suppose that being a rational animal is co-extensive with making tools?

Speculations about the essential nature of humanity license more of the same. Philosophers since Descartes have been attracted by the conjecture that humans are speakers. It has been urged that rationality, of its very nature, demands language, so humans as rational animals, and humans as speakers are indeed co-extensive. That is a satisfactory main theorem for a subject as feeble as fanciful anthropology. Yet despite the manifest profundity of this conclusion, a conclusion that has fuelled mighty books, I propose another fancy. *Human beings are representers.* Not *homo faber,* I say, but *homo depictor.* People make representations.

Limiting the metaphor

People make likenesses. They paint pictures, imitate the clucking of hens, mould clay, carve statues, and hammer brass. Those are the sorts of representations that begin to characterize human beings.

The word `representation' has quite a philosophical past. It has been used to translate Kant's word *Vorstellung,* a placing before the mind, a word which includes images as well as more abstract thoughts. Kant needed a word to replace the `idea' of the French and English empiricists. That is exactly what I do *not* mean by representation. Everything I call a representation is public. You

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cannot touch a Lockeian idea, but only the museum guard can stop you touching some of the first representations made by our predecessors/ I do not mean that all representations can be touched, but all are public. According to Kant, a judgement is a representation of a representation, a putting before the mind of a putting before the mind, doubly private. That is doubly not what I call a representation. But for me, some public verbal events can be representations. I think not of simple declarative sentences, which are surely not representations, but of complicated speculations which attempt to represent our world.

When I speak of representations I first of all mean physical objects: figurines, statues, pictures, engravings, objects that are themselves to be examined, regarded. We find these as far back as we find anything human. Occasionally some fortuitous event preserves even fragments of wood or straw that would otherwise have rotted. Representations are external and public, be they the simplest sketch on a wall, or, when I stretch the word 'representation', the most sophisticated theory about electromagnetic, strong, weak, or gravitational forces.

The ancient representations that are preserved are usually visual and tactile, but I do not mean to exclude anything publicly accessible to the other senses. Bird whistles and wind machines may make likenesses too, even though we usually call the sounds that they emit imitations. I claim that if a species as smart as human beings had been irrevocably blind, it would have got on fine with auditory and tactile representations, for to represent is part of our very nature. Since we have eyes, most of the first representations were visual, but representation is not of its essence visual.

Representations are intended to be more or less public likenesses. I exclude Kant's *Vorstellungen* and Lockeian internal ideas that represent the external world in the mind's eye. I also exclude ordinary public sentences. William James jeered at what he called the copy theory of truth, which bears the more dignified label of correspondence theory of truth. The copy theory says that true propositions are copies of whatever in the world makes them true. Wittgenstein's *Tractatus* has a picture theory of truth, according to which a true sentence is one which correctly pictures the facts. Wittgenstein was wrong. Simple sentences are not pictures, copies, or representations. Doubtless philosophical talk of representation

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invites memories of Wittgenstein's *Sätze.* Forget them. The sentence, `the cat is on the mat', is no representation of reality. As Wittgenstein later taught us, it is a sentence that can be used for all sorts of purposes, none of which is to portray what the world is like. On the other hand, Maxwell's electromagnetic theories were intended to represent the world, to say what it is like. Theories, not individual sentences, are representations.

Some philosophers, realizing that sentences are not representations, conclude that the very idea of a representation is worthless for philosophy. That is a mistake. We can use complicated sentences collectively in order to represent. So much is ordinary English idiom. A lawyer can represent the client, and can also represent that the police collaborated improperly in preparing their reports. *A single* sentence will in general not represent. A representation can be verbal, but a verbal representation will use a good many verbs.

Humans as speakers

The first proposition of my philosophical anthropology is that human beings are depictors. Should the ethnographer tell me of a race that makes no image (not because that is tabu but because no one has thought of representing anything) then I would have to say that those are not people, not *homo depictor*. If we are persuaded that humankind (and not its predecessors) lived in Olduvai gorge three million years ago, and yet we find nothing much except old skulls and footprints, I would rather postulate that the representations made by those African forbears have been erased by sand, rather than that people had not yet begun to represent.

How does my *a priori* paleolithic fantasy mesh with the ancient idea that humans are essentially rational and that rationality is essentially linguistic? Must I claim that depiction needs language or that humanity need not be rational? If language has to be tucked into rationality, I would cheerfully conclude that humans may *become* rational animals. That is, *homo depictor* did not always deserve Aristotle's accolade of rationality, but only earned it as we smartened up and began to talk. Let us imagine, for a moment, pictorial people making likenesses before they learn to talk.
tations

The beginnings of language

Speculation on the origin of language tends to be unimaginative and condescending. Language, we hear, must have been invented to help with practical matters such as hunting and farming. `How useful,' goes the refrain, `to be able to talk. How much more efficient people would have been if they could talk. Speech makes it much more likely that hunters and farmers will survive/'

Scholars who favour such rubbish have evidently never ploughed a field nor stalked game, where silence is the order of the day, not jabber/ People out in the fields weeding do not usually talk. They talk only when they rest. In the plains of East Africa the hunter with the best kill rate is the wild dog, yet middle-aged professors short of wind and agreeing never to talk nor signal are much better at catching the beeste and the gazelle than any wild dog. The lion that roars and the dogs that bark will starve to death if enough silent humans are hunting with their bare hands.

Language is not for practical affairs/ Jonathan Bennett tells a story about language beginning when one ` tribesman' warns another that a coconut is about to fall on the second native's head.' Native One does this first by an overacted mime of bonking on the head, and later on does this by uttering a warning and thereby starting language. I bet that no coconut ever fell on any tribesman's head except in racist comic strips, so I doubt this fantasy. I prefer a suggestion about language attributed to the Leakey family who excavate Olduvai gorge. The idea is that people invented language out of boredom. Once we had fire, we had nothing to do to pass away the long evenings, so we started telling jokes. This fancy about the origin of language has the great merit of regarding speech as something human. It fixates not on tribesmen in the tropics but on people.

Imagine *homo depictor* beginning to use sounds that we might translate as `real', or, `that's how it is', said of a clay figurine or a daub on the wall. Let discourse continue as `this real, then that real', or, more idiomatically, ` if this is how it is, then that is how it is too'. Since people are argumentative, other sounds soon express, `no, not that, but this here is real instead'.

((footnote:))

i J/ Bennett, `The meaning-nominalist strategy', *Foundations of Language* to (1973), pp/ 141-68/

In such a fantasy we do not first come to the names and descriptions, or the sense and reference of which philosophers are so fond. Instead we start with the indexicals, logical constants, and games of seeking and finding. Descriptive language comes later, not as a surrogate for depiction but as other uses for speaking are invented.

Language then starts with `this real', said of a representation/ Such a story has to its credit the fact that `this real' is not at all like `You Tarzan, Me Jane', for it stands for a complicated, that is, characteristically human, thought, namely that this wooden carving shows something real about what it represents.

This imagined life is intended as an antidote to the deflating character of the quotation with which I began: Reality is an anthropomorphic creation. Reality may be a human creation, but it is no toy; on the contrary it is the second of human creations. The first peculiarly human invention is representation. Once there is a practice of representing, a second-order concept follows in train. This is the concept of reality, a concept which has content only when there are first-order representations.

It will be protested that reality, or the world, was there before any representation or human language. Of course/ But conceptualizing it as reality is secondary. First there is this human thing, the making of representations. Then there was the judging of representations as real or unreal, true or false, faithful or unfaithful/ Finally comes the world, not first but second, third or fourth.

In saying that reality is parasitic upon representation, I do not join forces with those who, like Nelson Goodman or Richard Rorty, exclaim, `the world well lost!' The world has an excellent place, even if not a first one. It was found by conceptualizing the real as an attribute of representations.

Is there the slightest empirical evidence for my tale about the origin of language? No. There are only straws in the wind. I say that representing is curiously human/ Call it species specific. We need only run up the evolutionary tree to see that there is some truth in this. Drug a baboon and paint its face, then show it a mirror. It notices nothing out of the ordinary. Do the same to a chimpanzee. It is terribly upset, sees there is paint on its face and tries to get if off. People, in turn, like mirrors to study their make-up. Baboons will never draw pictures. The student of language, David Premack, has

ivory carving of a person, perhaps a god, in what we call formal or lifeless style. I see the gold leggings and cloak in which the ivory was dressed. It is engraved in the most minute and ` realistic' detail with scenes of bull and lion. The archaic and the realistic objects in different media are made in what the archaeologists say is the same period. I do not know what either is for. I do know that both are likenesses. I see the archaic bronze charioteer with its compelling human deep-set eyes of semi-precious stone. How, I ask, could craftspeople so keen on what we call lifeless forms work with others who breathed life into their creations? Because different crafts using different media evolve at different rates? Because of a forgotten combination of unknown purposes? Such subtle questions are posed against a background of what we take for granted/ We know at least this: these artifacts are representations/

We know likeness and representation even when we cannot answer, likeness to what? Think of the strange little clay figures on which are painted a sketch of garments, but which have, instead of heads, little saucer shaped depressions, perhaps for oil. These finger-high objects litter Mycenae. I doubt that they represent any-thing in particular. They most remind me of the angel-impressions children make by lying in the snow and waving their arms and legs to and fro to create the image of little wings and skirt. Children make these angels for pleasure/ We do not quite know what the citizens of Cnossus did with their figurines/ But we know that both are in some way likenesses. The wings and skirt are like wings and skirt, although the angel depicted is like nothing on earth.

Representations are not in general intended to say how it is. They can be portrayals or delights. After our recent obsession with words it is well to reflect on pictures and carvings. Philosophers of language seldom resist the urge to say that the first use of language must be to tell the truth. There should be no such compulsion with pictures. To argue of two bison sketches, `If this is how it is, then that is how it is too', is to do something utterly unusual. Pictures are seldom, and statues are almost never used to say how things are. At the same time there is a core to representation that enables archaeologists millenia later to pick out certain objects in the debris of an ancient site, and to see them as likenesses. Doubtless `likeness' is the wrong word, because the `art' objects will surely include products of the imagination, pretties and uglies made for their own

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sake, for the sake of revenge, wealth, understanding, courtship or terror. But within them all there is a notion of representation that harks back to likeness. Likeness stands alone. It is not a relation. It creates the terms in a relation. There is first of all likeness, and then likeness to something or other. First there is representation, and then there is `real'/ First there is a representation and much later there is a creating of concepts in terms of which we can describe this or that respect in which we have similarity. But likeness can stand on its own without any need of some concepts x,y, or z, so that one must always think, like in represent of z, but not of x or y. There is no absurdity in thinking that there is a raw and unrefined notion of likeness springing up with the making of representations, and which, as people become more skilful in working with materials, engenders all sorts of different ways of noticing what is like what.

Realism no problem

If reality were just an attribute of representation, and we had not evolved alternative styles of representation, then realism would be a problem neither for philosophers nor for aesthetes. The problem arises because we have alternative systems of representation.

So much is the key to the present philosophical interest in scientific realism. Earlier ` realistic' crises commonly had their roots in science. The competition between Ptolemaic and Copernican systems begged for a shoot-out between instrumentalist and realistic cosmologies. Disputes about atomism at the end of the nineteenth century made people wonder if, or in what sense, atoms could be real. Our present debate about scientific realism is fuelled by no corresponding substantive issue in natural science. Where then does it come from? From the suggestions of Kuhn and others that with the growth of knowledge we may, from revolution to revolution, come to inhabit different worlds. New theories are new representations. They represent in different ways and so there are new kinds of reality. So much is simply a consequence of my account of reality as an attribute of representation.

When there were only undifferentiated representations then, in my fantasy story about the origin of language, `real' was un-equivocal. But as soon as representations begin to compete, we had to wonder what is real. Anti-realism makes no sense when only one kind of representation is around. Later it becomes possible. In our

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time we have seen this as the consequence of Kuhn's *Structure of Scientific Revolutions.* It is, however, quite an old theme in philosophy, best illustrated by the first atomists.

The Democritean dream

Once representation was with us, reality could not be far behind. It is an obvious notion for a clever species to cultivate. The prehistory of our culture is necessarily given by representations of various sorts, but all that are left us are tiny physical objects, painted pots, moulded cookware, inlay, ivory, wood, tiny burial tools, decorated walls, chipped boulders. *Anthropologie* gets past the phantasies I have constructed only when we have the remembered word, the epics, incantations, chronologies and speculations/ The pre-Socratic fragments would be so much mumbo-jumbo were it not for their lineage down to the strategies we now calmly call ` science'. Today's scientific realist attends chiefly to what was once called the inner constitution of things, so I shall pull down only one thread from the pre-Socratic skein, the one that leads down to atomism. Despite Leucippus, and other forgotten predecessors, it is natural to associate this with Democritus, a man only a little older than Socrates. The best sciences of his day were astronomy and geometry. The atomists were bad at the first and weak in the second, but they had an extraordinary hunch. Things, they supposed, have an inner constitution, a constitution that can be thought about, perhaps even uncovered. At least they could guess at this: atoms and the void are all that exist, and what we see and touch and hear are only modifications of this.

Atomism is not essential to this dream of knowledge. What matters is an intelligible organization behind what we take in by the senses. Despite the central role of cosmology, Euclidean proof, medicine and metallurgy in the formation of Western culture, our current problems about scientific realism stem chiefly from the Democritean dream. It aims at a new kind of representation. Yet it still aims at likeness. This stone, I imagine a Democritus saying, is not as it looks to the eye. It is like this – and here he draws dots in the sand or on the tablet, itself thought of as a void. These dots are in continuous and uniform motion, he says, and begins to tell a tale of particles that his descendants turn into odd shapes, springs, forces, fields, all too small or big to be seen or felt or heard except in the

aggregate. But the aggregate, continues Democritus, is none other than this stone, this arm, this earth, this universe.

Familiar philosophical reflections ensue. Scepticism is inevitable, for if the atoms and the void comprise the real, how can we ever know that? As Plato records in the *Gorgias,* this scepticism is three-pronged. All scepticism had had three prongs, since Democritus formulated atomism/ There is first of all the doubt that we could check out any particular version of the Democritean dream. If much later Lucretius adds hooks to the atoms, how can we know if he or another speculator is correct? Secondly, there is a fear that this dream is only a dream; there are no atoms, no void, just stones, about which we can, for various purposes, construct certain models whose only touchstone, whose only basis of comparison, whose only reality, is the stone itself. Thirdly, there is the doubt that, although we cannot possibly believe Democritus, the very possibility of his story shows that we cannot credit what we see for sure, and so perhaps we had better not aim at knowledge but at the contemplative ignorance of the tub/

Philosophy is the product of knowledge, no matter how sketchy be the picture of what is known. Scepticism of the sort ` do I know this is a hand before me' is called `naive' when it would be better described as degenerate. The serious scepticism which is associated with it is not, `is this a hand rather than a goat or an hallucination?' but one that originates with the more challenging worry that the hand represented as flesh and bone is false, while the hand represented as atoms and the void is more correct. Scepticism is the product of atomism and other nascent knowledge. So is the philosophical split between appearance and reality. According to the Democritean dream, the atoms must be like the inner constitution of the stone. If `real' is an attribute of depiction, then in asserting his doctrine, Democritus can only say that his picture of particles pictures reality. What then of the depiction of the stone as brown, encrusted, jagged, held in the hand? That, says the atomist, must be appearance.

Unlike its opposite, reality, `appearance' is a thoroughly philosophical concept. It imposes itself on top of the initial two tiers of representation and reality. Much philosophy misorders this triad. Locke thought that we have appearance, then form mental representations and finally, seek reality. On the contrary, we make

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public representations, form the concept of reality, and, as systems of representation multiply, we become sceptics and form the idea of mere appearance/

No one calls Democritus a scientific realist: `atomism' and `materialism' are the only `isms' that fit. I take atomism as the natural step from the Stone Age to scientific realism, because it lays out the notion of an `inner constitution of things'. With this seventeenth-century phrase, we specify a constitution to be thought about and, hopefully, to be uncovered. But no one did find out about atoms for a long, long time. Democritus transmitted a dream, but no knowledge. Complicated concepts need criteria of application. That is what Democritus lacked. He did not know enough beyond his speculations to have criteria of whether his picture was of reality or not. His first move was to shout `real' and slander the looks of things as mere appearance/ Scientific realism or anti-realism do not become possible doctrines until there are criteria for judging whether the inner constitution of things is as represented.

The criteria of reality

Democritus gave us one representation: the world is made up of atoms. Less occult observers give us another. They painted pebbles on the beach, sculpted humans and told tales. In my account, the word `real' first meant just unqualified likeness. But then clever people acquired conjectured likenesses in manifold respects. `Real' no longer was unequivocal. As soon as what we would now call speculative physics had given us alternative pictures of reality, metaphysics was in place. Metaphysics is about criteria of reality. Metaphysics is intended to sort good systems of representation from bad ones. Metaphysics is put in place to sort representations when the only criteria for representations are supposed to be internal to representation itself.

That is the history of old metaphysics and the creation of the problem of realism. The new era of science seemed to save us from all that. Despite some philosophical malcontents like Berkeley, the new science of the seventeenth century could supplant even organized religion and say that it was giving the true representation of the world. Occasionally one got things wrong, but the overthrow of false ideas was only setting us on what was finally the right path. Thus the chemical revolution of Lavoisier was seen as a real

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revolution. Lavoisier got some things wrong: I have twice already used the example of his confidence that all acids have oxygen in them. So we sorted that out. In 1816 the new professor of chemistry at Harvard College relates the history of chemistry in an inaugural lecture to the teenagers then enrolled. He notes the revolutions of the recent past, and says we are now on the right road/ From now on there will only be corrections. All of that was fine until it began to be realized that *there might be several ways to represent the same facts.*

I do not know when this idea emerged. It is evident in the important posthumous book of 1894, Heinrich Hertz's *Principles of Mechanics.* This is a remarkable work, often said to have led Wittgenstein towards his picture theory of meaning, the core of his 1918 *Tractatus Logico-Philosophicus.* Perhaps this book, or its 1899 English translation, first offers the explicit terminology of a scientific `image' – now immortalized in the opening sentence of Kuhn's *Structure,* and, following Wilfred Sellars, used as the title of van Fraassen's anti-realist book. Hertz presents `three images of mechanics' – three different ways to represent the then extant knowledge of the motions of bodies. Here, for perhaps the first time, we have three different systems of representation shown to us/ Their merits are weighed, and Hertz favours one.

Hence even within the best understood natural science – mechanics – Hertz needed criteria for choosing between representations. It is not only the artists of the 1870s and 1880s who are giving us new systems of representation called post-impressionism or whatever. Science itself has to produce criteria of what is `like', of what shall count as the right representation. Whereas art learns to live with alternative modes of representation, here is Hertz valiantly trying to find uniquely the right one for mechanics. None of the traditional values – values still hallowed in 1983 – values of prediction, explanation, simplicity, fertility, and so forth, quite do the job. The trouble is, as Hertz says, that all three ways of representing mechanics do a pretty good job, one better at this, one better at that. What then is the truth about the motions of bodies? Hertz invites the next generation of positivists, including Pierre Duhem, to say that there is no truth of the matter – there are only better or worse systems of representation, and there might well be inconsistent but equally good images of mechanics.

Hertz was published in 1894, and Duhem in 1906. Within that

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span of years pretty well the whole of physics was turned upside down. Increasingly, people who knew no physics gossiped that everything is relative to your culture, but once again physicists were sure they were on the only path to truth. They had no doubt about the right representation of reality. We have only one measure of likeness: the hypothetico-deductive method. We propose hypo-theses, deduce consequences and see if they are true/ Hertz's warnings that there might be several representations of the same phenomena went unheeded. The logical positivists, the hypothetico-deductivists, Karl Popper's falsificationists – they were all deeply moved by the new science of 1905, and were scientific realists to a man, even when their philosophy ought to have made them somewhat anti-realist/ Only at a time when physics was rather quiescent would Kuhn cast the whole story in doubt. Science is not hypothetico-deductive. It does have hypotheses, it does make deductions, it does test conjectures, but none of these determine the movement of theory. There are – in the extremes of reading Kuhn – no criteria for saying which representation of reality is the best/ Representations get chosen by social pressures. What Hertz had held up as a possibility too scaring to discuss, Kuhn said was brute fact.

Anthropological summary

People represent. That is part of what it is to be a person/ In the beginning to represent was to make an object like something around us. Likeness was not problematic. Then different kinds of representation became possible. What was like, which real? Science and its philosophy had this problem from the very beginning, what with Democritus and his atoms. When science became the orthodoxy of the modern world we were able, for a while, to have the fantasy that there is one truth at which we aim/ That is the correct representation of the world. But the seeds of alternative representations were there. Hertz laid that out, even before the new wave of revolutionary science which introduced our own century. Kuhn took revolution as the basis for his own implied anti-realism. We should learn this: When there is a final truth of the matter – say, the truth that my typewriter is on the table – then what we say is either true or false. It is not a matter of representation. Wittgenstein's *Tractatus* is exactly wrong. Ordinary simple atomic sentences are not representations of anything. If Wittgenstein derived his picture account of meaning from Hertz he was wrong to do so. But Hertz was right about representation. In physics and much other interesting conversation we do make representations – pictures in words, if you like. In physics we do this by elaborate systems of modelling, structuring, theorizing, calculating, approximating. These are real, articulated, representations of how the world is. The representations of physics are entirely different from simple, non-representational assertions about the location of my typewriter/ There is a truth of the matter about the typewriter/ In physics there is no final truth of the matter, only a barrage of more or less instructive representations.

Here I have merely repeated at length one of the aphorisms of the turn-of-the-century Swiss-Italian ascetic, Danilo Domodosala: `When there is a final truth of the matter, then what we say is brief, and it is either true or false. It is not a matter of representation. When, as in physics, we provide representations of the world, there is no final truth of the matter.' Absence of final truth in physics should be the very opposite of disturbing. A correct picture of lively inquiry is given by Hegel, in his preface to the *Phenomenology of* Spirit: `The True is thus the Bacchanalian revel in which no member is not drunk; yet because each member collapses as he drops out, the revel is just as much transparent and simple repose.' Realism and anti-realism scurry about, trying to latch on to something in the nature of representation that will vanquish the other. There is nothing there. That is why I turn from representing to intervening.

Doing

In a spirit of cheerful irony, let me introduce the experimental part of this book by quoting the most theory-oriented philosopher of recent times, namely Karl Popper:

I suppose that the most central usage of the term `real' is its use to characterize material things of ordinary size — things which a baby can handle and (preferably) put into his mouth. From this, the usage of the term `real' is extended, first, to bigger things — things which are too big for us to handle, like railway trains, houses, mountains, the earth and the stars, and also to smaller things — things like dust particles or mites. It is further extended, of course, to liquids and then also to air, to gases and to molecules and atoms.

)at is the principle behind the extension? It is, I suggest, that the es which we conjecture to be real should be able to exert a causal effect

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the *prima facie* real things; that is, upon material things of an ordinary :hat we can explain changes in the ordinary material world of things by ausal effects of entities conjectured to be real/'

is Karl Popper's characterization of our usage of the word '. Note the traditional Lockeian fantasy beginnings. ` Real' is a ept we get from what we, as infants, could put in our mouths. is a charming picture, not free from nuance. Its absurdity Is that of my own preposterous story of reals and represenns. Yet Popper points in the right direction. Reality has to do causation and our notions of reality are formed from our ties to change the world.

aybe there are two quite distinct mythical origins of the idea of ity'/ One is the reality of representation, the other, the idea of affects us and what we can affect. Scientific realism is nonly discussed under the heading of representation. Let us discuss it under the heading of intervention. My conclusion is pus, even trifling. We shall count as real what we can use to vene in the world to affect something else, or what the world Ise to affect us. Reality as intervention does not even begin to I with reality as representation until modern science. Natural ice since the seventeenth century has been the adventure of the locking of representing and intervening. It is time that)sophy caught up to three centuries of our own past.

Popper and John Eccles, *The Self and its Brain,* **Berlin, New York and London,** 1977,

PART B INTERVENING *Experiment*

Philosophers of science constantly discuss theories and representation of reality, but say almost nothing about experiment, technology, or the use of knowledge to alter the world. This is odd, because `experimental method' used to be just another name for scientific method. The popular, ignorant, image of the scientist was someone in a white coat in a laboratory. Of course science preceded laboratories. Aristotelians downplayed experiment and favoured deduction from first principles. But the scientific revolution of the seventeenth century changed all that forever. Experiment was officially declared to be the royal road to knowledge, and the schoolmen were scorned because they argued from books instead of observing the world around them. The philosopher of this revolutionary time was Francis Bacon (1561-1626). He taught that not only must we observe nature in the raw, but that we must also `twist the lion's tail', that is, manipulate our world in order to learn its secrets.

The revolution in science brought with it new institutions. One of the first was the Royal Society of London, founded about 166o. It served as the model for other national academies in Paris, St Petersburg or Berlin. A new form of communication was invented: the scientific periodical. Yet the early pages of the *Philosophical Transactions of the Royal Society* have a curious air. Although this printed record of papers presented to the Society would always contain some mathematics and theorizing, it was also a chronicle of facts, observations, experiments, and deductions from experiments. Reports of sea monsters or the weather of the Hebrides rub shoulders with memorable work by men such as Robert Boyle or Robert Hooke. Nor would a Boyle or Hooke address the Society without a demonstration, before the assembled company, of some new apparatus or experimental phenomenon.

Times have changed. History of the natural sciences is now almost always written as a history of theory. Philosophy of science

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who also theorized, is almost forgotten, while Boyle, the theoretician who also experimented, is still mentioned in primary school text books.

Boyle had a speculative vision of the world as made up of little bouncy or spring-like balls. He was the spokesman for the corpuscular and mechanical philosophy, as it was then called. His important chemical experiments are less well remembered, while Hooke has the reputation of being a mere experimenter – whose theoretical insights are largely ignored. Hooke was the curator of experiments for the Royal Society, and a crusty old character who picked fights with people – partly because of his own lower status as an experimenter. Yet he certainly deserves a place in the pantheon of science. He built the apparatus with which Boyle experimentally investigated the expansion of air (Boyle's law). He discovered the laws of elasticity, which he put to work for example in making spiral springs for pocket watches (Hooke's law). His model of springs between atoms was taken over by Newton. He was the first to build a radical new reflecting telescope, with which he discovered major new stars. He realized that the planet Jupiter rotates on its axis, a novel idea. His microscopic work was of the highest rank, and to him we owe the very word `cell'. His work on microscopic fossils made him an early proponent of an evolutionary theory. He saw how to use a pendulum to measure the force of gravity. He co-discovered the diffraction of light (it bends around sharp corners, so that shadows are always blurred. More importantly it separates in shadows into bands of dark and light.) He used this as the basis for a wave theory of light. He stated an inverse square law of gravitation, arguably before Newton, although in less perfect a form. The list goes on. This man taught us much about the world in which we live. It is part of the bias for theory over experiment that he is by now unknown to all but a few specialists. It is also due to the fact that Boyle was noble while Hooke was poor and self-taught. The theory/experiment status difference is modelled on social rank.

Nor is such bias a thing of the past. My colleague C.W.F. Everitt wrote on two brothers for the *Dictionary of Scientific Biography.* Both made fundamental contributions to our understanding of superconductivity. Fritz London (1900–53) was a distinguished theoretical low-temperature physicist. Heinz London (1907–70) was a low-temperature experimentalist who also contributed to

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theory. They were a great team. The biography of Fritz was welcomed by the *Dictionary,* but that of Heinz was sent back for abridgement. The editor (in this case Kuhn) displayed the standard preference for hearing about theory rather than experiment.

Induction and deduction

What is scientific method? Is it the experimental method? The question is wrongly posed. Why should there be *the* method of science? There is not just one way to build a house, or even to grow tomatoes. We should not expect something as motley as the growth of knowledge to be strapped to one methodology.

Let us start with two methodologies. They appear to assign completely different roles to experiment. As examples I take two statements, each made by a great chemist of the last century. The division between them has not expired: it is precisely what separates Carnap and Popper. As I say in the Introduction, Carnap tried to develop a logic of induction, while Popper insists that there is no reasoning except deduction. Here is my own favourite statement of the inductive method:

> The foundations of chemical philosophy, are observation, experiment, and analogy. By observation, facts are distinctly and minutely impressed on the mind. By analogy, similar facts are connected. By experiment, new facts are discovered; and, in the progression of knowledge, observation, guided by analogy, leads to experiment, and analogy confirmed by experiment, becomes scientific truth.
>
> To give an instance. — Whoever will consider with attention the slender green vegetable filaments *(Conferva rivularis)* which in the summer exist in almost all streams, lakes, or pools, under the different circumstances of shade and sunshine, will discover globules of air upon the filaments that are shaded. He will find that the effect is owing to the presence of light. This is an *observation;* but it gives no information respecting the nature of the air. Let a wine glass filled with water be inverted over the

Conferva, the air will collect in the upper part of the glass, and when the glass is filled with air, it may be closed by the hand, placed in its usual position, and an inflamed taper introduced into it; the taper will burn with more brilliancy than in the atmosphere. This is an *experiment.* If the phenomena are reasoned upon, and the question is put, whether all vegetables of this kind, in fresh or in salt water, do not produce such air under like circumstances, the enquirer is guided by *analogy:* and when this is determined to be the case by new trials, *a general scientific truth* is established — That all Confervae in the sunshine produce a species of air that supports flame in a superior degree; which has been shown to be the case by various minute investigations.

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Those are the words with which Humphry Davy (1778–1829) starts his chemistry textbook, *Elements of Chemical Philosophy* (1812, pp. 2–3). He was one of the ablest chemists of his day, commonly remembered for his invention of the miner's safety lamp that prevented many a cruel death, but whose contribution to knowledge includes electrolytic chemical analysis, a technique that enabled him to determine which substances are elements (e.g. chlorine) while others are compounds. Not every chemist shared Davy's inductive view of science. Here are the words of Justus von Liebig (1803–73), the great pioneer of organic chemistry who indirectly revolutionized agriculture by pioneering artificial nitro-gen fertilizers.

In all investigations Bacon attaches a great deal of value to experiments. But he understands their meaning not at all. He thinks they are a sort of mechanism which once put in motion will bring about a result of their own. But in science all investigation is deductive or *a priori.* Experiment is only an aid to thought, like a calculation: the thought must always and necessarily precede it if it is to have any meaning. An empirical mode of research, in the usual sense of the term, does not exist. An experiment not preceded by theory, i.e. by an idea, bears the same relation to scientific research as a child's rattle does to music *(Uber Francis Bacon von Verulam and die Methode der Naturforschung, 1863, p. 49).*

How deep is the opposition between my two quotations? Liebig says an experiment must be preceded by a theory, that is, an idea. But this statement is ambiguous. It has a weak and a strong version. The *weak version* says only that you must have some ideas about nature and your apparatus before you conduct an experiment. A completely mindless tampering with nature, with no understanding or ability to interpret the result, would teach almost nothing. No one disputes this weak version. Davy certainly has an idea when he experiments on algae. He suspects that the bubbles of gas above the green filaments are of some specific kind. A first question to ask is whether the gas supports burning, or extinguishes it. He finds that the taper flares (from which he infers that the gas is unusually rich in oxygen?) Without that much understanding the experiment would not make sense. The flaring of the taper would at best be a meaningless observation. More likely, no one would even notice. Experiments without ideas like these are not experiments at all.

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There is however *a strong version* of Liebig's statement. It says that your experiment is significant only if you are testing a theory about the phenomena under scrutiny. Only if, for example, Davy had the view that the taper would go out (or that it would flare) is his experiment worth anything. I believe this to be simply false. One can conduct an experiment simply out of curiosity to see what will happen. Naturally many of our experiments are made with more specific conjectures in mind. Thus Davy asks whether all algae of the same kind, whether in fresh water or salt, produce gas of this kind, which he doubtless also guesses is oxygen. He makes new trials which lead him to a `general scientific truth'.

I am not here concerned with whether Davy is really making an inductive inference, as Carnap might have said, or whether he is in the end implicitly following Popper's methodology of conjecture and refutation. It is beside the point that Davy's own example is not, as he thought, a scientific truth. Our post-Davy reclassification of algae shows that *Confervae* are not even a natural kind! There is no such genus or species.

I am concerned solely with the question of the strong version: must there be a conjecture under test in order for an experiment to make sense? I think not. Indeed even the weak version is not beyond doubt. The physicist George Darwin used to say that every once in a while one should do a completely crazy experiment, like blowing the trumpet to the tulips every morning for a month. Probably nothing will happen, but if something did happen, that would be a stupendous discovery.

Which comes first, theory or experiment?

We should not underestimate the generation gap between Davy and Liebig. Maybe the relationship between chemical theory and chemical experiment had changed in the 50 years that separates the two quotations. When Davy wrote, the atomic theory of Dalton and others had only just been stated, and the use of hypothetical models of chemical structures was only just beginning. By the time of Liebig one could no longer practise chemistry by electrically decomposing compounds or identifying gases by seeing whether they support combustion. Only a mind fuelled by a theoretical model could begin to solve mysteries of organic chemicals.

We shall find that the relationships between theory and experi-

ment differ at different stages of development, nor do all the natural sciences go through the same cycles. So much may, on reflection, seem all too obvious, but it has been often enough denied, for example by Karl Popper. Naturally we shall expect Popper to be one of the most forthright of those who prefer theory over experiment. Here is what he does say in his *Logic of Scientific Discovery:*

> The theoretician puts certain definite questions to the experimenter, and the latter by his experiments tries to elicit a decisive answer to these questions, and to no others. All other questions he tries hard to exclude. . . . It is a mistake to suppose that the experimenter [. . . aims] `to lighten the task of the theoretician', or . . . to furnish the theoretician with a basis for inductive generalizations. On the contrary the theoretician must long before have done his work, or at least the most important part of his work: he must have formulated his questions as sharply as possible. Thus it is he who shows the experimenter the way. But even the experimenter is not in the main engaged in making exact observations; his work is largely of a Theoretical kind. Theory dominates the experimental work from its initial planning up to the finishing touches in the laboratory (p. 107).

'That was Popper's view in the 1934 edition of his book. In the much expanded 1959 edition he adds, in a footnote, that he should have also emphasized, `the view that observations, and even more so observation statements, and statements of experimental results, are always *interpretations* of the facts observed; that they are *interpretations in the light of theories'*. In a brief initial survey of different relations between theory and experiment, we would do well to start with the obvious counterexamples to Popper. Davy's noticing the bubble of air over the algae is one of these. It was not an ` interpretation in the light of theory' for Davy had initially no theory. Nor was seeing the taper flare an interpretation. Perhaps if he went on to say, 'Ah, then it is oxygen', he would have been making an interpretation. He did not do that.

Noteworthy observations (E)

Much of the early development of optics, between 1600 and 1800, depended on simply noticing some surprising phenomenon. Perhaps the most fruitful of all is the discovery of double refraction in Iceland Spar or calcite. Erasmus Bartholin (1625—98) examined some beautiful crystals brought back from

Iceland. If you were to place one of these crystals on this printed page, you would see the

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print double. Everybody knew about ordinary refraction, and by 1689, when Bartholin made his discovery, the laws of refraction were well known, and spectacles, the microscope and the telescope were familiar. This background makes Iceland Spar remarkable at two levels. Today one is still surprised and delighted by these crystals. Moreover there was a surprise to the physicist of the day, knowing the laws of refraction, who notes that in addition to the ordinary refracted ray there is an `extraordinary' one, as it is still called.

Iceland Spar plays a fundamental role in the history of optics, because it was the first known producer of polarized light. The phenomenon was understood in a very loose way by Huygens, who proposed that the extraordinary ray had an elliptical, rather than a spherical, wave surface. However our present understanding had to wait until the wave theory of light was revived. Fresnel (1788—1827), the founder of modern wave theory, gave a magnificent analysis in which the two rays are described by a single equation whose solution is a two-sheeted surface of the fourth degree. Polarization has turned out, time and again, to lead us ever deeper into the theoretical understanding of light.

There is a whole series of such `surprising' observations. Grimaldi (1613—63) and then Hooke carefully examined something of which we are all vaguely aware — that there is some illumination in the shadow of an opaque body. Careful observation revealed regularly spaced bands at the edge of the shadow. This is called diffraction, which originally meant `breaking into pieces' of the light in these bands. These observations preceded theory in a characteristic way. So too did Newton's observation of the dispersion of light, and the work by Hooke and Newton on the colours of thin plates. In due course this led to interference phenomena called Newton's rings. The first quantitative explanation of this phenomenon was not made until more than a century later, in 1802, by Thomas Young (1773—1829).

Now of course Bartholin, Grimaldi, Hooke and Newton were not mindless empiricists without an `idea' in their heads. They saw what they saw because they were curious, inquisitive, reflective people. They were attempting to form theories. But in all these cases it is clear that the observations preceded any formulation of theory.

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The stimulation of theory (E)

At a later epoch we find similar noteworthy observations that stimulate theory. For example in 18o8 polarization by reflection was discovered. A colonel in Napoleon's corps of engineers, E.L. Malus (1775—1812), was experimenting with Iceland Spar and noticed the effects of evening sunlight being reflected from the windows of the nearby Palais du Luxembourg. The light went through his crystal when it was held in a vertical plane, but was blocked when the crystal was held in a horizontal plane. Similarly, fluorescence was first noticed by John Herschel (1792—1871) in 1845, when he began to pay attention to the blue light emitted in a solution of quinine sulfate when it was illuminated in certain ways.

Noteworthy observation must, of its nature, be only the beginning. Might one not grant the point that there are initial observations that precede theory, yet contend that all deliberate experimentation is dominated by theory, just as Popper says? I think not. Consider David Brewster (1781—1868), a by now forgotten but once prolific experimenter. Brewster was the major figure in experimental optics between 1810 and 1840. He determined the laws of reflection and refraction for polarized light. He was able to induce birefringence (i.e. polarizing properties) in bodies under stress. He discovered biaxial double refraction and made the first and fundamental steps towards the complex- laws of metallic reflection. We now speak of Fresnel's laws, the sine and tangent laws for the intensity of reflected polarized light, but Brewster published them in 1818, five years before Fresnel's treatment of them within wave theory. Brewster's work established the material on which many developments in the wave theory were to be based. Yet in so far as he had any theoretical views, he was a dyed in the wool Newtonian, believing light consists of rays of corpuscles. Brewster was not testing or comparing theories at all. He was trying to find out how light behaves.

Brewster firmly held to the `wrong' theory while creating the experimental phenomena that we can understand only with the ' right' theory, the very theory that he vociferously rejected. He did not `interpret' his experimental findings in the light of his wrong theory. He made some phenomena for which any theory must, in the end, account. Nor is Brewster alone in this. A more recent

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brilliant experimenter was R.W. Wood (1868–1955) who between 1900 and 1930 made fundamental contributions to quantum optics, while remaining almost entirely innocent of, and sceptical about, quantum mechanics. Resonance radiation, fluorescence, absorption spectra, Raman spectra – all these require a quantum mechanical understanding, but Wood's contribution arose not from the theory but, like Brewster's, from a keen ability to get nature to behave in new ways.

Meaningless phenomena

I do not contend that noteworthy observations in themselves do anything. Plenty of phenomena attract great excitement but then have to lie fallow because no one can see what they mean, how they connect with anything else, or how they can be put to some use. In 1827 a botanist, Robert Brown, reported on the irregular movement of pollen suspended in water. This Brownian motion had been observed by others even 6o years before; some thought it was vital action of living pollen itself. Brown made painstaking observations, but for long it came to nothing. Only in the first decade of the present century did we have simultaneous work by experimenters, such as J. Perrin, and theoreticians, such as Einstein, which showed that the pollen was being bounced around by molecules. These results were what finally converted even the greatest sceptics to the kinetic theory of gases.

A similar story is to be told for the photoelectric effect. In 1839 A.-C. Becquerel noticed a very curious thing. He had a small electrovoltaic cell, that is, a pair of metal plates immersed in a dilute acid solution. Shining a light on one of the plates changed the voltage of the cell. This attracted great interest – for about two years. Other isolated phenomena were noticed. Thus the resistance of the metal selenium was decreased simply by illuminating it (1873). Once again it was left to Einstein to figure out what was happening; to this we owe the theory of the photon and innumerable familiar applications, including television (photoelectric cells convert the light reflected from an object into electric currents).

Thus I make no claim that experimental work could exist independently of theory. That would be the blind work of those whom Bacon mocked as `mere empirics'. It remains the case, however, that much truly fundamental research precedes any relevant theory whatsoever.

Happy meetings

Some profound experimental work is generated entirely by theory. Some great theories spring from pre-theoretical experiment. Some theories languish for lack of mesh with the real world, while some experimental phenomena sit idle for lack of theory. There are also happy families, in which theory and experiment coming from different directions meet. I shall give an example in which sheer dedication to an experimental freak led to a firm fact which suddenly meshed with theories coming from an entirely different quarter.

In the early days of transatlantic radio there was always a lot of static. Many sources of the noise could be identified, although they could not always be removed. Some came from electric storms. Even in the 1930s, Karl Jansky at the Bell Telephone Laboratories had located a ` hiss' coming from the centre of the Milky Way. Thus there were sources of radio energy in space which contributed to the familiar static.

In 1965 the radioastronomers Arno Penzias and R.W. Wilson adapted a radiotelescope to study this phenomenon. They expected to detect energy sources and that they did. But they were also very diligent. They found a small amount of energy which seemed to be everywhere in space, uniformly distributed. It would be as if everything in space which was not an energy source were about 4°K. Since this did not make much sense, they did their best to discover instrumental errors. For example, they thought that some of this radiation might come from the pigeons that were nesting on their telescope, and they had a dreadful time trying to get rid of the pigeons. But after they had eliminated every possible source of noise, they were left with a uniform temperature of 3°K. They were loth to publish because a completely homogeneous background radiation did not make much sense.

Fortunately, just as they had become certain of this meaningless phenomenon, a theoretical group, at Princeton, was circulating a preprint which suggested, in a qualitative way, that if the universe had originated in a Big Bang, there would be a uniform temperature throughout space, the residual temperature of the first explosion. Moreover this energy would be detected in the form of radio signals. The experimental work of Penzias and Wilson meshed beautifully with what would otherwise have been mere speculation.

Penzias and Wilson had showed that the temperature of the universe is almost everywhere about three degrees above absolute zero; this is the residual energy of creation. It was the first truly compelling reason to believe in that Big Bang.

It is sometimes said that in astronomy we do not experiment; we can only observe. It is true that we cannot interfere very much in the distant reaches of space, but the skills employed by Penzias and Wilson were identical to those used by laboratory experimenters. Shall we say with Popper, in the light of this story, that in general ` the theoretician must long before have done his work, or at least the most important part of his work: he must have formulated his questions as sharply as possible. Thus it is he who shows the experimenter the way'? Or shall we say that although some theory precedes some experiment, some experiment and some observation precedes theory, and may for long have a life of its own? The happy family I have just described is the intersection of theory and skilled observation. Penzias and Wilson are among the few experimenters in physics to have been given a Nobel Prize. They did not get it for refuting anything, but for exploring the universe.

Theory-history

It may seem that I have been overstating the way that theory-dominated history and philosophy of science skew our perception of experiment. In fact it is understated. For example, I have related the story of three degrees just as it is told by Penzias and Wilson themselves, in their autobiographical film *Three Degrees.'* They were exploring, and found the uniform background radiation prior to any theory of it. But here is what happens to this very experiment when it becomes `history':

Theoretical astronomers have predicted that if there had been an explosion billions of years ago, cooling would have been going on ever since the event. The amount of cooling would have reduced the original temperature of perhaps a billion degrees to 3°K — 3° above absolute zero.
Radioastronomers believed that if they could aim a very sensitive receiver at a blank part of the sky, a region that appeared to be empty, it might be possible to determine whether or not the theorists were correct. This was done in the early 197os. Two scientists at Bell Telephone Laboratories (the same place where Karl Jansky had discovered cosmic radio waves) picked up radio

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t Information and Publication Division, Bell Laboratories, 1979.

signals from `empty' space. After sorting out all known causes for the signals, there was still left a signal of 3° they could not account for. Since that first experiment others have been carried out. They always produce the same result — 3° radiation.
Space is not absolutely cold. The temperature of the universe appears to he 3°K. It is the exact temperature the universe should be if it all began some 13 billion years ago, with a Big Bang. [2]

We have seen another example of such rewriting of history in the case of the muon or meson, described in Chapter 6. Two groups of workers detected the muon on the basis of cloud chamber studies of cosmic rays, together with the Bethe–Heitler energy-loss formula. History now has it that they were actually looking for Yukawa's `meson', and mistakenly thought they had found it – when in fact they had never heard of Yukawa's conjecture. I do not mean to imply that a competent historian of science would get things so wrong, but rather to notice the constant drift of popular history and folklore.

Ampere, theoretician

Let it not be thought that, in a new science, experiment and observation precede theory, even if, later on, theory will precede observation. A.-M. Ampere (1775—1836) is a fine example of a great scientist starting out on a theoretical footing. He had primarily worked in chemistry, and produced complex models of atoms which he used to explain and develop experimental investigations. He was not especially successful at this, although he was one of those who, independently, about 1815, realized what we now call Avogadro's law, that equal volumes of gases at equal temperature and pressure will contain exactly the same number of molecules, regardless of the kind of gas. As we have already seen in Chapter 7 above, he much admired Kant, and insisted that theoretical science was a study of noumena behind the phenomena. We form theories about the things in themselves, the noumena, and are thereby able to explain the phenomena. That was not exactly what Kant intended, but no matter. Ampere was a theoretician whose moment came on September 11 1820. He saw a demonstration by Ørsted that a compass needle is deflected by an electric current. Commencing on September 20 Ampere laid out, in weekly lectures, the

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2 F.M. Bradley, *The Electromagnetic Spectrum*, New York, 1979, p. 100, my emphasis.

foundations of the theory of electromagnetism. He made it up as he went along.

That, at any rate, is the story. C.W.F. Everitt points out that there must be more to it than that, and that Ampere, having no official post-Kantian methodology of his own, wrote his work to fit. The great theoretician–experimenter of electromagnetism, James Clerk Maxwell, wrote a comparison of Ampere and Humphry Davy's pupil Michael Faraday, praising both ` inductivist ' Faraday and `deductivist' Ampere. He described Ampere's investigation as `one of the most brilliant achievements in science . . . perfect in form, unassailable in accuracy . . . summed up in a formula from which all the phenomena may be deduced', but then went on to say that whereas Faraday's papers candidly reveal the workings of his mind,

We can scarcely believe that Ampere really discovered the law of action: by means of the experiments which he describes. We are led to suspect what, indeed, he tells us himself, that he discovered the law by some process he has not shewn us, and that when he had afterwards built up a perfect demonstration he removed all traces of the scaffolding by which he had raised it.

Mary Hesse remarks, in her *Structure of Scientific Inference* (pp. 20If, **262),** that Maxwell called Ampere the Newton of electricity. This alludes to an alternative tradition about the nature of induction, which goes back to Newton. He spoke of deduction from phenomena, which was an inductive process. From the phenomena we infer propositions that describe them in a general way, and then are able, upon reflection, to create new phenomena hitherto unthought of. That, at any rate, was Ampere's procedure. He would usually begin one of his weekly lectures with a phenomenon, demonstrated before the audience. Often the experiment that created the phenomenon had not existed at the end of the lecture of the preceding week.

Invention (E)

A question posed in terms of theory and experiment is misleading because it treats theory as one rather uniform kind of thing and experiment as another. I turn to the varieties of theory in Chapter 12. We have seen some varieties in experiment, but there are also other relevant categories, of which invention is one of the most important. The history of thermodynamics is a history of practical

invention that gradually leads to theoretical analysis. One road to new technology is the elaboration of theory and experiment which is then applied to practical problems. But there is another road, in which the inventions proceed at their own practical pace and theory spins off on the side. The most obvious example is the best one: the steam engine.

There were three phases of invention and several experimental concepts. The inventions are Newcomen's atmospheric engine (1709-15), Watt's condensing engine (1767–84) and Trevithick's high-pressure engine (1798). Underlying half the developments after Newcomen's original invention was the concept, as much one of economics as of physics, of the `duty' of an engine, that is, the number of foot-pounds of water pumped per bushel of coal. Who had the idea is not known. Probably it was not anyone recorded in a history of science but rather the hard-headed value-for-money outlook of the Cornish mine-managers, who noticed that some engines pumped more efficiently than others and did not see why they should be short-changed when the neighbouring mine had a better rating. At first, the success of Newcomen's engine hung in the balance because, except in deep mines, it was only marginally cheaper to operate than horse-driven pumps. Watt's achievement, after seventeen years of trial and error, was to produce an engine guaranteed to have a duty at least four times better than the best Newcomen engine. (Imagine a marketable motor car with the same power as existing cars but capable of doing too miles per gallon instead of 25.)

Watt first introduced the separate condenser, then made the engine double-acting, that is, let in steam on one side of the cylinder while pulling a vacuum on the other, and finally in 1782 introduced the principle of expansive working, that is, cutting off the flow of steam into the cylinder early in its stroke, and allowing it to expand the rest of the way under its own pressure. Expansive working means some loss of power from an engine of a given size, but an increase in `duty'. Of these ideas, the most important for pure science was expansive working. A very useful practical aid, devised about 1790 by Watt's associate, James Southern, was the *indicator diagram*. The indicator was an automatic recorder which could be attached to the engine to plot pressure in the cylinder against the volume measured from the stroke: the area of the curve so traced was a measure of the work done in each stroke. The indicator was

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used to tune the engine to maximum performance. That very diagram became part of the Carnot cycle of theoretical thermodynamics.

Trevithick's great contribution, at first more a matter of courage than of theory, was to go ahead with building a high-pressure engine despite the danger of explosions. The first argument for high-pressure working is compactness: one can get more power from an engine of a given size. So Trevithick built the first successful locomotive engine in 1799. Soon another result emerged. If the high-pressure engine was worked expansively with early cut-off, its duty became higher (ultimately much higher) than the best Watt engine. It required the genius of Sadi Carnot (1796–1832) to come to grips with this phenomenon and see that the advantage of the high-pressure engine is not pressure alone, but the increase in the boiling point of water with pressure. The efficiency of the engine depends not on pressure differences but on the temperature difference between the steam entering the cylinder and the expanded steam leaving the cylinder. So was born the Carnot cycle, the concept of thermodynamic efficiency, and finally when Carnot's ideas had been unified with the principle of conservation of energy, the science of thermodynamics.

What indeed does `thermodynamics' mean? The subject deals not with the flow of heat, which might be called its dynamics, but with what might be called thermostatic phenomena. Is it misnamed? No. Kelvin coined the words `thermo-dynamic engine' in 185o to describe any machine like the steam engine or Carnot's ideal engine. These engines were called dynamic because they convert heat into work. Thus the very word `thermodynamics' recalls that this science arose from a profound analysis of a notable sequence of inventions. The development of that technology involved endless `experiment' but not in the sense of Popperian testing of theory nor of Davy-like induction. The experiments were the imaginative trials required for the perfection of the technology that lies at the centre of the industrial revolution.

A multitude of experimental laws, waiting for a theory **(E)**

The *Theory of the Properties of Metals and Alloys* (1936) is a standard old textbook whose distinguished authors, N.F. Mott and H. Jones, discuss, among other things, the conduction of electricity

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and heat in various metallic substances. What must a decent theory of this subject cover? Mott and Jones say that a theory of metallic conduction has to explain, among others, the following experimental results:

(1) The Wiedemann—Franz law which states that the ratio of the thermal to the electrical

conductivity is equal to *LT,* where *T* is the absolute temperature and *L* is a constant which is the same for all metals.

(2) The absolute magnitude of the electrical conductivity of a pure metal, and its dependence on the place of the metal in the periodic table, e.g., the large conductivities of the monovalent metals and the small conductivities of the transition metals.

(3) The relatively large increases in the resistance due to small amounts of impurities in solid solution, and the Matthiessen rule, which states that the change in resistance due to a small quantity of foreign metal in solid solution is independent of the temperature.

(4) The dependence of the resistance on temperature and on pressure.

(5) The appearance of supraconductivity [superconductivity].

Mott and Jones go on to say that `with the exception of *(5)* the theory of conductivity based on quantum mechanics has given at least a qualitative understanding of all these results' *(p. 27). (A* quantum mechanical understanding of superconductivity was eventually reached in *1957.)*

The experimental results in this list were established long before there was a theory around to fit them together. The Wiedemann—Franz law (1) dates from 1853, Matthiessen's rule from *1862 (3),* the relationships between conductivity and position in the periodic table from the 1890s (2), and superconductivity *(5)* from 1911. The data were all there; what was needed was a coordinating theory. The difference between this case and that of optics and thermodynamics is that the theory did not come directly out of the data, but from much more general insights into atomic structure. Quantum mechanics was both the stimulus and the solution. No one could sensibly suggest that the organization of the phenomenological laws within the general theory is a mere matter of induction, analogy or generalization. Theory has in the end been crucial to knowledge, to the growth of knowledge, and to its applications. Having said that, let us not pretend that the various phenomenological laws of solid state physics required a theory—any theory — before they were known. Experimentation has many lives of its own.

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Too many instances?

After this Baconian fluster of examples of many different relation-ships between experiment and theory, it may seem as if no statements of any generality are to be made. That is already an achievement, because, as the quotations from Davy and Liebig show, any one-sided view of experiment is certainly wrong. Let us now proceed to some positive ends. What is an observation? Do we see reality through a microscope? Are there crucial experiments? Why do people measure obsessively a few quantities whose value, at least to three places of decimals, is of no intrinsic interest to theory or technology? Is there something in the nature of experimentation that makes experimenters into scientific realists? Let us begin at the beginning. What is an observation? Is every observation in science loaded with theory?

10 Observation

Commonplace facts about observation have been distorted by two philosophical fashions. One is the vogue for what Quine calls semantic ascent (don't talk about things, talk about the way we talk about things). The other is the domination of experiment by theory. The former says not to think about observation, but about observation statements – the words used to report observations. The latter says that every observation statement is loaded with theory – there is no observing prior to theorizing. Hence it is well to begin with a few untheoretical unlinguistic platitudes.

1 Observation, as a primary source of data, has always been a part of natural science, but it is not all that important. Here I refer to the philosophers' conception of observation: the notion that the life of the experimenter is spent in the making of observations which provide the data that test theory, or upon which theory is built. This kind of observation plays a relatively minor role in most experiments. Some great experimenters have been poor observers. Often the experimental task, and the test of ingenuity or even greatness, is less to observe and report, than to get some bit of equipment to exhibit phenomena in a reliable way.

2 There is, however, a more important and less noticed kind of observation that is essential to fine experimentation. The good experimenter is often the observant one who sees the instructive quirks or unexpected outcomes of this or that bit of the equipment. You will not get the apparatus working unless you are observant. Sometimes persistent attention to an oddity that would have been dismissed by a lesser experimenter is precisely what leads to new knowledge. But this is less a matter of the philosophers' observation-as-reporting-what-one-sees, than the sense of the word we use when we call one person observant while another is not.

3 Noteworthy observations, such as those described in the previous chapter, have sometimes been essential to **initiating**

inquiry, but they seldom dominate later work. Experiment supersedes raw observation.

4 Observation is a skill. Some people are better at it than others. You can often improve this skill by training and practise.

5 There are numerous distinctions between observation and theory. The philosophical idea of a pure `observation statement' has been criticized on the ground that all statements are theory-loaded. This is the wrong ground for attack. There are plenty of pre-theoretical observation statements, but they seldom occur in the annals of science.

6 Although there is a concept of `seeing with the naked eye', scientists seldom restrict observation to that. We usually observe objects or events with instruments. The things that are `seen' in twentieth-century science can seldom be observed by the unaided human senses.

Observation has been over-rated

Much of the discussion about observation, observation statements and observability is due to our positivist heritage. Before positivism, observation is not central. Francis Bacon is our early philosopher of the inductive sciences. You might expect him to say a lot about observations. In fact he appears not even to use the word. Positivism had not yet struck.

The word `observation' was current in English when Bacon wrote, and applied chiefly to observations of the altitude of heavenly bodies, such as the sun. Hence from the very beginning, observation was associated with the use of instruments. Bacon uses a more general term of art, often translated by the curious phrase, *prerogative instances.* In 1620 he listed 27 different kinds of these. Included are what we now call crucial experiments, which he called crucial instances, or more correctly, instances of the crossroads *(instantiae crucis).* Some of Bacon's 27 kinds of instances are pre-theoretical noteworthy observations. Others are motivated by a desire to test theory. Some are made with devices that `aid the immediate actions of the senses'. These include not only the new microscopes and Galileo's telescope but also `rods, astrolabes and the like; which do not enlarge the sense of sight, but rectify and direct it'. Bacon moves on to `evoking' devices that `reduce the non-sensible to the sensible; that is, make manifest, things not

directly perceptible, by means of others which are'. *(Novum Organum* Secs. xxi–lii.)

Bacon thus knows the difference between what is directly perceptible and those invisible events which can only be `evoked'. The distinction is, for Bacon, both obvious and unimportant. There is some evidence that it really matters only after 1800, when the very conception of `seeing' undergoes something of a transformation. After 1800, to see is to see the opaque surface of things, and all knowledge must be derived from this avenue. This is the starting point for both positivism and phenomenology. Only the former concerns us here. To positivism we owe the need to distinguish sharply between inference and seeing with the naked eye (or other unaided senses).

Positivist observation

The positivist, we recall, is against causes, against explanations, against theoretical entities and against metaphysics. The real is restricted to the observable. With a firm grip on observable reality the positivist can do what he wants with the rest.

What he wants for the rest varies from case to case. The logical positivists liked the idea of using logic to `reduce' theoretical statements, so that theory becomes a logical short-hand for expressing facts and organizing thoughts about what can be observed. On one version this would lead to a wishy-washy scientific realism: theories may be true, and the entities that they mention may exist, so long as none of that talk is understood too literally.

In another version of logical reduction, the terms referring to theoretical entities would be shown, on an analysis, not to have the logical structure of referring terms at all. Since they are not referential, they don't refer to anything, and theoretical entities are not real. This use of reduction leads to a fairly stringent anti-realism. But since nobody has made a logical reduction of any interesting natural science, such questions are vacuous.

The positivist then takes another tack. He may say with Comte or van Fraassen that theoretical statements are to be understood literally, but not to be believed. As the latter puts it, in *The Scientific Image,* `When a scientist advances a new theory, the realist sees him as asserting the (truth of the) postulate. But the anti-realist sees him

as displaying this theory, holding it up to view, as it were, and claiming certain virtues for it' (p. 27). A theory may be accepted because it accounts for phenomena and helps in prediction. It may be accepted for its pragmatic virtues without being believed to be literally true.

Positivists such as Comte, Mach, Carnap or van Fraassen insist in these various ways that there is a distinction between theory and observation. That is how they make the world safe from the ravages of metaphysics.

Denying the distinction

Once the distinction between observation and theory was made so important, it was certain to be denied. There are two grounds of denial. One is conservative, and realist in its tendencies. The other is radical, more romantic, and often leans towards idealism. There was an outburst of both kinds of response around 196o.

Grover Maxwell exemplifies the realist response. In a 1962 paper he says that the contrast between being observable and merely theoretical is vague. It often depends more on technology than on anything in the constitution of the world.' Nor, he continues, is the distinction of much importance to natural science. We cannot use it to argue that no theoretical entities really exist.

In particular Maxwell says that there is a continuum that starts with seeing through a vacuum. Next comes seeing through the atmosphere, then seeing through a light microscope. At present this continuum may end with seeing using a scanning electron micro-scope. Objects like genes which were once merely theoretical are transformed into observable entities. We now see large molecules. Hence observability does not provide a good way to sort the objects of science into real and unreal.

Maxwell's case is not closed. We should attend more closely to the very technologies that he takes for granted. I attempt this in the next chapter, on microscopes. I agree with Maxwell's playing down of visibility as a basis for ontology. In a paper I discuss later in this chapter, Dudley Shapere makes the further point that physicists regularly talk about observing and even seeing using devices in which neither the eye nor any other sense organ could play any

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1 G. Maxwell, `The ontological status of theoretical entities', *Minnesota Studies in the Philosophy of Science 3 (1962),* pp. 3-27.

essential role at all. In his example, we try to observe the interior of the sun using neutrinos emitted by solar fusion processes. What counts as an observation, he says, itself depends upon current theory. I shall return to this theme, but first we should look at the more daring and idealist-leaning rejection of the distinction between theory and observation. Maxwell said that the observability of *entities* has nothing to do with their ontological status. Other philosophers, at the same time, were saying that there are no purely observation *statements* because they are all infected by theory. I call this idealist-leaning because it makes the very content of the feeblest scientific utterances determined by how we think, rather than mind-independent reality. We can diagram these differences in the following way:

Conservative response (realistic): there is no significant distinction between observable and unobservable entities.

Positivism: (a sharp distinction between theory and observation)

Radical response (idealistic): all observation statements are theory-loaded.

Theory-loaded

In 1959 N.R. Hanson gave us the catchword `theory-loaded' in his splendid book, *Patterns of Discovery*. The idea is that every observational term and sentence is supposed to carry a load of theory with it.

One fact about language tends to dominate those parts of *Patterns of Discovery* in which the word `theory-loaded' occurs. We are reminded that there are very subtle linguistic rules about even the most commonplace words, for example the verb `to wound' and the noun `wound'. Only some cuts, injuries, etc., in quite specific kinds of situations, count as wounds. If a surgeon describes a gash in a man's leg as a wound, that may imply that the man was hurt in a fight or in battle. Such implications occur all the time, but they are not in my opinion worth calling theoretical assumptions. This part

of the theory loaded doctrine is an important and unexceptionable assertion about ordinary language. It in no way implies that all reports of observation must carry a load of scientific theory.

Hanson also points out that we tend to notice things only when we have expectations, often of a theoretical sort, which will make them seem interesting or at least to make sense. That is true but it is different from the theory-loaded doctrine. I shall turn to it presently. First, I address some more dubious claims.

Lakatos on observation

Lakatos, for example, says that the simplest kind of falsificationism – the kind we often attribute to Popper – won't do because it takes for granted a theory/observation distinction. We cannot have the simple rule about theories, that people propose them and nature disposes of them. That, says Lakatos, rests on two false assumptions. First, that there is a psychological borderline between speculative propositions and observational ones, and, secondly, that observational propositions can be proved by (looking at) the facts. For the past 15 years these assumptions have been jeered at, but we ought also to have argument. Lakatos's arguments are dismayingly facile and ineffective. He says that a ` few characteristic examples already undermine the first assumption'. In fact he gives one example, of Galileo using a telescope to see sun-spots, a seeing which cannot be purely observational. Is that supposed to refute, or even undermine, the theory/observation distinction?

As for the second point, that one can look and see whether observation sentences are true, Lakatos writes in italics, `no factual proposition can ever be proved from an experiment . . . one cannot prove statements from experience. . . . This is one of the basic points of elementary logic, but one which is understood by relatively few people even today' (I, p. 16). Such an equivocation on the verb `prove' is particularly disheartening from a writer from whom I learned the several senses of the verb: that the verb properly bears the sense of `test' (the proof of the pudding is in the eating, galley proofs), and that such tests often lead to establishing facts (the pudding is stodgy, the galleys full of misprints).

On containing theoretical assumptions

Paul Feyerabend's essays, contemporary with work by Hanson, also played down the distinction between theory and observation.

I le has since come to dismiss the philosophical obsession with language and meanings. He has denounced the very phrase, 'theory-laden'. But this is not because he thinks that some of what we say is free from theory. Quite the contrary. To call statements theory-laden, he says, is to suggest that there is a sort of observational truck on to which a theoretical component is loaded. There is no such truck. Theory is everywhere.

In his most famous book, *Against Method* (1977), Feyerabend says that there is no point to the distinction between theory and observation. Curiously, for all his avowed rejection of linguistic discussions, he still speaks as if the theory/observation distinction were a distinction between sentences. He suggests it is just a matter of obvious and less obvious sentences, or between long ones and short ones. 'Nobody will deny that such distinctions *can be made*, but nobody will put great weight on them, for they do not play any decisive role in the business of science.' (p. 168). We also read what sounds like the 'theory-loaded' doctrine in full force: 'observational reports, experimental results, "factual statements", either *contain* theoretical assumptions or assert them by the manner in which they are used.' (p. 31). I disagree with what is actually said here, but before explaining why, I want to cancel something suggested by remarks like this. They give the idea that experimental results exhaust what matters to an experiment, and that experimental results are stated by, or even constituted by, an observation report or a 'factual statement'. I shall insist on the truism that experimenting is not stating or reporting but doing – and not doing things with words.

Statements, records, results

Observation and experiment are not one thing nor even opposite poles of a smooth continuum. Evidently many observations of interest have nothing to do with experiments. Claude Bernard's 1865 *Introduction to the Study of Experimental Medicine* is the classic attempt to distinguish the concepts of experiment and observation. He tests his classification by a lot of difficult examples from medicine where observation and experiment get muddled up. Consider Dr Beauchamp who, in the Anglo-American war of 1812, had the good fortune to observe, over an extended period of time, the workings of the digestive tract of a man with a dreadful stomach wound. Was that an experiment or just a sequence of fateful
observations in almost unique circumstances? I do not want to pursue such points, but instead to emphasize something that is more noticeable in physics than medicine.

The Michelson–Morley experiment has the merit of being well known. It is famous because with hindsight it seemed to some historians to refute the entire theory of the electromagnetic aether, and thus to be the experimental forerunner of Einstein's theory of relativity. The chief published *report* of the experiment of 1887 is 12 pages long. The *observations* were made in the course of a couple of hours on July 8, 9, I I, and 12. The *results* of the experiment are notoriously controversial; Michelson thought the chief result was a refutation of the earth's motion relative to the aether. As I show in Chapter 15 below, he also thought that it discredited a theory used to explain why the stars are not quite where they appear to be. At any rate the experiment lasted over a year. This included making and remaking the apparatus and getting it to work, and above all acquiring the curious knack of knowing when the apparatus is working. It has been common practice to use the label `the Michelson–Morley experiment' to denote a sequence of intermit-tent work with Michelson's initial success of 1881 (or even earlier, some failures) and going on to include Miller's work of the 1920S. One could say that the experiment lasted half a century, while the observations lasted maybe a day and a half. Moreover the chief result of the experiment, although not an experimental result, was a radical transformation in the possibilities of measurement. Michel-son won a Nobel prize for this, not for his impact on aether theories.

In short Feyerabend's `factual statements, observation reports, and experimental results' are not even the same kinds of thing. To lump them together is to make it almost impossible to notice anything about what goes on in experimental science. In particular they have nothing to do with Feyerabend's difference between long and short sentences.

Observation without theory

Feyerabend says that observational reports, etc., always contain or assert theoretical assumptions.

This assertion is hardly worth debating because it is obviously false, unless one attaches a quite attenuated sense to the words, in which case the assertion is true but trivial.

Most of the verbal quibble arises over the word `theory', a word best reserved for some fairly specific body of speculation or propositions with a definite subject matter. Unfortunately the Feyerabend of my quotation used the word `theory' to denote all sorts of inchoate, implicit, or imputed beliefs. To condense him without malice, he wrote of some alleged habits and beliefs:

> Our habit of saying the table is brown when we view it under normal circumstances, or saying the table seems brown when viewed under other circumstances . . . our belief that some of our sensory impressions are veridical and some are not . . . that the medium between us and the object does not distort . . . that the physical entity that establishes the contact carries a true picture... .

All these are supposed to be theoretical assumptions underlying our commonplace observations, and `the material which the scientist has at his disposal, his most sublime theories and his most sophisticated techniques included, is structured in exactly the same way'.

Now taken literally most of this is, to be polite, rather hastily said. For example, what is this `habit of saying the table is brown when we view it under normal circumstances'? I doubt that ever in my life, before, have I uttered either the sentence `the table is brown' or the `table seems to be brown'. I am certainly not in the habit of uttering the first sentence when looking at a table in a good light. I have only met one person with any such habit, a French lunatic who habitually and repeatedly uttered, *C'est de la merde, ca* whenever he saw excrement in normal viewing conditions, for example, when we were manuring a field. Nor would I impute to poor Boul-boul any of the assumptions listed by Feyerabend. Feyerabend has shown us how not to talk about observation, speech, theory, habits, or reporting.

Of course we have all sorts of expectations, prejudices, opinions, working hypotheses and habits when we say anything. Some of these we express. Some are contextual implications. Some can be imputed to the speaker by a sensitive student of the human mind. Some propositions which could be assumptions or presuppositions in another context are not so in the context of routine existence. Thus I could make the assumption that the air between me and the printed page does not distort the shapes of the words I see, and I

could *pernaps i*nvestigate tors assumption. (How?) But when I read aloud, or make corrections on this page I simply interact with something of interest to me, and it is wrong to speak of assumptions. In particular it is wrong to speak of theoretical assumptions. I have not the remotest idea what a theory of non-distortion by the air would be like. Of course if you want to call every belief, protobelief, and belief that could be invented, a theory, do so. But then the claim about theory-loaded is trifling.

There have been important observations in the history of science, which have included no theoretical assumptions at all. The noteworthy observations of the previous chapter furnish examples. Here is another, of more recent date, where we can set down a pristine observation statement.

Herschel and radiant heat

William Herschel was an adroit and insatiable searcher of the midnight sky, builder of the greatest telescope of his time and immensely extending our catalogue of the heavens. Here I consider an incidental event of 1800, when Herschel was 61. That was the year in which, as we now put it, he discovered radiant heat. He made about 200 experiments and published four major papers on the topic, of which the last is 100 pages long. All are to be found in the *Philosophical Transactions of the Royal Society* for 1800. He began by making what we now think of as the right proposal about radiant heat, but ended up in a quandary, not sure where the truth might lie.

He had been using coloured filters in one of his telescopes. He noticed that filters of different colours transmit different amounts of heat: `When I used some of them I felt a sensation of heat, though I had but little light, while others gave me much light with scarce any sensation of heat.' We shall not find a better sense-datum report than this, in the whole of physical science. Naturally we remember it not for its sensory quality but because of what came next. Why did Herschel do anything next? First of all he wanted filters better suited for looking at the sun. Certainly he also had his mind on certain speculative issues that were then coming to the fore.

He used thermometers to study the heating effect of rays of light separated with a prism. This really set him going, for he found not only that orange warms more than indigo, but that there is also a heating effect below the visible red spectrum. His first guess about this phenomenon was roughly what we now believe. He took it that

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both visible and invisible rays are emitted from the sun. Our eyes are sensitive to only one part of the spectrum of radiation. We are warmed by a different overlapping part. Since he believed in the Newtonian corpuscular theory of light, he thought in terms of rays composed of particles. Sight responds to corpuscles of violet through red, while the sense of heat responds to corpuscles of yellow through infra-red.

He now set out to investigate this idea by seeing whether heat and light rays in the visible spectrum have the same properties. So he compared their reflection, refraction and differential refrangibility, their tendency to be stopped by diaphanous bodies, and their liability to scattering from rough surfaces.

At this stage in Herschel's papers we have a large number of observations of various angles, proportions of light transmitted and so forth. He certainly has an experimental idea, but only one of a rather nebulous sort. His theory was entirely Newtonian: he thought that light consisted of rays of particles, but this had limited impact upon the details of his research. His difficulties were not theoretical but experimental. Photometry – the practice of measuring aspects of transmitted light – had been in fair state for 40 years, but calorimetry was almost nonexistent. There were procedures for filtering out rays of light, but how should one filter rays of heat? Herschel was probing phenomena. He made many claims to accuracy which we now think to be misplaced. He measured not only transmission of light but also transmission of heat to one part in a thousand. He could not have done that! But we have a special problem, if we want to repeat what he might have done, for Herschel worked with a wide range of filters to hand – such as brandy in a decanter, for example. As one historian has noticed, his brandy was almost pitch black. We cannot repeat a measurement on that substance, whatever it was, today.

Herschel showed that heat and light are alike in reflection, refraction and differential refrangibility. He became troubled by transmission. He had the picture of a translucent medium stopping a definite proportion of the rays of a certain character, for example, red. His idea about red was that the heat ray, which refracts with the coefficient of red light, is identical to the red light with the same coefficient. So if x% of the light gets through, and heat and light are identical in this part of the spectrum, x% of the heat should go through t00. He asks, ` Is the heat, which has the refrangibility of

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the red rays, occasioned by the light of those rays?' He finds not. A certain piece of glass that transmits nearly all the red light impedes 96.2% of the heat. Hence heat cannot be the same as light.

Herschel abandoned his original hypothesis and did not quite know what to think. Thus by the end of 1800, after 200 experiments and four major publications, he gave up. The very next year Thomas

Young, whose work on interference commenced (or recreated) the wave theory of light, gave the Bakerian lecture in which he favoured Herschel's original hypothesis. Thus he was rather indifferent to Herschel's experimental dilemma. Perhaps the wave theory was more hospitable to radiant heat than was the Newtonian theory of rays of light particles. But in fact scepticism about radiant heat lasted long after Newtonian theory had gone into decline. It was resolved only by equipment invented by Macedonio Melloni (1798-1854). As s00n as the thermocouple had been invented (1830) Melloni realized that he now had an instrument with which to measure the transmission of heat by different substances. This provides one of the innumerable examples in which an invention enables an experimenter to undertake another inquiry which in turn makes clear the route which the theoretician must follow.

Herschel had more primitive experimental problems. What was he observing? That was the question asked by his critics. He was rather viciously challenged in 1801. The experimental results were denied. A year later they were reproduced, more or less. There were many hard and simple experimental difficulties. For example, a prism does not neatly end at red. Some ambient light is diffused and comes below red as pale white light. So might not the `infra-red' heat be caused by this white light? A new experimental idea intervened here. There is no significant invisible heat above purple, but might there not still be `radiation'? It was known that silver chloride reacts when exposed at the purple end of the spectrum. (This is the beginning of photography.) Ritter exposed it beyond the violet and obtained a reaction; we now say that he discovered the ultraviolet in 1802.

On noticing

Herschel noticed the phenomenon of a differential heating by coloured light and reported this in as pure a sense-datum statement

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as we shall ever find in physics. I do not mean to discount the facts urged by N.R. Hanson, that one may see or notice a phenomenon only if one has a theory that makes sense of it. In Herschel's case it was lack of theory that made him sit up and take notice. Often we find the reverse. Hanson's book *The Positron* (1965), although containing some controversial accounts of discovery, is a sustained illustration of this thesis. He claims that people could see the tracks of positrons only when there was a theory, although after the theory, any undergraduate can see the selfsame tracks. We might call this the doctrine that noticing is theory-loaded.

Undoubtedly people tend to notice things that are interesting, surprising, and so forth, and such expectations and interests are influenced by theories they may hold – not that we should play down the possibility of the gifted `pure' observer either. But there is a tendency to infer from stories like that of the positron, that anyone who reports, on looking at a photographic plate, `that's a positron', is thereby implying or asserting a lot of theory. I do not think that this is so. An assistant can be trained to recognize those tracks without having a clue about the theory. In England it is still not t00 uncommon to find in a lab a youngish technician, with no formal education past 16 or 17, who is not only extraordinarily skilful with the apparatus, but also quickest at noting an oddity on for example the photographic plates he has prepared from the electron microscope.

But, it may be asked, is not the substance of the theory about positrons among the truth conditions or truth presuppositions for the type of utterance that we may represent by `that's a positron'? Possibly, but I doubt it. The theory might be abandoned or superseded by a totally different theory about positrons, leaving intact what had, by then, become the class of observation sentences represented by `that's a positron'. Of course the present theory might be wrecked in quite a different way, in which it turns out that so-called positron tracks are artifacts of the experimental device. That is only slightly more likely than the possibility that we shall discover that all sheep are only wolves in woolly suits. We would talk differently in that event too! I am not claiming that the sense of `that's a

positron' is any more unconnected to the rest of the discourse than `that's a sheep'. I claim only that its sense need not be necessarily entangled in some particular theory, so that every time you say `that's a positron' you somehow assert the theory.
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Observation is a skill

An example similar to Hanson's makes the point that noticing and observation are skills. I think that Caroline Herschel (sister of William) discovered more comets than any other person in history. She got eight in a single year. Several things helped her do this. She was indefatigable. Every moment of cloudless night she was at her station. She also had a clever astronomer for a brother. She used a device, reconstructed only in 198o by Michael Hoskin, that enabled her, each night, to scan the entire sky, slice by slice, never skimping on any corner of the heavens.[2] When she did find something curious `with the naked eye', she had good telescopes to look more closely. But most important of all, she could recognize a comet at once. Everyone except possibly brother William had to follow the path of the suspected comet before reaching any opinion on its nature. (Comets have parabolic trajectories.)

In saying that Caroline Herschel could tell a comet just by looking, I do not mean to say that she was some mindless automaton. Quite the contrary. She had one of the deepest understandings of cosmology and one of the most profound speculative minds of her time. She was indefatigable not because she specially liked the boring task of sweeping the heavens, but because she wanted to know more about the universe.

It might well have turned out that Herschel's theory about comets was radically wrong. It might by now have been replaced by an account so different that some would call it incommensurable with hers. Yet this need not call in question her claim to fame. It would still be true that she discovered more comets than anyone else. Indeed if our new theory made comets into mere nothings, optical illusion on a cosmic scale, then her discovery of eight comets in a single year might bring more a smile of condescension than a gasp of admiration, but that is something else.

Seeing is not saying

The drive to displace observations by linguistic entities (observation sentences), persists throughout recent philosophy. Thus W.V.O. Quine proposes, almost as if it were a novelty, that we

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2 M. Hoskin and B. Warner, `Caroline Herschel's comet *sweepers'. journal for the History of Astronomy* 12 (1981), PP. 27-34.

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should `drop the talk of observation and talk instead of observation sentences. sentences, the sentences that are said to report observations'. *(The Roots of Reference,* pp. *36-9.)*

Caroline Herschel not only serves to rebut the claim that observation is just a matter of saying something, but also leads us to call in question the grounds for Quine's assertion. Quine was quite deliberately writing against the doctrine that all observations are theory-loaded. There is, he says, a perfectly distinguishable class of observation sentences, because `observations are what witnesses will agree about, on the spot'. He assures us that a `sentence is observational insofar as its truth value, on any occasion, would be agreed to by just about any member of the speech community witnessing the occasion'. And `we can recognize membership in the speech community by mere fluency of dialogue'.

It is hard to imagine a more wrong-headed approach to observation in natural science. No one in Caroline Herschel's speech community would in general agree or disagree with her about a newly spotted comet, on the basis of one night's observation. Only she, and to a lesser extent William, had the requisite skill. This does not mean that we would say she had the skill unless other students, using other means, did not in the end come to agree on many of her identifications. Her judgements attain full validity only in the context of the rich scientific life of the period. But Quine's agreement `on the spot' has little to do with observation in science.

If we want a comprehensive account of scientific life, we should, in exact opposition to Quine, drop the talk of observation sentences and speak instead of observation. We should talk carefully of reports, skills, and experimental results. We should consider what, for example, it is to have an experiment working well enough that the skilful experimenter knows that the data it provides may have some significance. What is it that makes an experiment convincing? Observation has precious little to do with that question.

Augmenting the senses

The unaided eye does not see very far or deep. Some of us need spectacles to avoid being practically blind. One way in which to extend the senses is by the use of ever more imaginative telescopes and microscopes. In the next chapter I discuss whether we see with a microscope (I think we do, but the issue is not simple). There are
more radical extensions of the idea of observation. It is commonplace in the most rarefied reaches of experimental science to speak of `observing' what we would naively suppose to be unobservable – if' observable' really did mean, using the five senses almost unaided. Naturally if we were pre-positivist, like Bacon, we would say, `so what?' But we still have a positivist legacy, and so we are a little startled by routine remarks by physicists. For example, the fermions are those fundamental particles with angular momentum such as 1/2, or 3/2, and which obey Fermi–Dirac statistics: they include electrons, nuons, neutrons, and protons, and much else, including the notorious quarks. One says things like: `Of these fermions, only the *t* quark is yet unseen. The failure to observe *tt′* states in e^+e^--annihilation at PETRA remains a puzzle.[3]

The language which has been institutionalized among particle physicists may be seen by glancing at something as formal as a table of mesons. At the head of the April 1982 Meson Table one reads that `quantities in italics are new or have been changed by more than one (old) standard deviation since April 1980.4 It is not clear even how to count the kinds of mesons which are now recorded, but let us limit ourselves to one open page (pp. 28–9) with nine mesons classified according to six different characteristics. Of interest is the ` partial decay mode' and the fraction of decays which are quantitatively recorded only when one has a statistical analysis at the 90% confidence level. Of the 31 decays associated with these nine mesons, we have 11 quantities or upper bounds, one entry `large', one entry `dominant', one entry *`dominant',* eight entries `seen', six entries *`seen',* and three `possibly seen'. Dudley Shapere has recently attempted a detailed analysis of such discourse[5] He takes his example from talk of observing the interior of the sun, or another star, by collecting neutrinos in large quantities of cleaning fluid, and deducing various properties of the inside of the sun. Clearly this involves several layers, undreamt of by Bacon, of Bacon's idea of `making manifest, things not directly perceptible, by means of others which are'. The trouble is that the physicist still calls this

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3 C.Y. Prescott, `Prospects for polarized electrons at high energies', Stanford Linear Accelerator, *SLAG-PUB-263o,* October 1980, p. 5. (This is a report connected with the experiment described in Chapter 16 below.)

4 *Particle Properties Data Booklet,* April 1982, p. 24. (Available from Lawrence Berkeley Laboratory and CERN. Cf. `Review of physical properties', *Physics Letters* 111B (1982).)

5 D. Shapere, `The concept of observation in science and philosophy', *Philosophy of **Science 49*** (1982), pp. 231-67.

'direct observation'. Shapere has many quotations like these: 'There is no way known other than by neutrinos to see into a stellar interior.' 'Neutrinos,' writes another author, `present the only way of directly observing' the hot stellar core.

Shapere concludes that this usage is apt and analyses it as follows: 'x is directly observed if (1) information is received by an appropriate receptor and (2) that information is transmitted directly, i.e. without interference, to the receptor from the entity x (which is the source of the information.)' I suspect that the usage of some physicists – illustrated by my quark quotation above – is even more liberal than this, but clearly Shapere gives the beginnings of a correct analysis.'

Shapere notes that whether or not something is directly observable depends upon the current state of knowledge. Our theories of the workings of receptors, or of the transmission of information by neutrinos, all assume massive amounts of theory. So we might think t hat, as theory becomes taken for granted, we extend the realm of what we call observation. Yet we must never fall prey to the fallacy of talking about theory without making distinctions.

For example, there is an excellent reason for speaking of observation in connection with neutrinos and the sun. The theory of the neutrino and its interactions is almost completely in-dependent of speculations about the core of the sun. It is precisely the disunity of science that allows us to observe (deploying one massive batch of theoretical assumptions) another aspect of nature (about which we have an unconnected bunch of ideas). Of course whether or not the two domains are connected itself involves, not exactly theory, but a hunch about the nature of nature. A slightly different example about the sun will illustrate this.

How might we investigate Dicke's hypothesis that the interior of the sun is rotating to times faster than its surface? Three methods have been proposed: (I) use optical observations of the oblateness of the sun; (2) try to measure the sun's quadruple mass-moment with t he near fly-by of Starprobe, the satellite that goes within four solar radiuses of the sun; (3) measure the relativistic precession of a

((footnote:))

6 See K.S. Shrader Frechette, 'Quark quantum numbers and the problem of microphysical observation', *Synthese 50* (1982), pp. 125-46.

gyroscope in orbit about the sun. Do any of these three enable us to `observe' interior rotation?

The first method assumes that optical shape is related to mass shape. A certain shape of the sun may help us infer something about internal rotation, but it is an inference based on an uncertain hypothesis which is itself connected with the subject matter under study.

The second method assumes that the only source of quadruple mass-moment is interior rotation, whereas it could be attributable to internal magnetic fields. Thus an assumption about what is going on (or not going on) in the sun itself is necessary for us to draw an inference about interior rotation.

On the other hand, relativistic precession of the gyroscope is based upon theory having nothing to do with the sun, and within the framework of present theory, one cannot conceive of anything except angular momentum of an object (e.g. the sun) that could produce such and such relativistic precession of a polar-orbiting gyro about the sun.

The point is not that the relativistic theory is better established than the theories involved in the other two possible experiments. Maybe relativistic precession theory will be the first to be abandoned. The point is that within the framework of our present understanding, the body of theoretical assumptions underlying the gyro proposal are arrived at in a completely different way from the propositions that people invent about the core of the sun. On the other hand, the first two proposals involve assumptions which in themselves concern beliefs about the sun's interior.

It is thus natural for the experimenter to say that the polar-orbiting gyro gives us a way to observe the interior rotation of the sun, while the other two investigations would only suggest inferences. This is not even to say that the third experiment would be the best one – its sheer cost and difficulty make the first two more attractive. I am making only a philosophical point about which experiments lead to observation, and which do not.

Possibly this connects with the debates about theory-loaded observation with which I began this chapter. Maybe the first two experiments contain theoretical assumptions connected with the subject under investigation, while the third, though loaded with theory, contains no such assumptions. In the case of seeing tables, our statements similarly contain no theoretical assumptions connected with the objects under inquiry, namely tables, even if (by an abuse of the words `theory' and `contain') they contain theoretical assumptions about vision.

Independence

On this view, something counts as observing rather than inferring when it satisfies Shapere's minimal criteria, and when the bundle of heories upon which it relies are not intertwined with the facts about t he subject matter under investigation. The following chapter, on microscopes, confirms the force of this suggestion. I do not think that the issue is of much importance. Observation, in the philosophers' sense of producing and recording data, is only one aspect to experimental work. It is in another sense that the experimenter must be observant – sensitive and alert. Only the observant can make an experiment go, detecting the problems that are making it foul up, debugging it, noticing if something unusual is a clue to nature or an artifact of the machine. Such observation seldom appears in the finished reports of the experiment. It is at least as important as anything that does go into final write-ups, but nothing philosophical hangs on that.

Shapere had a more philosophical purpose in his analysis of observing. He holds that the old foundationalist view of knowledge was on the right track. Knowledge is in the end founded upon observation. He notes that what counts as observations depends upon our theories of the world and of special effects, so that there is no such thing as an absolute basic or observational sentence. But the fact that observing depends upon theories has none of the anti-rational consequences that have sometimes been inferred from the thesis that all observation is theory-loaded. Thus although Shapere has written the best extended study of observation in recent times, in the end he has an axe to grind, concerning the foundations for, and rationality of, theoretical belief. Van Fraassen also notes, in passing, that theory may delimit the bounds of observation. His purposes are different again. The real, for him, is observational, but he grants that theory itself can modify our beliefs about what is observational, and what is real. My purposes in this chapter have been more mundane. I have wanted to insist on some of the more humdrum aspects of observation. A philosophy of experimental science cannot allow theory-dominated philosophy to make the very concept of observation become suspect.

11 Microscopes

One fact about medium-size theoretical entities is so compelling **an** argument for medium-size scientific realism that philosophers blush to discuss it: Microscopes. First we guess there is such and such a gene, say, and then we develop instruments to let us see it. Should not even the positivist accept this evidence? Not so: the positivist says that only theory makes us suppose that what the lens teaches rings true. The reality in which we believe is only a photograph of what came out of the microscope, not any credible real tiny thing.

Such realism/anti-realism confrontations pale beside the meta-physics of serious research workers. One of my teachers, chiefly a technician trying to make better microscopes, could casually remark: `X-ray diffraction microscopy is now the main interface between atomic structure and the human mind.' Philosophers of science who discuss realism and anti-realism have to know a little about the microscopes that inspire such eloquence. Even the light microscope is a marvel of marvels. It does not work in the way that most untutored people suppose. But why should a philosopher care how it works? Because it is one way to find out about the real world. The question is: How does it do it? The microscopist has far more amazing tricks than the most imaginative of armchair students of the philosophy of perception. We ought to have some understanding of those astounding physical systems `by whose augmenting power we now see more/than all the world has ever done before '.[1]

The great chain of being

Philosophers have written dramatically about telescopes. Galileo himself invited philosophizing when he claimed to see the moons of Jupiter, assuming the laws of vision in the celestial sphere are the

t From a poem, ` In commendation of the microscope', by Henry Powers, t 664. Quoted in the excellent historical survey by Saville Bradbury, *The Microscope, Past and Present, Oxford,* 1968.

same as those on earth. Paul Feyerabend has used that very case to urge that great science proceeds as much by propaganda as by reason: Galileo was a con man, not an experimental reasoner. Pierre Duhem used the telescope to present his famous thesis that no theory need ever be rejected, for phenomena that don't fit can always be accommodated by changing auxiliary hypotheses (if the stars aren't where theory predicts, blame the telescope, not the heavens). By comparison the microscope has played a humble role, seldom used to generate philosophical paradox. Perhaps this is because everyone expected to find worlds within worlds here on earth. Shakespeare is merely an articulate poet of the great chain of being when he writes in *Romeo and* Juliet of Queen Mab and her minute coach `drawn with a team of little atomies . . . her wag-goner, a small grey coated gnat not half so big as a round little worm prick'd from the lazy finger of a maid'. One expected tiny creatures beneath the scope of human vision. When dioptric glasses were to hand, the laws of direct vision and refraction went unquestioned. That was a mistake. I suppose no one understood how a microscope works before Ernst Abbe (1840-1905). One immediate reaction, by a president of the Royal Microscopical Society, and quoted for years in many editions of Gage's *The* Microscope–long the standard American textbook on microscopy – was that we do not, after all, see through a microscope. The theoretical limit of resolution

IA] Becomes explicable by the research of Abbe. It is demonstrated that microscopic vision is *sui generis.* There is and there can be no comparison between microscopic and macroscopic vision. The images of minute objects are not delineated microscopically by means of the ordinary laws of refraction; they are not dioptical results, but depend entirely on the laws of diffraction.

I think that this quotation, which I simply call [A] below, means that we do not see, in any ordinary sense of the word, with a microscope.

Philosophers of the microscope

livery twenty years or so a philosopher has said something about microscopes. As the spirit of logical positivism came to America, one could read Gustav Bergman telling us that as he used philosophical terminology, `microscopic objects are not physical

things in a literal sense, but merely by courtesy of language and pictorial imagination. . . . When I look through a microscope, all I see is a patch of color which creeps through the field like a shadow over a wall.' [2] In due course Grover Maxwell, denying that there is any fundamental distinction between observational and theoretical entities, urged a continuum of vision: `looking through a window pane, looking through glasses, looking through binoculars, looking through a low power microscope, looking through a high power microscope, etc.' [3] Some entities may be invisible at one time and later, thanks to a new trick of technology, they become observable. The distinction between the observable and the merely theoretical is of no interest for ontology.

Grover Maxwell was urging a form of scientific realism. He rejected any anti-realism that holds that we are to believe in the existence of only the observable entities that are entailed by our theories. In *The Scientific Image* van Fraassen strongly disagrees. As we have seen in Part A above, he calls his philosophy constructive empiricism, and he holds that ` *Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate' (p.* 12). Six pages later he attempts this gloss: ` To accept a theory is (for us) to believe that it is empirically adequate – that what the theory says *about what is observable* (by us) is true.' Clearly then it is essential for van Fraassen to restore the distinction between observable and unobservable. But it is not essential to him, exactly where we should draw it. He grants that ` observable' is a vague term whose extension itself may be determined by our theories. At the same time he wants the line to be drawn in the place which is, for him, most readily defensible, so that even if he should be pushed back a bit in the course of debate, he will still have lots left on the `unobservable' side of the fence. He distrusts Grover Maxwell's continuum and tries to stop the slide from seen to inferred entities as early as possible. He quite rejects the idea of a continuum.

There are, says van Fraassen, two quite distinct kinds of case arising from Grover Maxwell's list. You can open the window and see the fir tree directly. You can walk up to at least some of the

2 G. Bergman, 'Outline of an empiricist philosophy of physics', *Anterican journal of Physics* 11 (1943), PP. 245-58. 335-42.

3 G. Maxwell, The ontological status of theoretical entities', in *Minnesota Studies in the Philosophy of Science 3* (1962), pp. 3-27.

objects you see through binoculars, and see them in the round, with the naked eye. (Evidently he is not a bird watcher.) But there is no way to see a blood platelet with the naked eye. The passage from a magnifying glass to even a low powered microscope is the passage from what we might be able to observe with the eye unaided, to what we could not observe except with instruments. Van Fraassen concludes that we do not see through a microscope. Yet we see through some telescopes. We can go to Jupiter and look at the moons, but we cannot shrink to the size of a paramecium and look at it. He also compares the vapour trail made by a jet and the ionization track of an electron in a cloud chamber. Both result from similar physical processes, but you can point ahead of the trail and spot the jet, or at least wait for it to land, but you can never wait for the electron to land and be seen.

Don't just peer: interfere

Philosophers tend to regard microscopes as black boxes with a light source at one end and a hole to peer through at the other. There are, as Grover Maxwell puts it, low power and high power microscopes, more and more of the same kind of thing. That's not right, nor are microscopes just for looking through. In fact a philosopher will certainly not see through a microscope until he has learned to use several of them. Asked to draw what he sees he may, like James Thurber, draw his own reflected eyeball, or, like Gustav Bergman, see only `a patch of color which creeps through the field like a shadow over a wall'. He will certainly not be able to tell a dust particle from a fruit fly's salivary gland until he has started to dissect a fruit fly under a microscope of modest magnification.

That is the first lesson: you learn to see through a microscope by doing, not just by looking. There is a parallel to Berkeley's *New Theory of Vision* of 1710, according to which we have three-dimensional vision only after learning what it is like to move around in the world and intervene in it. Tactile sense is correlated with our allegedly two-dimensional retinal image, and this learned cueing produces three-dimensional perception. Likewise a scuba diver learns to see in the new medium of the oceans only by swimming around. Whether or not Berkeley was right about primary vision, new ways of seeing, acquired after infancy, involve learning by doing, not just passive looking. The conviction that a particular part of a cell is there as imaged is, to say the least, reinforced when, using straightforward physical means, you microinject a fluid into just that part of the cell. We see the tiny glass needle — a tool that we have ourselves hand crafted under the microscope — jerk through the cell wall. We see the lipid oozing out of the end of the needle as we gently turn the micrometer screw on a large, thoroughly macroscopic, plunger. Blast! Inept as I am, I have just burst the cell wall, and must try again on another specimen. John Dewey's jeers at the `spectator theory of knowledge' are equally germane for the spectator theory of microscopy.

This is not to say that practical microscopists are free from philosophical perplexity. Let us have a second quotation, [B], from the most thorough of available textbooks intended for biologists, E.M. Slayter's *Optical Methods in Biology:*

[B] The microscopist can observe a familiar object in a low power microscope and see a slightly

> enlarged image which is `the same as' the object. Increase of magnification may reveal details in the object which are invisible to the naked eye; it is natural to assume that they, also, are `the same as' the object. (At this stage it is necessary to establish that detail is not a consequence of damage to the specimen during preparation .for microscopy.) But what is actually implied by the statement that `the image is the same as the object?'
>
> Obviously the image is a purely optical effect. . . . The ` sameness' of object and image in fact implies that the physical interactions with the light beam that render the object visible to the eye (or which would render it visible, if large enough) are identical with those that lead to the formation of an image in the microscope... .
>
> Suppose however, that the radiation used to form the image is a beam of ultraviolet light, x-rays, or electrons, or that the microscope employs some device which converts differences in phase to changes in intensity. The image then cannot possibly be `the same' as the object, even in the limited sense just defined! The eye is unable to perceive ultraviolet, x-ray, or electron radiation, or to detect shifts of phase between light beams. . . .
>
> This line of thinking reveals that the image must be *a map of interactions between the specimen and the imaging radiation (pp. 261-3).*

The author goes on to say that all of the methods she has mentioned, and more, `can produce "true" images which are, in some sense, "like" the specimen'. She also remarks that in a technique like the radioautogram ` one obtains an " image " of the specimen .. .

obtained exclusively from the point of view of the location of radioactive atoms. This type of "image" is so specialized as to be, generally, uninterpretable without the aid of an additional image, the photomicrograph, upon which it is superposed.'

This microscopist is happy to say that we see through a microscope only when the physical interactions of specimen and light beam are 'identical' for image formation in the microscope and in the eye. Contrast my quotation [A] from an earlier generation, and which holds that since the ordinary light micro-scope works by diffraction even it is not the same as ordinary vision but is *suigeneris.* Can microscopists [A] and [B] who disagree about he simplest light microscope possibly be on the right philosophical track about 'seeing'? The scare quotes around 'image' and 'true' suggest more ambivalence in [B]. One should be especially wary of the word 'image' in microscopy. Sometimes it denotes something at which you can point, a shape cast on a screen, a micrograph, or whatever; but on other occasions it denotes as it were the input to he eye itself. The conflation results from geometrical optics, in which one diagrams the system with a specimen in focus and an 'image' in the other focal plane, where the 'image' indicates what you will see if you place your eye there. I do resist one inference that might be drawn even from quotation [B]. It may seem that any statement about what is seen with a microscope is theory-loaded: loaded with the theory of optics or other radiation. I disagree. One needs theory to make a microscope. You do not need theory to use one. Theory may help to understand why objects perceived with an interference-contrast microscope have asymmetric fringes around them, but you can learn to disregard that effect quite empirically. Hardly any biologists know enough optics to satisfy a physicist. Practice – and I mean in general doing, not looking – creates the ability to distinguish between visible artifacts of the preparation or the instrument, and the real structure that is seen with the microscope. This practical ability breeds conviction. The ability may require some understanding of biology, although one can find first class technicians who don't even know biology. At any rate physics is simply irrelevant to the biologist's sense of microscopic reality. The observations and manipulations seldom bear any load of physical theory at all, and what is there is entirely independent of the cells or crystals being studied.