

The Ethnographic Autobiography

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The author draws upon an earlier article by anthropologist Stanley Brandes to recommend that when the intent and purposes of relating a life story are clearly anthropological, the work be labelled 'ethnographic autobiography'. An ethnographic autobiography is defined as a life story told to an anthropologist or used in ways that implicate a sociocultural rather than a psychological interpretation.

INTRODUCTION

In the fall of 1983 I published an article titled 'Adequate schools and inadequate education: the life history of a sneaky kid.' It was about an out-of-school youth whom I call 'Brad.' It dealt in case study fashion with a young man who had built and was living in a rude cabin at the back of my property. It seems remarkable even today that I did not discover him for about the first five weeks of his residence, but my 20-acre property is on a hillside bordering undeveloped forest land, and I did not often make the steep climb to have a look around when there was always much to do closer to the house.

The purpose of that account was to emphasize the distinction that anthropologists (and others) make between education and schooling. Brad had already been out of school for about four years, and his attendance had been sporadic prior to that. I wrote the study to show how, although his formal schooling had long ceased, his education continued apace. To live the way he had chosen, there were things to learn every day, especially as he had opted to live essentially without 'working'. He managed to make himself eligible for food stamps, which gave him a regular source of income (about \$70 US per month, enough to eke out a living in 1980s dollars) and he had learned to

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invest his subsidy wisely. Eventually that source dried up as futile efforts to find a job in a period of staggering unemployment no longer made him eligible for the handout, but by that time he had found occasional part-time employment.

I was away much of the first year that Brad lived on the property. I had taken a one-term leave of absence from the university to teach at another institution. I saw him rather infrequently until I returned home after Christmas. Following my return, I discovered that I needed to install a new water line down the hill from its source in a small spring. Talking with a contractor one day about the work, to my surprise Brad made an informal bid of his own, successfully arguing that if he dug a trench by hand he would produce far less of a muddy mess than would any machinery brought in to do the work. Since there was no urgency about the job, I let him go ahead with it, not really sure if he would complete the work but certainly willing to let him try. When he encountered rocky spots or roots where the going was tough, we worked together to dig the trench. As we worked I became more interested in him and in his story. I was fascinated with what he had done to build and then improve the cabin, just as he seemed to be doing with his life.

My own academic interest is in the nexus between education and anthropology, specifically in how people acquire culture. And here, not only in my own cultural milieu but in my own backyard, was someone doing just that, yet in a very different way, a way that I might come to understand but could never emulate. I pondered the idea of doing a case study with Brad, but I was unsure how to go about asking him, or what form such a study might take, or what I might do with it when completed. The idea did not go away. I was not sure what to do about it but I kept wishing there was some way to bring it all together.

And lo and behold – serendipity's finest hour – came a call from someone at the Office of Education in Washington, DC, asking if I would be willing to join efforts with a group of other social scientists who had each been asked to submit a 'white paper' on educational adequacy. I saw my chance in a long shot, inquiring whether they would accept a case study of an out-of-school youth who seemed to offer a different perspective on educational adequacy from what I assumed others would contribute. I never learned how they happened to select my name as a possible contributor, but by the time they invited me they already had enough economists and political scientists writing, so my counter proposal was acceptable, even if a bit quirky. (I might note that finding a way to contribute through ethnography was personally quite satisfying.)

Now I had a reason for doing the case study. And Brad, no doubt inspired more by the offer to be paid than by the higher purposes of the project, agreed to be interviewed. He even was able to estimate how long he thought it would take to tape his life story: 12 hours.

Not given to long discourses, Brad's estimate was a bit generous. At our first taped session he spoke rapidly for about six minutes, ending abruptly with '... and so I came up here. And that's it!' As far as he was concerned, that *was* it. From that point on, the impetus for further dialogue had to come from me.

But we had a start. By returning again and again to some of the big themes in his life – especially his roster of 'experiences' – and extending a series of short interviews over several weeks, I had enough to begin putting parts of his story together: his parents' divorce, his struggle to get along in various household arrangements, his escapades, trouble with the police, a year spent in reform school, his thoughts about getting his life together. Plus, I could supplement what he said in interviews with what I was learning from him through casual conversation.

Eventually I had a draft of the report I would send to the Office of Education. Brad read it over without enthusiasm, pretty much dismissing my effort as 'the kind of stuff professors write'. But he was satisfied with the accuracy of the draft, and he suggested few changes. I reworked the draft. He did not take the opportunity to read it again but expressed the hope that it 'might help make people understand', a phrase on which he did not elaborate.

Brad was still living in his cabin when I submitted the report. It fulfilled Contract No. NIE-P-81-0271. To the best of my knowledge, nothing ever became of it. The report did not rouse a nation to do something about a problem, it showed that something had already been done. That is, the *reporting* function was completed; contractual responsibility ended there.

My original report was titled, 'Adequate schools and inadequate education: an anthropological perspective' (Wolcott, 1982). Personally I was more than satisfied with it, both for what I felt was the remarkable degree of Brad's candour as an informant and for my organization and analysis of the material as a study of an out-of-school youth. The Office of Education, which by then had become the National Institute of Education, offered useful editorial suggestions and thanked me profusely for my effort. But I realized that nothing was to become of the report itself or the other reports that together comprised the School Finance Project.

Brad had put me in touch with his life. I felt that his words, coupled with my efforts to bring some analysis to the case, warranted

a broader audience. I wondered whether the report might be rewritten for a professional journal. Even my own journal, for instance. For in the interim following Brad's unannounced arrival, I had been asked to assume the editorship of the *Anthropology and Education Quarterly* (AEQ), my tenure to begin with the spring issue of 1983.

The journal's previous editor had held the post for six years. He had spent months tying up loose ends and publishing manuscripts that had been lost or misplaced, and he resolved not to saddle his successor with a similar backlog of manuscripts. The result was that while he had received too many manuscripts from the previous editor, he left me with none. One year was too short a time to get the submission process rolling smoothly again. I came to my first publication with virtually nothing to publish, save some short pieces I had commissioned and a piece by one of my graduate students. So, with the blessing of the journal's founding editor, John Singleton, I printed my article as headliner. In a brief introduction, John acknowledged my 'provocative style' in setting the tone of an editorship with an instance of my own writing.

For a journal titled the *Anthropology and Education Quarterly* I did not have to announce that my paper took an anthropological perspective, but I wanted to emphasize that the study was conducted in the anthropological tradition. It was a case about *somebody*, not *everybody*, an anthropological life history, albeit a brief one. Brad himself had explained that in his childhood his mother had accused him of being a sneaky kid. So my subtitle for the rewritten piece became 'The life history of a sneaky kid'.

And here I turn to the title of this paper, 'The ethnographic autobiography', to pursue a different course from the story of Brad. His account, presented in the original sneaky kid article, was followed by two additional pieces elaborating on further developments in the case. The complete story has now been written in a book published by AltaMira Press titled *Sneaky kid and its aftermath: ethics and intimacy in fieldwork* (Wolcott, 2002).

By 1983, when the original article was published in *AEQ*, Brad had left. I rather doubted that I would ever see him again. At first I hoped I would, but things took a different turn, and when next we met the circumstances were not the same. Nevertheless, I often thought about him, and since I could not get him out of my mind, I continued to write about him. Any further writing had to be more analytical and conjectural, for in the subsequent years I have learned little about him except that he is still alive, or was when I last checked. Today he would be approaching his mid-forties.

I wrote the two additional pieces in the 1980s, filling in details that were not relevant to the purposes of the original article about educational inadequacy but were critical to the continuing story of Brad himself. I addressed more directly the nature of the personal relationship that eventually developed between us (Wolcott, 1987a; 1990). Subsequently the three articles were reprinted together in *Transforming qualitative data* (Wolcott, 1994), published by Sage. There they became known as the Brad Trilogy, and Brad and I became widely known, at least in certain qualitative research circles.

Subsequent to that, I was encouraged to retell 'our' story in book form, inspired by publisher Mitch Allen of AltaMira Press and by playwright Johnny Saldaña, who worked the material into a play, 'Finding my place: the Brad trilogy' (Saldaña, 2002). I trust that readers will realize that the book's title, *Sneaky kid and its aftermath* announces that it is the *story* of the sneaky kid that has now become the subject, not a further story about Brad himself, for, as noted, I have nothing further to report about him.

The problem I pose for this paper goes back to the article that I prepared for the Office of Education and its subsequent transformation for the *Anthropology and Education Quarterly*. The subtitle of that paper was 'The life history of a sneaky kid'. The question: Was 'life history' a suitable subtitle at the time? And would 'life history' be a suitable title today?

THE LIFE HISTORY IN ANTHROPOLOGY

In editing my original report of 1982 for publication in a professional journal, the term 'life history' seemed not only the best but the obvious phrase to describe what I had written. And I presented it as an anthropological account. True, I had not written about some member of an exotic tribe; I had written about a member of my own tribe, a youth who had spent most of his life in the same locale where I lived. We also shared pretty much the same values, although by my standards his means for obtaining what he needed seemed to require more cunning and be more 'devious'. Nor did my report feature a tribal elder who could reflect back over a lifetime struggle to achieve, but a 20-year-old youth whose 'struggle' was day-to-day and whose life still lay ahead of him, at least to the extent that he could maintain his low profile. So although of a slightly different kind than what ordinarily appeared in the anthropological literature, Brad's account was a life history.

During the ensuing years, however, anthropologists, like other social scientists, have become more acutely aware of their

relationships among those with whom they deal and more acutely aware of the words they use to convey the nature of those relationships. At the same time, they are no longer as wary of being described as 'story tellers' as they were in the past. With no opprobrium attached to the idea of telling a story, they have become content with, and less apprehensive about, relating someone else's 'story', in place of claiming to have recounted a life history, even a partial one (see, for example, Peacock and Holland, 1993). Had the circumstances been right, I would have been only too happy to label Brad's account his *story*, thus pointing to certain anecdotal features rather than seeming to lay claim to know more than I ever will know about his history.

ENTER 'AUTOETHNOGRAPHY'

At the time we did not recognize the potential for confusion in a term that anthropologist David Hayano only recently had introduced, *autoethnography* (Hayano, 1979; 1982). Hayano coined the term to describe his then current project, an ethnography of people like himself who spent their leisure hours playing cards in Southern California's legitimate card rooms. He found a seemingly perfect phrase in 'autoethnography', a reference to conducting a study among those who share a common activity in which one is himself or herself engaged. For Hayano, who admits to his pleasure at 'risk-taking activities', the poker room was where he was spending a lot of time. He decided he might as well write up what he was learning, and he needed to signal to his reader his advantaged position as a fellow player. He certainly was not doing an ethnography 'of' or 'on' himself; that was not the meaning he intended for autoethnography. As he used it, the phrase simply described conducting research as a true insider, in contrast to the more customary role in which the anthropologist is, at best, a peripheral participant.

Had the term 'autoethnography' been employed only in the strict sense that Hayano intended, it might have served us well. Taking my own field of 'anthropology and education' as a prime example, we might have nodded approvingly at the overwhelming number of studies conducted in schools and classrooms where the teacher, or a former teacher, was the principal investigator. But, as often happens, Hayano's catchy label was soon separated from the type of study that had spawned it.

Instead of being applied exclusively to studies where the researcher was also a participant, the term 'autoethnography' was applied as

well to the study of oneself, with or without a group in tow. First came individual articles, then collections and books reporting examples of autoethnography (see, for example, Reed-Danahay, 1997 or the series edited by Carolyn Ellis and Arthur Bochner). And this produced confusion, for the term 'autoethnography' was far more often applied to the latter, 'auto' referring to the self, and 'ethnography' added to validate a method that most often took the form of a personal narrative or diary. The articles were serious in intent and often painfully personal, but they seemed to follow no particular form nor owe allegiance to any particular discipline.

I found the new format distracting. It was not because the stories did not read well; they often did. But I was distressed to see the term 'ethnography' bandied about by researchers who assigned the label to their work only because their accounts were personal. 'Narratives of the self', Laurel Richardson labelled them, and they were just that, 'highly personalized', a 'revealing text in which an author tells stories about his or her own lived experience' (Richardson, 1994: 521).

I do not mean to assign mystical properties to ethnography. In general, the term refers both to the research arm of cultural anthropology (and qualitative sociology) and to the customary product of that research, the (usually written) 'picture' of a 'people' that the ethnographer offers as a result of fieldwork. Since ethnography is concerned with people, the quality of ethnographicness in a life story comes through in the social setting in which an individual life is played out vis-à-vis others in that setting.

In my own studies I have endeavoured to define the essence of ethnography, and I have suggested the distinction that can be made between 'doing ethnography' and borrowing an ethnographic technique or two. But there was little point in haranguing that autoethnographers were not already exploring, developing and committed to their approach. What seemed to be needed was a term that would convey to the reader when the story, or life history, did bear the stamp of an anthropological orientation, an ethnographer present but never centre stage; when it did in fact illustrate what I have called 'ethnographic intent' (Wolcott, 1987b).

ETHNOGRAPHIC AUTOBIOGRAPHY

There is a more suitable term. It was available even back when I was preparing the original sneaky kid account. I realize that now because I see that among my references I included an article in which it was introduced, although the usefulness of the term completely eluded me at the time.

The phrase is ‘ethnographic autobiography’. It was introduced and discussed at length in anthropologist Stanley Brandes’ article ‘Ethnographic autobiographies in American anthropology’ (Brandes, 1979; 1982). My problem, presumably shared among my peers at the time, was that we did not feel we needed a new term. For all the life-history research being reported, there was no confusion about terms, no self-consciousness about the terms that we were using, no thought of making ourselves the subject of our inquiries.

Brandes presented his views on the ethnographic autobiography in a paper given at the Spring Hill (Minnesota) Conference on ‘American social and cultural anthropology: past and future’, in October 1976. That paper was published in 1979. Subsequently a number of the conference papers were revised for reissue in a book from Garland Press titled *Crisis in anthropology: view from Spring Hill, 1980*, published in 1982. That is where I came across it.

There had been crises before, and there have been crises since, and as best I recall the book did not precipitate a call to arms. However, the articles have held up well, and I can recommend the book for a lively read about topics that concerned anthropologists at the time. But for me, Brandes had put his finger on something even more timely now than it seemed at the moment.

Brandes’ assigned topic was the state of ‘life history research’ in anthropology. He begins his article with some cogent remarks about the anthropological life history in general. They bear repeating to help establish the fact that the anthropological life history has its unique place in life history research, if only for the lack of uniqueness of its customary subjects.

As Brandes notes, the protagonist of an *ethnographic* autobiography is almost always ‘an ordinary member of his or her society, whose individual achievements are not noteworthy in and of themselves’, and who is ‘far from being an introspective, intellectual person self-motivated to produce an autobiography’ (1982: 188-89). Most frequently the informant had been a member of a non-Western society ‘of the type anthropologists typically study’ (1982: 188). But Brandes recognized changes taking place that were opening new opportunities for research closer to home at a time when opportunities for research overseas seemed to be diminishing.

Brandes’ description fits Brad’s case superbly: an ordinary member of society whose individual achievements are not noteworthy in and of themselves. Yet I found Brad to be a remarkable example of resilience and self-reliance in living almost completely on his own. And no doubt it gave Brad a certain pleasure to be able to recount and review his accomplishments and ‘experiences’, although I doubt that

he ever quite fathomed the point of my devoting so much attention to the project.

Brandes stresses that the ethnographic autobiography is a true autobiography, not to be confused with a third-person account or reminiscence but a first-person narrative told in the actual words of a real person. And that is the case with Brad, whose actual words in the original article make up at least half the story – and made his role relatively easy to recast in play form.

There is always the condition of ‘time’ in the ethnographic account. It is not that long, intimate acquaintance assures a full account, but that ‘quickie’ studies and brief expressive interviews do not fill the bill for what Brandes considers the ‘larger, more ambitious life histories’ about which he was writing (1982: 189). In that, perhaps I was more fortunate than some ethnographers, for although my formal tape-recorded interviews were completed in a matter of weeks, Brad lived on my place for more than two years. That long period provided a rich context.

Brandes argues for the ethnographic autobiography on the basis of its humanistic contribution, railing against those of his colleagues who believed (and continue to believe) that social anthropology can approximate the methods of the so-called hard sciences. ‘People are not automatons, responding blindly to the vague factors and forces that are said to complete this or that type of action’ (1982: 190). I could not agree more. Brad’s story has been around for more than 20 years now, and although we both have our detractors, countless numbers of graduate students and others who have encountered Brad in their reading have had to come to grips with him, and with me, in the specific form that each of us took in the original story and its sequels.

Brandes discusses some of the criteria that the anthropologist tries to consider in selecting an informant for an ethnographic autobiography. But he concludes, ‘Whatever the particular research aims of the ethnographer, the selection of an informant is frequently conditioned by purely fortuitous circumstances of fieldwork’ (1982: 192). It seemed to take forever for me to realize the link between my long-term professional interest in cultural acquisition and the fact that Brad had come to live on my place. That I might achieve some understanding of his case as an example of cultural acquisition could have escaped me all too easily. It still leaves me wondering how often we try too hard to seek out informants or search for the perfect locale. There is no such thing as the ideal informant or the perfect setting. But there may be quite satisfactory informants standing right in front of us.

FINE TUNING

Brandes takes a quote from an early and popular treatise on life history written by L.L. Langness, *The life history in anthropological science*, to point out that life histories are 'seldom the product of the informant's clearly articulated, expressive, chronological account of his life' (1965: 48). It would seem almost self-evident that, by reason of their very ordinariness, most informants' accounts, essentially oral as they are, would need rearranging and editing in order to read well, especially if one's purpose is to portray a cultural rather than a psychological setting. I was surprised that some critics took exception to my statement when I reported that I 'worked with a heavy hand in reorganizing material and selecting the most cogent excerpts from months of informal conversation and many hours of formal interviews' (Wolcott, 1983: 8) to produce an article length account about sneaky kid. I did not expect Brad to come up with a coherent account. When we began, I was not sure there would be any account at all.

It must also be noted, however, that an informant's account does not *necessarily* need editing. In his day Brandes could point to a recently completed account that was published by Charles Hughes, *Eskimo boyhood: an autobiography in psychosocial perspective* (1974), which was solicited from a youthful informant who was hospitalized at the time with tuberculosis. That autobiography was published with only slight editorial changes. In similar fashion, I have recently published (in the 2003 AltaMira reissue of *A Kwakiutl village and school*) an autobiographical statement written in 1963 by Lucy Puglas. Lucy was a Kwakiutl Indian woman who wrote her own story so well that I left it intact except for some additional punctuation. In her case, I did not even realize that she had completed the writing after I proposed it, for she drowned in a boating accident before she was able to present me with the finished product.

Brad, on the other hand, had the barest of writing skills. Although he had procured a dictionary for the cabin and was able to read, his efforts to produce anything in writing were limited to brief notes, which took some charitable deciphering to be understood. An oral tradition was our only hope.

Statements like the one obtained by Hughes, or that of Lucy, are true autobiographies. They are 'ethnographic' only in the sense that they might never have been recorded had not an ethnographer suggested it; in Lucy's case, that was all I had to do. Whether or not they find their way into the anthropological literature depends on who

does the asking. My hunch is that today other literary genres account for much of what might have become 'anthropological' in earlier times.

After reading *Sneaky kid and its aftermath*, a colleague, anthropologist Allan Burns, noted a parallel with an account he had read earlier in Spanish, Juan Gamella's *La historia de Julián: memorias de heroína y delincuencia* (1990). Julián's account, covering a period of 10 years of his life, grew out of a larger project investigating the then new wave of young intravenous drug users in Madrid. Gamella's interviews with Julián began the same year as my interviews with Brad (1981); the two youths were only a year apart in age. Although Brad did not use hard drugs, their exploits during those years had many similarities. The major difference occurs in narrative style. Gamella has Julián tell his own tale, interspersed only with comments by a few others close to him, especially Julián's mother. Both studies are ethnographic autobiographies, each intended to offer readers an insider's view.

For third-person accounts, where an anthropologist either shares the stage with an informant or uses a life history to illustrate a different point (for example, about the nature of fieldwork), Brandes suggests the term 'anthropological' rather than 'ethnographic'. It is probably useful to have a residual category such as 'anthropological life history', although my interest here is with the ethnographic one. I should note, however, that I consider everything else in the full account of *Sneaky kid and its aftermath* to be of this second type. It is anthropological, and sometimes autobiographical (not only about Brad but also about me), but personally I do not consider it to be ethnographic, at least in the sense that the original sneaky kid article is. The distinction works, at least for me. I prefer it to the label 'autoethnography'. It draws a finer line between what is truly 'ethnographic' and what is essentially a personal account in which I am one of the players.

IN SUMMARY

In the mid-1970s when Stanley Brandes first drafted his article, he was clarifying something that benefited from the attention but had not yet been muddled by time or competing terms. Today, with life history reported in a wide variety of ways, in so many fields, and with many alternative labels to draw from, there is more reason to examine the labels we choose and to apply our labels with care. I applaud efforts to produce life history, or life story, or autoethnography. But

in cases where an ethnographic perspective is central to the purpose, I suggest that we can, and should, signal our intent by labeling our work as 'ethnographic autobiography'. That was my intent with the original sneaky kid article. Were I looking for a label today, that is the label I would use.

Brandes notes that an ethnographic autobiography usually is 'recorded and edited by a social anthropologist or by some other professional with interests closely allied to sociocultural anthropology' (1982: 188). Whether that individual is a card carrying anthropologist seems of less importance than the perspective from which one works. There are many with social science training working outside academic institutions today, researchers who find useful applications of that perspective in their work. They are not excluded from using the label.

The life history is not peculiarly anthropological, but there should be something peculiarly anthropological about a life history or life story designated as ethnographic. Of necessity life history deals with personality, but the purpose of an ethnographic account is to situate personality within a cultural context. Whether explicit or implicit, we should be able to discern the influence of a cultural system in the direction the account takes.

Brandes reaffirms his belief that life histories in general, and the ethnographic autobiography in particular, have provided an essential research tool through which to achieve the intellectual task of defining the relationship between individual and culture. He laments what he saw in the 1970s as the tendency for individuals to be 'forgotten' in anthropological analyses, a tendency he felt was exacerbated by government funding that attempted to portray anthropology as a 'fully positivistic, objective discipline, whose main goal is to discover laws of human behavior' (Brandes, 1982: 200).

In the years since Brandes wrote, we have witnessed a softening in this regard. Some anthropologists have resisted the urgency to make their discipline 'fully positivistic', and government spending exerts less influence, in part because there is relatively less of it. As long as we have researchers willing to collect and report ethnographic autobiographies, there will be a prevailing force to counteract claims about the dominant role of 'culture' to set the course of human lives. Culture provides us with notions of an ideal: what we have come to expect from others and what we believe they expect from us. But only as we direct attention to how specific individuals – like Brad, or like me or you – actually live out our lives can we ever know how things really work out.

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