



Introduction

Life History Method

History as a general discipline has had its own long and convoluted history, with diverse topics of interest and reasons for pursuing them coming in and out of favor. At heart there is the question of *what* happened in the past, followed by an inquiry into *why* certain things happened. There can also be a question of the relative importance of certain events and their consequences, which depends on your reasons for inquiring in the first place. Thus, we have political history, economic history, social history, military history, and on and on. For centuries, history as a discipline concerned itself with matters that the elite members of cultures cared about because they were the ones with the ability to read and write, or had the resources to employ a class of people who could. We know about kings and queens of the past and their general exploits because they took the trouble to have them written down. Thus, we can speculate, to a degree, about the interests and exploits of Cyrus the Great or Richard the Lionheart because we have a few surviving documents to work with produced by scribes and priests. But if we want to know about the life of an English peasant in the tenth century CE or a Persian soldier of the sixth century BCE, we have to resort primarily to archeology, and even there we have precious little data.

Hundreds of thousands of Anglo-Saxon people lived, worked, and died in England, facing sorrows and hardships along with joys and delights, yet we know next to nothing about the particulars of their lives.

There are glimpses now and again when a scholar gives us a hint or two, but centuries drift by with—nothing. What did it feel like to spend the winter at Valley Forge with George Washington (who had a private house to hole up in), or to face down the French troops as a sick and exhausted Bowman under Henry V at Agincourt? Our views are forever colored by drama, fiction, and ahistorical speculation. Even letters home, diaries, and poetic musings are specialized, and crafted, views. When Stephen Crane wanted to get the real feel of life on the battlefield during the American Civil War before writing the *Red Badge of Courage*, he sat around in the park in Port Jervis, New York, and talked to veterans of the war. Those are the real stories of everyday people, not the carefully polished ruminations of professional historians.

I am not trying to make the case that the people who lived through momentous times tell a story that is somehow better or more real than the story historians tell. But it is a *different* story—and it is different in critical ways. First and foremost, the first-person narrative of events can get inside the experience of the event—what it felt like to be there. For the most part, historians are not concerned with issues of lived experience, and they have precious little to go on even if they are. Social history, which exploded in importance in the second half of the twentieth century, made a valiant effort to come to grips with the daily lives of ordinary people but without much data to work with. Enter the ethnographer.

We cannot go back into the past beyond living memory and magically create primary documents that bring those times to life, but we can speak to people who are now in their nineties, which puts us back into the late 1930s, and all points from then until now. How did the Vietnam War change life in Australia? (Yes Australian conscripts were sent to that war.) What was it like to live through the Pol Pot years in Cambodia? Where were you when the Twin Towers fell on 9/11? What was life like in Uganda under Idi Amin? . . . And on and on. Pick the time and place you want, and you can find an abundance of eyewitnesses whose recounting of history has a very different quality from official records.

An oral narrative about one's own lived experience is not like any other kind of history—it is in a genre by itself. While such a tale can be characterized as autobiography, it is a different animal from what is normally thought of as an autobiographical story. There are several key differences between the two types. First, an oral life history is spoken and not written. It is told in the narrator's own voice—what is sometimes (misleadingly) called “authentic voice”—suggesting that the tale is not artificially crafted in the way that written prose is. It is crafted, of course, but not in the same way that a written story is. An oral tale does not re-

quire skills in writing, or even literacy. Second, an oral narrative is told to another person rather than being a solo enterprise, and, therefore, the interaction between teller and listener is an essential component in the way the narrator shapes the tale, depending on the relationship between the two—which can be conceived of as a certain kind of partnership. How this relationship operates is a major theme of this book.

Back in the 1980s, teaching fieldwork methods to undergraduate anthropology majors was a rarity, and, indeed, there were many graduate programs that did not require such training. I went into the field for my doctoral fieldwork in 1978 with no training beyond the occasional hands-on project I had done for a term paper. I was expected to just sort of pick it up. Since then, things have changed radically. For example, H. Russell Bernard pioneered summer programs in fieldwork methods of diverse kinds for anthropologists at all stages of professional development and accompanied them with diverse publications (e.g., Bernard 1998 and 2005).

When I was asked to revise the requirements for anthropology majors at Purchase College, SUNY in 1988, I added a course in fieldwork methods to be undertaken in the fall semester of the junior year. The college had (and has) a senior thesis requirement, which for anthropology majors up to that point had usually consisted of library research. Having taken a methods course, the students could then contemplate conducting a limited field research project, which proved to be immensely successful. The star component of the methods course for most of the students was the life history project, which a great many of them found utterly transformative. From the first year in which I taught the course (1989), I was blown away by the quality of the life histories produced, and I even coauthored a paper with one of my students on her work (Forrest and Jackson 1990). She is also represented in this volume.

Thirty plus years later, expectations for undergraduate field research have changed dramatically, and methods courses are becoming the norm. There are, nonetheless, complex challenges when directing undergraduate field research despite its obvious benefits.

Undergraduates as Fieldworkers

In 1969, as part of a root-and-branch reexamination of anthropology, Dell Hymes made the following apocalyptic pronouncement:

The future of anthropology in the United States is . . . a question of whether its present institutional context, especially the graduate department, will prove to have been chrysalis or coffin. If anthropology remains confined

there, it may wax as an instrument of domination, wane into irrelevance, or—more likely—combine both fates. It is unlikely to contribute much to the liberation of mankind. (Hymes 1972a: 6–7)

Many in the profession might quietly affirm that this vision has proven extraordinarily prescient. The problems that Hymes was alluding to may reasonably be subsumed under the general rubric of professionalism; that is, a largely tacit, but all-consuming, function of graduate departments is to perpetuate themselves and to enhance the reputations of their members more than to increase our knowledge of humankind and to further the needs of its members, and their goals are fatally introverted.

The gilded path for the graduate student in a major department is something on the order of: coursework, exotic fieldwork, PhD, professorship at a major department leading to the training of other graduate students who perpetuate the cycle. Despite seemingly heartfelt appeals by successive generations of presidents of the American Anthropological Association to push anthropological insights into public consciousness, the development of numerous applied programs, the employment of anthropologists as congressional advisers, and so forth, there is still a strong sense within the profession that the gilded path is the way of the best and brightest and that these other courses are second best—at best. More usually, popularizers and activists are held up to subtle (and unsubtle) forms of ridicule and discrimination within the ranks of graduate faculty at elite research universities.

Teaching undergraduates is often perceived by the luminaries of graduate departments as an index of failure as a professional anthropologist. That is, PhDs who accept appointments at predominantly undergraduate institutions may be characterized as having had to settle for the low road, ultimately destined for obscurity and drudgery. Likewise, the task of instructing undergraduates at major research institutions is frequently seen as a necessary evil: it pays the bills and keeps the deans happy, but few graduate faculty relish the prospect. Introductory courses, commonly the only anthropology courses that undergraduates take and the mainstay of undergraduate programs, can be relegated to the care of graduate assistants—in part or in total. Furthermore, at some elite universities, a record of good undergraduate teaching is registered as a black mark on one's professional credentials. I was once interviewed for a position at one of the top anthropology departments in the United States, and a senior faculty member advised me beforehand not to mention my love of undergraduate teaching, or to even bring up the topic.

Compare the actual state of affairs nowadays with Hymes's hope for the future of the discipline:

The critical and scholarly role is indispensable . . . especially given the present disarray and inadequacy of relevant knowledge. Nevertheless, within the academy, a redistribution of attention and prestige from graduate to undergraduate training of anthropologists is important. Given the opportunity, undergraduates could be trained in anthropological work as well as graduate students, perhaps better; much graduate time is spent on activities required, not for training, but for induction into the hegemony of a particular department and a prospective profession. . . . Undergraduates would be freer to acquire relevant training and do good work, having in mind long-range plans not under the control of their teachers. The greatest contribution of anthropology departments might be to send into the world many lawyers, historians, activists, workers for various institutions and agencies, well trained in anthropological work. (Hymes 1972a: 56–57)

The agenda here is, in its broad outlines, disarmingly simple: allow graduate departments to continue in their scholarly function, but pay much greater attention and devote more resources to the training of undergraduates, especially those whose ultimate professional goals are *outside* the discipline. Thus, the broader professional and activist world becomes leavened with the spirit of anthropology, and thus are spread the knowledge, aims, and ideals of the discipline beyond the narrow confines of academic circles. In principle, such redistribution of energy and resources is perfectly practicable; all that is entailed is the will to do it. But the will is tied to a rather thorny detail, namely, the ability (and desire) to shift prestige from graduate to undergraduate training. This cannot occur by an act of will alone; the training of undergraduates must develop its own prestige.

The attraction and kudos of graduate training lie in its association with creative scholarship. Graduate seminars keep faculty up to the mark in the latest research developments in specialized fields and may lead to publications by faculty and participants. Undergraduate training, by contrast, is routinely treated as drudgery—roughly equivalent to conjugating verbs in French I or observing the effects of osmosis on potato cells in a general bio lab. The task is often conceived as the duty to convey a certain body of information and basic theory on the part of the teacher and to memorize it sufficiently to pass a few examinations on the student's part. But if this is all there is to undergraduate training, then it should not be a matter of wonder that Hymes's ambitions for it have not and will not ever be realized. It is hard to imagine prestige accruing to such a minimal exercise, or much leavening of the general public occurring as a consequence. More likely, all parties are equally delighted when the semester ends and the experiences therein can be forgotten.

For many decades there has been a certain level of disingenuousness hovering over introductory classes in anthropology. Look over the vast welter of textbooks on offer and you will see much the same substantive content packaged only marginally differently from decades gone by. Somewhere along the way you will encounter the Iroquois kinship system, potlatch, and the kula ring, as if these topics are part of an essential grounding in the field. Why? The fieldwork that such knowledge is based on is really old—at least one hundred years, or more—and a critical eye surveying these eternal verities of the field easily spots the fissures in what ought to be firm foundations for new research. Nor does eschewing the old ways for a radical postcolonial perspective help the cause sufficiently; instead, such an enterprise has to be tacked on to existing texts, written under old models, because a radically innovative textbook stripped of all the old and outdated theory and substance has not been written yet.¹ The simple fact is that intro courses in anthropology are largely obsolete methods of indoctrination into a field that no longer exists. This is not how to realize Hymes's model for undergraduate education in anthropology. We need to be more practical and get down into the weeds.

For undergraduate training to hold the same significance as graduate training, undergraduates must be taught to appreciate anthropology by practicing it, and they must be guided to produce worthy, original work. I have experimented with numerous methods over the years, but this book cannot delve into all of them (see Forrest 2022 for more). Rather, I will expound on one significant component (maybe the most significant) of my overarching teaching technique, that is, to introduce students at the earliest possible moment to field materials and to get them out doing their own fieldwork projects as quickly as is reasonable.

My purpose is straightforward. Without an appreciation of how field data are gathered and what they look like in the raw state, it is difficult to understand where ideas about cultural patterns and general social theory come from, and therefore all too easy to imagine that it is hocus pocus: one theory being as good as another (which is not awfully far from the truth). The notion of probing basic data personally is the justification for having new students of biology dissect frogs, or physics students stretch springs, or language students take a summer abroad. In introductory classes for freshmen and sophomores, I expose them to my own field data in the very first class, and periodically throughout the semester—unedited video footage, audio tapes, transcriptions of interviews, slides—sometimes so fresh that I have barely had time to catalog, let alone reflect on, them myself. My students see both the richness of

fieldwork and also all of my mistakes, bumbling maneuvers, and errors of judgment. Together we explore—through discussion—patterns and meanings latent in the materials, as well as the advantages and pitfalls of classic methods. I also give all of these students the opportunity to design and implement small, supervised fieldwork projects of their own, such as collecting kinship data from their families or photodocumenting a familiar ritual. First-semester-junior majors begin on their own more extensive field investigations in the course I created, and senior majors design, execute, and write up a complete ethnographic project of their own devising over the course of an academic year.

I have only one basic directive that I expect my undergraduates to follow in all of their projects: they must *care* about what they are doing. They must demonstrate to me that they have some purpose for the task beyond handing in an assignment or getting a grade. My goal with this directive is twofold. My first concern is ethical. I do not want my students engaging with the lives of others without a strong sense that people matter. When a student in a lab plunks zinc into a beaker of sulfuric acid to produce hydrogen, there is little need to be concerned about the feelings of the zinc or the morality of the act. But interviewing people about their lives can always be touchy. Doing it without thinking or feeling is always morally wrong. Secondly, when my students misguidedly ignore this directive, their work is always trivial and uninteresting; when they follow it, their work always contains valuable nuggets.

It is not enough simply to give the order that they must care about what they are doing, however. To do no more than that is as alienating as saying “you must all get As” or “you must not miss any classes,” and likely to be as productive. The process has to start by showing them what it means to care, and how to work on caring as an intrinsic part of the methodological process. One step in the right direction is to show them by example that I care about what I am doing. This approach, however, may appear as no more than vanity, or eccentricity, or worse if unsupported by other conditions. What seems to turn the trick more often than not is the students’ encounters with people in the field who care about what they are doing. In such cases it is the person being questioned, rather than the fieldworker, who provides the motivation and justification for doing the fieldwork, that is, empowers the fieldworker. Thus, finding someone who cares deeply about their work can be a kind of conversion experience for new fieldworkers. The symptoms are usually clear: a glow of success, a babble of talk about how exciting the project was, and a write-up that is reams longer than required. The sense of fieldwork being a collaboration between student and interlocuter, with both of them empowering the enterprise, begins at this point.

Good work of the sort produced by my best and most caring undergraduate fieldworkers deserves to be published, but the justification for publishing it is not quite the same as for the work of graduate students and other professionals in the field (see Spradley and McCurdy 1972; Jenike 2005; Lancy 2003). As has been noted several times by such scholars engaged with undergraduate researchers, undergraduate fieldwork is strongest in producing rich data, and often rather less successful when it comes to the sophistication of analysis. I do not find this state of affairs troubling, limiting, or even surprising. Why would we expect an undergraduate of traditional college age to produce a nuanced appraisal of field data that is as profound as that of a seasoned scholar? Their competence as fieldworkers lies less in their encyclopedic knowledge of anthropological theory and practice and more in their energy and devotion to their interviewees. In fact, their limitations in theoretical depth and rigor may be a virtue in that they may be more inclined to gather data without a strong sense that they are championing a specific ideological cause. They may, thereby, discover hitherto untapped narrative resources.

Because of this relatively low level of interest in professional concerns, and career advancement, via fieldwork, undergraduate relationships with people in the field are special, and I will discuss this issue at greater length in due course. For the moment I will note simply that the projects represented in this volume are not of the same type and quality as those created by postgraduate researchers. For example, these life histories quite often act as the vehicles for the presentation of authentic voices that are seldom, if ever, heard in professional circles or even in mainstream media. Neither are these voices dressed up in flamboyant theory nor otherwise laden with interpretation. Rather, they are allowed to speak for themselves; their theoretical stances latent within them.

Whether or not such work can attract to it the prestige that is currently attached to the more usual types of postgraduate professional research is not for me to decide or legislate. What I might say instead is that the notion of the shifting (or redistributing) of “prestige,” as such, is unlikely to occur for all manner of reasons associated with professionalism; but if we replace “prestige” with “value,” then some kind of reorientation of interest may happen. The discipline may not have the capacity or desire to bestow prestige on the works presented in this book; but these projects do, nonetheless, have value for the discipline, and may be used by it. Although this semantic difference may seem meager or unimportant at first, the implications of seeing genuine value in undergraduate fieldwork are weighty. Not least of these is the recognition that undergraduates can be much more than the passive recipients

of an education (if passive receiving can be honored with the name “education”). They can be our collaborators, as an intrinsic part of the process of universal learning; we can learn from them as well as vice versa. The appropriate trope for this kind of education is, therefore, “sharing” rather than “instructing,” and suggests ways in which learning in the social sciences might act as an example for other disciplines.

Related curricular initiatives have been garnering much attention in higher education circles for some time under a variety of rubrics such as “collaborative learning” and “cooperative learning,” and, indeed, such pedagogic concepts have an exceedingly long and distinguished history (see, for example, Rau and Heyl 1990; Slavin 1989; and Totten et al. 1991). But my goal is much greater than simply to create a learning environment that motivates undergraduates to do good work for me. I am encouraging students to produce work that has intrinsic interest for professionals within the discipline as a whole. I am aiming to make my students my professional colleagues in certain well-defined ways, and to make their research *matter* outside the classroom and the college.

It seems to me to be profoundly contradictory and ultimately self-defeating for us to preach humanism but teach tyrannically. Valuing undergraduate contributions to the discipline does not arise from a desire to have it so, however, any more than endowing them with prestige does. Part of the function of this book, therefore, is to make it clear through both direct presentation and through a measure of interpretive analysis where the value in this work lies.

Undergraduate fieldwork at its best is so good—as Hymes suggests—because students’ care for their data can, and does, take precedence over all other personal and vocational interests. My students have no need to act as my academic shock troops, as graduate students frequently act for their advisers (the reward being a strong job reference). Nor are they in the service of any particular ideology or theoretical stance. Most of the time they are innocent of such drives so that they are potentially more humanistic than further advanced scholars; and we can all learn from their devotion to the work itself as opposed to how it looks to others, or where it will get them professionally.

The materials represented here were all gathered and written up by undergraduates, although all have long since graduated. Only one of them continued to pursue postgraduate training in anthropology, my colleague and consultant Janette Yarwood (who vacillated for many years between medicine and anthropology). All the others are employed as social workers, activists, and professionals of various stripes. But all retain a strong sense of the anthropological method, and they often refer back to their fieldwork classes as turning points in their understanding

of key concepts: how to listen well, how to attend to authentic voice, how to situate meaning in cultural context, and so forth. They are the realization of the vision that Hymes had over fifty years ago; they are also the personification of my own hopes and aspirations for the field, and of a series of individually rewarding relationships.

For the sake of fairness I should point out that the engagement of undergraduates in ethnographic research has had its problems, and there is an ongoing, though now largely muted, debate within the discipline concerning the advisability of having undergraduates participating in fieldwork (see Ingold 1991; Middleton 2018; Piot 2016; Sharma 1989 and 1991; Sharma and Wright 1989; Thorn and Wright 1990; Watson 1995). Many social and cultural anthropology programs in Britain and America in the 1980s and 1990s embraced undergraduate fieldwork as a core curricular component, but this trend met with loud resistance from some quarters. The chief objections to undergraduate fieldwork are that the positive benefits are limited and do not outweigh the harm that can be caused by inexperienced researchers both to themselves and to those they are working with. Without getting knee-deep into this debate, I would simply say that adequate advanced preparation and constant supervision by trained faculty mitigate the potential dangers that can arise and, in my experience, the value of undergraduate projects has been immense. If it were not, I would not be writing this work. Not least, I believe that training in field research methods, contrary to some critiques, is a solid marketable skill upon graduation, and many of my students have been hired from college by research firms as fieldworkers.

(Anti-)Objectivism in Anthropology

Caring deeply about people with whom one is engaged as a fieldworker makes it completely impossible to record and interpret their behavior in objective fashion. The critique of objectivity in anthropology was well under way at the time that the essays in Hymes's collection *Reinventing Anthropology* (Hymes 1972b) were being developed, and the last two sections of this anthology focus on several different facets of this critique (see especially Jay 1972; Scholte 1972; and Diamond 1972). Since then, the forces of philosophical opposition to objectivity in anthropology have shaped into well-known main currents in the discipline, such as the movements toward reflexivity and humanism (see, e.g., Clifford and Marcus 1986; Dwyer 1999; Geertz 1990; Kumoll 2010; Marcus 1998; Richardson 1990; Scotford 2012).

The critique of objectivity in the social sciences has taken many forms. What I would like to examine here, though, is less the notion that the materials of life history—as represented in this volume—are *unobjective*, or incapable of being made objective, than the idea that they are quite deliberately *anti-objective*: they stand in conscious *opposition* to objectifiable data. No doubt parts of life histories may be objectified when it comes to certain kinds of facts, but that is not their point. Personal meanings are inscribed deeply within them, and these are not matters of objectifiable fact at all but, rather, what certain incidents or events *mean* to the individual in question (see Linde 1993; Rosenwald and Ochberg 1992). Such meanings cannot, by their very nature, be made “objective,” and any desire to do so is misguided or muddled.

What I am given to wonder, however, is whether this anti-objective approach to field data does not always require some kind of conversion experience mediated by people in the field. Robert Jay hints at such a possibility (Jay 1972), and the experiences of my students, plus my own time doing field research, seem to support such a claim. I have reported at greater length elsewhere how my friends in the field in Tidewater, North Carolina, helped me move away from attempting to objectify the data that constituted their daily lives, but it was a hard-fought battle for a while (Forrest 1988; and Forrest and Blincoe 1995).

The following exchange took place in my very first interview in the town where I conducted my doctoral fieldwork in 1978. I was sitting in FI's² living room, and she had a quilt on her lap that was a prized family heirloom. Because I was eagerly intent on getting the facts I asked:

JF: So when was it made?

FI: Well let me see . . . it was made by Lizzy Brown, my daddy's granddaddy's second wife when they were first married. I think he was around 42 or 3 at the time. Now he died when I was five and he was 93, and I was born in 1902, so you work it out.

JF: OK, He must have died in 1907 at the age of 93, so he was born in 1814, which would have made him 42 in 1856. That means it's about 120 years old.

FI: Uh-huh.

It took me well over six years of listening to this and other interviews again and again before the penny dropped, and I realized the degree to which FI and I were pursuing radically different agendas. It is perfectly possible to objectify certain facts about the quilt, such as its date of manufacture (or its age), but such objective facts are outside FI's value sys-

tem in relation to it. Two responses in FI's discourse make this clear. The first is the command, "so you work it out," which could be expanded to something of the following order (which she would never have dreamed of saying directly to anyone): "I will tell you what I know about the quilt's chronology in terms that matter to me, or are significant to me, and you can convert them into your value system if you want to—I don't want to and am not going to." Having then finished that part of our conversation, and being proud of my mental skills, and, indeed, beginning to see the "value" of the quilt in *my* terms (i.e., "my gosh, 120 years old"), FI brushed aside my interest with the noncommittal "Uh-huh," which stops short of being impolite but could be translated as "if you say so, but so what?" Her value terms in relation to the quilt are:

1. the maker's *name*
2. her *affinal* relationship to the maker
3. her *consanguineal* relationship to the maker's husband
4. chronological information associated with rites of passage
 - a. the maker's husband's age at *marriage*
 - b. the maker's husband's age at *death*
 - c. FI's age at the maker's husband's *death*
 - d. FI's year of *birth*

From this we may further learn that in this community, affines get *named* when discussing them, but consanguineal kin are referred to almost exclusively by *kin terms*. Furthermore, even though an affine made the quilt, its history is linked to the chronological details of a blood relative. Having made this discovery, I could then to go back to other interviews with FI and note that she always said, "my daddy made . . ." or "my mama did . . ." but never "my husband worked . . ."; always "Lem worked . . ." (and also important to see that both the kin terms and the names used are affectionate diminutives).

I was too far removed from the value system of the community at the time of these interviews to be able to be instructed in anything other than a kind of objectified way. It was not just a simple matter of me being a foreigner geographically (born in Argentina, raised in Australia and England); I was a cognitive stranger as well. The sad truth is that I would have related to them in their own terms a lot quicker than I did if I had never studied anthropology, because the discipline had inclined me to *study* them rather than to *learn* from them. They tried their best to include me in their value system, but my training resolutely resisted their efforts. I get it now—too late, of course. What is critically important to understand, though, is that no matter how bad a pupil I was, our

interviews always had an instructional purpose, and that what I thought I was trying to discover was less important in the minds of the people in the community than what they thought I ought to know.

This last point brings us directly to consider the nature and purpose of life histories as social documents. A life history, meaning a particular subset or species of oral history—a personal narrative recounted orally to someone else—is, strictly, neither autobiography nor biography. It is autobiographical in the sense that it is a person's self-construction (and derives meaning and power from this fact). But it is told *to* someone and not simply created as a general work for universal consumption. Often the narrative would not exist in any preservable form were it not for the person that it is told to; and who that person is vitally important in the construction of the narrative.

If the person recording the life history acts as a representative of a generalized “other” (that is, the tale teller sees the listener as an outsider), then the tale may well be relatively “objectified” or neutral, in both its overall construction and in specific details (inasmuch as this is possible). Much of my early fieldwork in life histories in Tidewater, North Carolina, was of this sort. My “other”-ness often forced the discourse into modes of communicating that my interviewees thought I would understand or were what I wanted. And, the kinds of questions I asked, and the kinds of responses I gave to their narratives, reinforced their (largely unconscious) moves in that direction.

If the teller has some direct and obvious affinity for, or some clear cultural connection with, the fieldworker, however, the life story may well take on more personalized meanings. The fieldworker may be co-opted as a kind of apprentice or novice, for example, to be taught the powerful components of the teller's life—and to pass them on in turn. The samples represented in this work are of this type, although the variations on the basic theme are myriad and of extreme importance in understanding the material so produced.

In addition, the fieldworker has the capacity to shape the narrative in the same way that a biographer would, but is shaping the direct transcription of the words of the teller rather than the more comprehensively objectifiable components of data that go to make up a conventional biography. The fieldworker is channeled and directed (perhaps even limited) by the interviewee's words and constructions. So, the fieldworker may use a number of editorial devices and decisions to augment or amplify the oral text, but he or she almost invariably uses the patterns and meanings evoked by the narrator as foundational (whereas the classic biographer, as essentially *external* to the data—i.e., “objective” observer—characteristically sees the job of biography as finding

or creating the pattern and meaning in the materials—or, perhaps of weighing alternative patterns). As such, life history, unlike biography, is a *collaboration* between narrator and fieldworker.

In addition, it is vitally important to remember that life histories, while written and edited, are still essentially *oral* documents. Such oral documents have a long history. The oldest known so-called autobiography in English, for example—*The Book of Margery Kempe* (Windeatt 1985)—is really an oral life history, since Margery Kempe could not read or write, and, therefore, had to enlist the aid of a lettered priest to record and edit her oral narrative sometime around 1436. This text stands as a good example of the type in that it reveals several qualities that are unique to, and diagnostic of, the oral life history. These are elaborated more fully in the following section, but, in brief, they include the twin features of *authentic voice* and *personally constructed meaning*. A life history is never simply a neutral description of a life course (even when recounted to a completely indifferent transcriber) but always contains critically personalized elements.

The way in which a narrator constructs meaning in a life history is strongly influenced by the relationship between the teller and listener, and within this collaboration, the issue of who has initiated the narration (teller or listener) has a profound impact on the structure of meaning within the narrative. Conventionally within the social sciences, it is the fieldworker who initiates the sessions because of a desire to gather information. But, as in the case of Margery Kempe, some life histories have an inner compulsion built into them by their narrators—an Ancient Mariner effect, as it were—that governs their structure and meaning. The tale teller feels constantly obligated to seek out listeners and retell the life tale. Yet, whether the act of recounting a life history is initiated by teller or listener, these texts are rarely, if ever, records of the objective facts of a person's life: they serve specific personal purposes.

It is because life histories are recounted in personalized terms—and not in spite of this fact—that they are critical documents for social scientists. They record lived experience in a singular manner. It is precisely their lack of objectivity that makes them so important: they are testimonies of the subjective felt sense of experience in a particular period—the essence of what German social scientists, such as Wilhelm Dilthey and Max Weber, called *Verstehen*, “empathic insight,” emanating from the actors of history themselves.

Dilthey places a high value on historical documents that exhibit subjective *understanding* rather than objectified *knowledge*, and among these he sees autobiography as key cultural data from which to generate theory:

Autobiography is the highest and most instructive form in which the understanding [*Verstehen*] of life confronts us. Here is the outward phenomenal course of a life which forms the basis for understanding what has produced it within a certain environment. The man who understands it is the same as the one who created it. A particular intimacy of understanding results from this. The person who seeks the connecting threads in the history of his life has already, from different points of view, created a coherence in that life which he is now putting into words. He has created it by experiencing values and realizing purposes in his life . . . (Dilthey 1961: 85–86)

But the autobiography that Dilthey so values is of the literary type: Augustine, Rousseau, and Goethe. Oral life history, because it exists within oral tradition, goes several steps further than these in that it is both *immediate* and *democratic*. The autobiographies that Dilthey analyzes are all literary forms; meticulously crafted products of highly educated minds, bent on certain kinds of reflexive thought. They are composed by being written, and are written in order to be read. Oral life histories are crafted and reflexive also, it is true, but they are destined to be told and retold—bearing all of the hallmarks of a folk form versus a literary one (the delimiting points of these two types being the sum and substance of folklore scholarship).

Thus, the oral life history has a degree of spontaneity and malleability (absent from the literary autobiography) because it is generated afresh each time it is told. Furthermore, it is democratic in the sense that it requires neither literary training nor access to publishing resources (nor an identifiable capital market) to disseminate it—coupled with the inherent limitation of all oral forms, namely, its dissemination must occupy a meager compass.

To some extent the invention of the tape and digital recorder (along with the development of mass-produced, cheap equipment), has cut into previous limitations of the oral life history, making the ephemeral potentially permanent and more widely distributable. But this fact brings another person into the picture: the fieldworker. The fieldworker acts as the broker between the world of the narrator and a wider audience by recording, transcribing, and editing the life history; and to a great degree, the voice recorder has also democratized this process. Prior to the invention of cheap, portable tape recorders that could record several hours of material at a time (with occasional pauses to change tapes), one would have had to have been an extremely facile stenographer to record a life history verbatim while allowing the narrator to talk in a natural manner. Such skills are not easily acquired, and the use of them would have fundamentally narrowed the potential

for personal interaction between teller and recorder—unless the steno were the human equivalent of a tape recorder hired by the fieldworker, at considerable expense.

That life histories, as subjectively legitimate understandings of life and culture, are fundamentally valuable social documents is becoming increasingly evident in anthropology and the social sciences in general. Certain of these documents—e.g., *Black Elk Speaks* (Neihardt 1932), *Crashing Thunder* (Radin 1920), *Sun Chief* (Simmons 1942), *Son of Old Man Hat* (Dyk 1938)—have already achieved a kind of classic status within the discipline. But there are also many more that are being solicited in order to broaden and deepen our understanding of social systems from the individual's point of view (see, e.g., Behar 1993; Crane 1987 and 1999; Freeman 1979; Hurston 2018; Rosengarten 1974; Rosenwald and Ochberg 1992).

Beyond the employment of life histories in social science, there is also developing a body of theoretical insight into the nature of oral life histories themselves (Angrosino 1989; Behar, 1990; Bertaux 1981; Chamberlayne et al. 2000; Jolly 2001; Hertz 1997; Linde 1993; Ochs and Capps 1996; Peacock and Holland 1993; Riessman 1990; Urban 1989), as a significant component of the general interest in reflexivity, metacommunication, and meaning that has evolved out of the post-modern critique of the fundamental materials of social science (Berg 2006; Clifford and Marcus 1986; Davies 1999; Ellis and Bochner 1996; Goodall 2000; Sanjek 1990). As part of this general reevaluation of the agenda of social enquiry, attention is being devoted to the very methods of collecting and transcribing life histories as a concern related to, yet also significantly distinct from, general interview technique (see, e.g., Crane and Angrosino 1984: 75–87; Crapanzano 1984; Forrest 2022; Ives 1974; Langness 1965; Langness and Frank 1981; Mishler 1991 and 1995; Riessman 1993).

What still needs to be better explored is the nature of the individual and subjective *relationship* between fieldworker and life history narrator, and how a life history emerges out of such a partnership: that is, the prime subject of the present work (see, for example, Casagrande 1960; Kratz 2001; Riessman 1987 and 1993; Shostak 1981). The bond between interlocutor and fieldworker is delicate yet strong; a bond that we have all experienced but not always adequately analyzed or understood. It is the cornerstone of everything we do in the field and everything we become in our disciplines; it limits, colors, and shades our insights; it empowers us. Such empowerment can and should be mutual and reciprocal.

Reciprocal Empowerment

It is easy for a fieldworker to feel enriched by the life stories of key interlocuters; to see them as a gift or as something to be taken away in a personal economy of subtraction. You had a life story to give, and you gave it to me—it is now mine. This trope of narrative as (precious) commodity, albeit a commodity that may be refashioned and given out anew, seems to me to miss the point of life history telling altogether. *Giving* may be an important component of the act, but the narrator may be *receiving* a great deal as well.

The strength of the life history method is that it allows us to build historical, or ethnographic, or biographical images from multiple subjective points of view, and such images are inherently more faceted than an analysis that seeks the objective truth from the single point of view of the academic observer. As such, we may tease out certain kinds of motivations that underlie the telling of a life history, each of which *empowers* the narrator in some obvious way.

By definition, a life history is *personal* history. Sometimes life histories relate directly to events that the academic or popular worlds deem “important”—the Holocaust, say, from the perspective of a concentration camp survivor. But regardless of the “importance” of the broader framework of the narrative, the stock in trade of the narrative is the personal detail. The importance of the details is governed strictly by their personal value within the meaning system of the narrator. Thus, an entire generation of US citizens asks, “What were you doing when you heard the news of Kennedy’s assassination?” They do not ask, “Did it matter?” or, “Were you upset?” Not only are the answers to these questions obvious, but they are also implicit in the first question. What the answers to the first question reveal is the kaleidoscopically rich world that the event burst in upon. And the fact that everyone can give an immediate (well-structured) answer is adequate testimony to the power of the event—in microscopically personal terms. It is banal to assert *that* the event was important, or even to suggest reasons *why* it was important; life history mines the riches of *how* it was important, one person’s experience at a time.

Similarly, we may see life history as *alternate* cultural viewpoint. Typically, it does not present the meanings of events in terms of an elite or hegemonic frame of reference—although this state of affairs is greatly colored by the status of the narrator (see Frisch 1990). Inasmuch as such meanings are habitually couched in personal terms, they may modify, augment, or even flatly contradict prevailing interpretations in the

academy (or in popular consciousness). This is not to suggest that life history is somehow “better” or more “real” than conventional academic analysis but only that it can provide a different lens through which to perceive the world.

Within the domain of life history as personal or alternate point of view, we may perceive certain general motives for the telling (and recording) of oral narratives, and, again, fieldworkers are active agents here insofar as they act as both immediate audience and as mediators to wider audiences. The two most obvious, and more or less related, motivations for the telling of life history narratives are as part of personal *therapy*, and as general *testimony*. These are linked to a third possibility, namely, life history as *exhortation* or *social instruction*.

The “talking cure” is a well-known component of Freudian, and derivative, modalities of psychotherapy. Perhaps less well-known is the particular use to which life histories are put within “twelve-step” programs—initiated by the founders of Alcoholics Anonymous (AA). One of Bill W.’s great insights into the recovery process was that alcoholics do not like to be lectured to concerning their problems or told what to do. What really got through to them was hearing *someone else’s* story of decline and revival. The telling of the alcoholic’s life history is a two-way therapeutic process, however, helping the listener and the teller. The teller needs an audience for his/her story, but he/she is really telling it reflexively to himself/herself as a form of reinforcement of the rightness of his/her actions. The (alcoholic) listener vicariously identifies with components of the teller’s story and is able to use it as a model without feeling directly instructed.

What began as an almost accidental discovery became quickly integrated into the formal structure of AA. At every meeting, attendees relate fragments of their life history as part of the group process, but each is expected at some point in recovery—usually well into it—to present a formal life history narrative to the group. These follow a well-worn path: how I started drinking, the slippery slope, hitting bottom, finding AA, and so on. But for all the formulas, each tale is unique and rich in personal detail, and what is clear from hearing a number of such narratives is that their purpose is to frame a life in such a way that its linear structure reveals the more global meaning latent within it. Furthermore, the AA type of life history allows for a realignment of meaning. Wrecking a car, losing a job, losing a family, etc., are the kinds of life events that are generally categorized as “bad” in Western culture, but the AA brand of life history frequently reassesses such events as “good,” inasmuch as they are the components of hitting bottom where recovery can begin.³

Outside the structures of formal modes of recovery and analysis, the life history may perform similar therapeutic functions to those within them, although the process may not be as self-conscious or as formulaic. That is, what AA does as part of its system may be achieved by fieldworkers who provide both the necessary audience and the overarching framework within which narrators may work through their life problems by discovering latent meaning. It is important to understand, though, that the fieldworker is not a therapist, and a professional therapeutic relationship is not a desired outcome of the method, or one to be encouraged. But some components of the traditional “talking cure” may emerge spontaneously and unconsciously as part of the process of gathering a life history.

Related to this power of life history as a therapeutic tool is the notion that telling one’s story may serve as *testimony* (see, e.g., Peacock and Tyson 1989, Titon 1988 for extended accounts). Obviously, the commonest context of such narratives is Christian evangelism, within which the structure and vocabulary may be as stylized and formulaic as within AA. But the motivations for telling a life history as testimony are importantly different from the therapeutic. In particular, the talking cure is fundamentally reflexive, whereas a testimonial usage serves essentially to send a message outward. Certain kinds of events—such as the public life histories of AA—may share both purposes, and one might argue that all life histories participate in both to some degree. Thus, therapy versus testimony is not so much a dichotomy as different ways to perceive a complex whole.

As already implied, such narratives may also serve the function of indirectly exhorting listeners to action of a certain sort. This is as true of AA narratives as of evangelical testimony. The teller may be relating a very particular life course, but the purpose is more than the simple passing on or dissemination of information. The teller is also implicitly saying “this is the life path for you too if you are in similar circumstances.” Such life histories, thus, serve as *exemplars* that instruct.

Complementing the notion that life histories are personal narratives with personal motivations is the simple fact that they are told in a uniquely personal *voice*. The preservation and admiration of such distinct voices—often characterized as inferior by hegemonic institutions—are critical ways in which the fieldworker may empower an individual. Countless commentators have extolled the power of narrative voice and have shown over and over again that “ways of saying” contribute mightily to the meaning and reception of literature. Convincingly simple in this regard is George Orwell’s paraphrase of a famous passage in Ecclesiastes—“I returned, and saw under the sun, that the race is not to the swift, nor the battle to the

strong, neither yet bread to the wise, nor yet riches to men of understanding, nor yet favour to men of skill; but time and chance happeneth to them all”—from the standard to the voice of a modern academic: “Objective consideration of contemporary phenomena compels the conclusion that success or failure in competitive activities exhibits no tendency to be commensurate with innate capacity, but that a considerable element of the unpredictable must invariably be taken into account” (Orwell 1946).” Likewise, the ethnopoetics and sociolinguistics agendas advanced by Dell Hymes and others, have repeatedly indicated the poetic and stylistic complexities of oral texts—all of which may be lost if a narrative is not transcribed verbatim (see, for example, Bauman and Sherzer 1974; Giglioli 1972; Gumperz and Hymes 1972; Hymes 1974 and 1981). It is superfluous to rehearse the principles of these schools of thought here. It is important to say, though, that even neophyte fieldworkers are capable of appreciating the complexities and subtleties of authentic voice, and by using even relatively rudimentary recording equipment, they may create verbatim transcriptions that maintain many of the essentials of that voice. A fieldworker’s most basic service to the narrator, therefore, is to empower that voice by valuing it, and preserving it intact.

The central issue here is that a life history is not the same kind of narrative as a literary autobiography, nor is it simply a justification or exposition of a particular life course. Many of its meanings emerge because of the voice in which it is told. As with all linguistic phenomena, meanings and values are expressed on many different levels—cognitive, affective, aesthetic, and so forth. The fieldworker’s principal task here is to transcribe the recorded material in such a way that the written material is fluently readable, yet the essential qualities of the narrator’s voice are maintained for the average reader to pick out (see Frisch 1996).

Such a rehearsal, which is admittedly no more than a series of signposts to the general academic literature, serves to outline some ways in which life histories advance the needs and interests of narrators. What has yet to be fully acknowledged is the degree to which the method empowers the fieldworker. We all understand at some level that anthropologists may become powerful in their own circles by being in command of powerful data, but it is an actively debated question whether the power of the data derives more from the ability of the fieldworker to garner (and construe) it or from its intrinsically powerful properties to begin with. Again, such straightforward dichotomizing is potentially unrewarding in that all fieldwork when seen as collaboration is empowered by all parties to its practice, and if any imbalance needs to be redressed, it is in granting greater significance to the role of the storyteller in furthering the agendas of the fieldworker.

The picture is rather different for undergraduate fieldworkers from that of postgraduate researchers and other professionals in the field, however. It is my general experience that undergraduates, because they are not normally driven by personal desire for professional advancement in the field or by the correlate need to attack or defend high-profile theoretical stances, have a greater (and perhaps more humane) desire to *identify* with their interviewees rather than to *distance* themselves from them; or, in my terms above, to *learn* from them rather than to *study* them. Such identification is the cornerstone of their empowerment by their fieldwork partners.⁴

All of the life history segments presented in this volume are clear examples of the strong identification of the student fieldworkers with their interlocuters because of some shared social or cultural value, and in almost every case the fieldworkers were inspired to accomplish the work because of vicarious pride in the achievements of the people they interviewed and a desire to emulate them in their life course. Generally, this means that fieldworker and interviewee are perceived by the larger culture as co-members of negatively stereotyped social categories—welfare mother, gay man, poor immigrant, disabled worker, African American slum dweller—and, therefore, the process of recording each life history becomes one of collaboration to erect models of the members of those categories that counteract the negative stereotyping.

Some kind of vicarious identification with one's interlocuters may be the norm, certainly commonplace, for anthropological fieldworkers. But in the case of these undergraduates the identification is scarcely vicarious, and there is less a sense of the fieldworker looking at another culture through the eyes of one of these "other" people and more a sense of direct bonding between a senior and a junior member of the *same* social group.

The process of taking down the life history, thus, may become a form of cultural instruction for the fieldworker, learning the strength and power of the positive components of their shared social experience and also learning appropriate symbols of pride, revised social history, and so on. As such, this kind of fieldwork is really kin to traditional modes of learning rather than an intrusion into them; and where traditional transmission of culture has been obstructed or disrupted somehow, fieldwork may function as an agent of cultural (re)construction.

Such cultural learning is clearly marked in Michael Avrut's piece (chapter 4), for example, indicating a timeless dilemma for gays and lesbians; namely, that, unlike almost all other social groups, they cannot learn appropriate in-group behavior from their families—the crucible of culture for virtually everyone else—because their families are not gay

or lesbian (except under rare circumstances). They must acquire their culture elsewhere, and at a more advanced age than other children (see Blincoe and Forrest 1993).

When a life history interview replicates traditional modes of learning from elders, certain inherent problems of the power relationship between fieldworker and interviewee are automatically diminished, allowing for a greater reciprocity between the two. In classic fieldwork situations of the early twentieth century, the fieldworker came from a powerful culture, with all the tools and paraphernalia of that culture, and came to a culture that was commonly economically and politically subordinate to the fieldworker's culture. Power relations in that situation were obviously asymmetric, and it was hard, if not impossible, to create a balance. But such is not the case with my students. They are working within their own cultures (in the role of younger members of those cultures), and they are coming more obviously in the role of learners to be instructed than as representatives of a dominant culture. Thus, interviewees can perceive their own roles as being more powerful than in more traditional fieldwork settings because they are instructing junior members in the ways of their own culture, as well as possibly helping them to succeed in the hegemonic world of the university. Proof of the latter feeling comes when, as is commonplace, the interviewee wants to know if the student fieldworker got a good grade for the project.

A vital element of this kind of cultural instruction, as outlined above, is the use of authentic voice. But it is not just a matter of the narrator using or even teaching the fieldworker a way of speaking by example. Even within the most liberal of educational institutions, certain ways of speaking are classified as substandard (i.e., “ungrammatical” or “illiterate” or “wrong”) when used as a written form of English. So there remains the implicit message that, while certain ways of *speaking* may have in-group power, the forces of hegemony still control the *written* word. Transcription of authentic voices of narrators allows an escape route from this trap. Transcription turns ways of speaking into ways of writing, allowing authentic voice to exist as a literary form, and allowing both interviewee *and* fieldworker to share in the power of that voice as a written form.

Specific Methods

All of the life histories presented here grew out of work for a single course, *Fieldwork: Qualitative Methods*, which is a core component in the curriculum for undergraduate social sciences majors at Purchase Col-

lege, SUNY. It is a requirement for all anthropology majors, and for those majoring in the interdisciplinary social sciences and the arts program. These programs are committed to teaching fieldwork methods to undergraduates on the grounds that concentrated, hands-on experience in gathering data—along with collateral activities, such as, preparing a research design, professional data presentation and the like—provides students with a better understanding of the empirical aspects of the social sciences and of the process of generating theory from field materials than a comparable period of time spent in the library. The course may also satisfy requirements in sociology, and a few stray students from photography and film making ask to join now and again.

The course as I originally organized it was divided into seven discrete projects; or, roughly one every two weeks over a single semester. In brief these were as follows:

1. *Self study*: An essay in which the students analyze their motives for wanting to engage in fieldwork and assess their strengths and weaknesses.
2. *Proxemics*: One or two hour-long periods of observation in public spaces in which the students document how people use spatial relations (between each other, or between themselves and objects in the environment) in socially meaningful ways.
3. *Mapping*: Students draw several scale maps that demonstrate social aspects of space.
4. *Process documentation*: Students each pick a skilled artisan, and through a combination of observation and interviewing, document a skilled process in its sociocultural, psychological, or personal contexts. Photography or videography may be used here.
5. *Participant observation*: Students each choose a public function (ritual, ceremony, entertainment) that is unfamiliar to them, at which they act as both active participants and observers.
6. *Sensory documentation*: Students either conduct a second participant observation study or document some other activity, focusing on only *one* of the senses (and they must choose one other than sight).
7. *Life history*: Students conduct two one-hour taped interview sessions (one directed the other undirected), concerning a selected person's life history. The students then select portions for detailed transcription.

This course has several general objectives. It is designed to sensitize students to different methods, which in larger research projects may

be used together, but which in this course are treated as discrete units so that the students can appreciate the merits and functions of each in isolation. The course is also structured to lead the students slowly into deeper and deeper methods, beginning with those that require observation at a distance and concluding with those that are more intimate or intrusive. There is also a sense in which the later projects build on the skills of the earlier (participant observation, for example, may incorporate proxemics or mapping).

As I taught it, each two week unit was divided into five parts:

1. *Description*: Students prepare by reading a chapter in a methods text. I described each method in detail, and indicated my expectations for the project.
2. *Practicum*: I set up an experimental situation in the classroom that the students can use as a practice session to learn the pitfalls of each method before embarking on their own projects. This practicum could take several forms: sometimes I showed videotapes of other students at work on similar projects, or I used myself as a guinea pig. The idea was for the situation to be controllable and open to constant reflexive commentary.
3. *Discussion*: Students brought problems to class that they had encountered in designing or carrying out their individual projects, for group discussion and resolution. We could also discuss problems encountered in the practicum session if they were germane.
4. *Presentation*: Two or three students presented their projects to the entire class (on a rotating basis, so that everyone presented at least once), for peer critique.
5. *Critique*: After reviewing the class's work as a whole for each project I gave a summary critique. In conjunction with this exercise the students read selected sections of each other's finished projects.

Thus, the life history project may be seen as the capstone of an extremely intense program of activities, and for many of the students in the class this project marks the turning point in their understanding of social investigation. My directives to them for the project are relatively simple:

- find someone to recount part of a life history
- record a one hour undirected narrative
- record a one hour directed narrative
- transcribe about 45 minutes of narrative

Working through the various projects over the course of the semester supposedly prepares the students for this kind of intensive one-to-one activity, but they are, nonetheless, frequently intimidated by the scope and the intimacy of the life history method.

Finding the right narrator is easy for some, next to impossible for others. From the very beginning of the semester, when I review the sweep of the course, students are aware that the life history is lurking on the horizon, and many ponder deeply from the outset whom to choose. Others find the right person in the course of other projects (the process documentation, for example), or stumble on an appropriate choice, having been sensitized to the power of the method in the class (samples of previous projects are available for them to review). Yet others come to the class already aware of potential narrators, having been warned by more advanced students at the college of what lies ahead for them.

I request that they do two interviews for a number of reasons. Not least is the lesson of redundancy: two interviews act as an insurance policy against the failure of one (recorder malfunctions, broken appointments, unexpected interruptions). But, the primary methodological purpose is to have them understand the importance of listening intently and reflecting on what they have heard. Thus, the first interview is expressly aimed at encouraging them to keep their mouths shut and their ears open. I ask them to initiate the interview with the broadest of openings; such as, “tell me about your personal history,” or the like, and to remain silent subsequently, unless silence would be uncomfortably awkward, or in some other way socially inappropriate (I do allow them to use general phatic utterances to keep a sense of connectedness).

The next step is to listen to the recording—several times if possible. The goals here are many: to index the recording, to review technique, and to get a deeper sense of the substance of the narrative—voice, content, structure, and so forth. Not uncommonly, this act of listening is an eye opener. Students find out that while they believed they were as silent as tombs during the interviews, the recorder tells them otherwise; or they discover that they have completely misunderstood whole chunks of the narrative, or failed to hear key passages at the time of the interview. Armed with these insights they pick a passage of the first interview that they feel could use explication or elaboration and return for a second—still trying to keep in the background, but also attempting to keep the narrative within certain predetermined bounds.

As with the first recording, I expect them to listen to the second with the same general goals in mind. The point is to help them understand that the method is endlessly dialectical—listening poses questions, and the responses to these questions pose new questions—and

that, therefore, listening is far from a passive act. They may then begin transcribing.

I ask them to pick two 20 minute segments (ideally one from each tape) for verbatim transcription, basing their decision on their own interest in the content of those segments. I recommend that they use one of two related transcription methods, namely, that they use no punctuation, but, rather, listen for the pauses in the narrator's speech and indicate them by a dash or by starting a new line at every pause. Part of my purpose here is to keep the process as simple as possible, but it is also to get them to concentrate on the strictly oral voice and its natural rhythms—without the added complications and distraction of having to translate these voices into a literary form.

Even without any concern for the potentially Byzantine convolutions of advanced transcription techniques, the students are always amazed at what a grind it is to get an interview down on paper. They average a transcription ratio of around 9:1, that is, one hour of tape takes about nine hours to transcribe. Yet, despite the dogged effort involved, it is often during this process that much of what I have been trying to stress over the course of the semester falls into place. Verbatim transcription is itself a form of intense and detailed listening: *really listening*. They hear the same phrases over and over, strain to catch nuances of diction, puzzle over unusual terms and turns of phrase, anguish over pauses and phrase length, so that by the end of it all they know their transcription better than any other text they have encountered in their education.

Their pride of accomplishment is outstripped, however, by the insight they have gained into the nature of data collection and into what is required of a good listener. These are precisely the kinds of life skills that training in anthropological method can teach anyone, and which can be turned to good usage in all walks of life. That is, Hymes' ambitions for anthropology in the larger world might well most consistently be achieved by teaching field methods as intensively as theory.

Study Questions

1. This chapter mentions the difference between *studying* a group of people and *learning* from them as a fieldwork method? What do you think the difference is? Are there different benefits and pitfalls to each method?
2. How is the notion of studying people versus learning from them related to the idea that education can be a form of sharing rather than instructing?

3. What are your career goals at this point? How do you imagine training in oral history and fieldwork will enrich that career—if at all?

Notes

1. My son and I are working on it.
2. FI is her code name in my system of note taking. All interlocutors' names in this work are codes or pseudonyms
3. See Bill Wilson's *Alcoholics Anonymous* for further details on life history as a method within AA.
4. To some extent my goals here fall under the general aims of the pedagogic theory commonly called "action research" (see Berg 2006, Kemmis and McTaggart 1988, McCutcheon and Jurg 1990, and McKernan 1991). There is also an element of constructivist learning theory involved (see Brooks and Brooks 1993, Crotty 1998, Kukla 2000, and Prawat and Floden 1994) which has a venerable history from Vygotsky and Piaget to Gregory Bateson and beyond.