

1

Introduction

Part I A Brief History of Fieldwork

Why Fieldwork?

Ethnographic fieldwork is the hallmark research approach of sociocultural anthropology. Its centrality has not waned since its inception more than a century ago, yet the variety of questions that fieldwork answers has expanded greatly. For instance, anthropologist Olga Lidia Olivia Hernandez studies Aztec dance collectives in multiple sites in Baja California, Mexico; and California, USA. She conducts fieldwork to understand why Aztec dance emerged as a form of ethnicity on the US-Mexico border among non-indigenous participants, and how national, political, religious, and bodily processes are involved in the reappropriation of Aztec dancing (Olivas 2018). Taking a more multidisciplinary approach in her fieldwork among Orangutan care workers in Borneo, anthropologist Juno Salazar Parreñas draws on anthropology, primatology, Southeast Asian history, gender studies, queer theory, and science and technology studies. She explores the violence care workers and Orangutans experience. She asks if conservation biology can turn away from violent techniques to ensure Orangutan population growth and embrace a feminist sense of welfare (Parreñas 2018). Anthony Kwame Harrison conducts fieldwork in San Francisco among the underground hip-hop scene. Harrison interviewed area hip-hop artists and also performed as the emcee “Mad Squirrel.” His immersion in the subculture allowed him a unique vantage point to examine the changing nature of race among young North Americans, as well as issues of ethnic and racial identification, and how different ethnic groups engage hip-hop in different ways as a means to claim racial and establish subcultural authenticity (Harrison 2009).

Fieldwork is an extraordinarily flexible and expansive methodology, allowing researchers to ask challenging questions and uncover deep, nuanced, and contextualized answers that are rarely self-evident. The purpose of this book is to guide you step by step as you learn ethnographic techniques. Ultimately, I hope you will use them to answer the types of questions you are most passionate about. However, in order to gain competence in ethnographic fieldwork techniques, it is important to understand what ethnographic fieldwork is, what makes it special, and how it evolved into the preeminent research approach in cultural anthropology

Armchair Anthropology

Before ethnographic fieldwork became the well-established and rigorous research tool it is today, it went through a number of significant changes. In the nineteenth century, a very few (primarily North American) anthropologists traveled away from the comforts of their hometowns to study “others,” while the majority of them preferred to stay at home and gather their data by consulting the records of travelers instead. By the start of the twentieth century, the balance had shifted, with the great majority venturing out from their homes. At that time (and later), the seemingly self-evident divide between sociology and anthropology was that sociologists studied “us,” whereas anthropologists studied “them.” Otherwise, both disciplines were interested in how societies/cultures worked.

In the nineteenth century, influential anthropologists such as E.B. Tylor, and James George Frazer developed highly generalized analyses of cultures. They tended to view small scale societies as if they were isolated from one another and from the impact of global forces, such as colonialism, which most certainly had a major impact on their internal social structures and belief systems, yet were largely ignored. These anthropologists, who dominated the field in Europe, did not conduct fieldwork at all, but, instead, sought cross-cultural information from the seclusion of their library armchairs. They drew their grand conclusions about the evolution of culture around the world from diverse sources without any systematic concern for the reliability of the materials they used nor the context in which they were written. Their goal was to show that under the incredible diversity of cultural practices there was a bedrock unity.

A classic example of this approach is Frazer’s multivolume *Golden Bough* (Frazer 1890), which was one of the towering centerpieces of anthropological theorizing in the nineteenth century, well into the twentieth. He took data from any and all available sources – travelers’ journals, newspaper articles, historical archives, etc. – with no clear assessment of the truth or validity of the information, nor was he concerned with contextualizing the data culturally because overarching theorizing about religion worldwide was the ultimate goal. Thus, finding common patterns globally took precedence over the specialized analysis of fine-grained cultural details.

The weakness of Frazer’s method is that without cultural context, cross-cultural comparison of specific symbols is meaningless. For example, take the image of a snake. What a snake means in different cultures varies tremendously, in no small part because snakes are themselves enormously varied, both within geographic regions and between them. Some snakes are small and harmless, some are large and terrifying, some are fatally poisonous, some have domestic uses, and so on. When snakes are a main component of tales globally they can take on myriad meanings. It is simply impossible to gather together all the tales and images of snakes worldwide and meld them into a unified theory of the meaning of snakes to humans. They are forces for good or for evil, protectors or destroyers, creators or demolishers, healers or killers, wise teachers or treacherous betrayers, etc. etc. But if you gather together a group of tales from around the world without paying attention to such cultural details, as Frazer did, you can weave fantastical theories concerning the meaning of the snake in human culture. Ethnographic fieldwork that paid attention to fine-grained details and cultural context put an end to that kind of theorizing.

At the turn of the twentieth century, scholars such as Émile Durkheim and Max Weber were interested in how modern European cultures functioned and evolved, but they did not limit themselves to that sphere alone. They wrote about Pacific Rim cultures at great length as well, including indigenous Australian and traditional Chinese and Indian religious beliefs and practices. Both assumed that, at some level, all humans operated according to certain fundamental, discoverable principles. In other words, they were searching for human universals, rather than exploring what made people diverse. Like anthropologists of the nineteenth century, their data were second-hand. They did no fieldwork for themselves. As social science matured in the early twentieth century the split between sociology and anthropology opened up, with anthropologists carving out intensive, long-term fieldwork projects with peoples well outside of Europe and North America as their domain – in large part, supported by, and promoted by, colonialist governments for their own ends. In this context, getting out of their arm-chairs and getting to grips with the pragmatics of rigorous, “scientific” (that is, grounded and verifiable) fieldwork in foreign places was a major breakthrough in the development of anthropology as a discipline.

The Evolution Century

In many ways we can think of the nineteenth century as the “evolution century.” The notion that things evolve according to scientifically discoverable principles became the mantra across the board in academia. Wilhelm and Jacob Grimm, famous for their folktale collections, were significant linguists who documented in detail how languages evolved, Charles Lyell suggested that rocks and the physical landscape evolved according to discoverable scientific principles, Mary Anning found fossil evidence of change in lifeforms over time, and Charles Darwin (as well as Alfred Russel Wallace) argued that species evolve via the process of natural selection. Evolution seemed like the perfect paradigm to cover all branches of inquiry, and the study of human cultures looked as if it could easily fit under the umbrella of evolution along with the others. E. B. Tylor in Britain (Tylor 1871) and Lewis Henry Morgan (Morgan 1877) in North America developed complementary paradigms of universal sociocultural evolution that became normative in anthropology until the early twentieth century. Both Tylor and Morgan argued that the movement through various stages – which Morgan labeled savagery, barbarism, and civilization – was controlled by discoverable scientific principles akin to the three-age system which is still in use (in considerably modified form) in many branches of Old World archeology.

The three-age system, or the movement from Stone Age to Bronze Age to Iron Age, was once seen as an obvious and inevitable sequential progression. There is a simple, although flawed, logic which governs the three-stage progression, as follows. The first humans used rocks to make their tools for cutting, mashing, scraping, and killing because rocks and muscle power were all they had at the outset. They would have used wood, bones, and vines also, of course, but stone tools are what remain in the archeological record, so the era was dubbed the Stone Age. This era saw numerous innovations, including the domestication of plants and animals. Thus, it was eventually divided into the Old Stone Age (or Paleolithic), the period before domestication, and

the New Stone Age (or Neolithic), the period after domestication. Domestication of plants led to sedentism because people had to stay in one place to cultivate their crops. Sedentary peoples could build permanent structures which migratory or nomadic foragers could not. These included ovens for baking that could also be used for firing pottery. Subsequently, these peoples could control heat sufficiently to smelt metals. Copper and tin were smelted first because they have low melting points. Thus, they could produce bronze (an alloy of copper and tin) to make tools which are superior to stone tools ... and so it goes.

Archeologists now see that the three-age system is neither applicable to all cultures worldwide nor as logical a series of steps as once appeared obvious. Some cultures never developed metallurgy indigenously, yet they managed to evolve in complex ways sociopolitically. Some cultures had a distinct copper-using era (the Chalcolithic) in between their Stone and Bronze Ages. Some cultures skipped over the Bronze Age entirely and moved directly from stone tools to iron ones. The possibilities are seemingly endless, and what was once presumed to be a series of inevitable advancements taken in logical steps is now seen to be a much more fluid and malleable process, influenced by a variety of variables including geography and contact with other cultures. In the nineteenth century, however, such nuances were less well known because both archeological methods and ethnographic fieldwork were in their infancy. Reliable data were scarce.

While the 1870s were productive in North America in some spheres of ethnographic fieldwork, such as Morgan's extensive collection of kinship data, first from the Iroquois (Morgan 1851), and later from the Winnebago, Crow, Yankton, Kaw, Blackfeet, Omaha, and others (Morgan 1871), there were two significant problems with the research. First, Morgan relied almost exclusively on interviews, which meant that the information which he collected had no cultural context with which to frame it and, hence, make sense of it, and, second, because there was a physical and intellectual separation between museum departments, which were engaged in field expeditions, and university teaching departments of anthropology, which might have been bastions of ethnographic theory but were virtually nonexistent. Consequently, field data did not translate into theory within a university environment. Morgan never held an academic position but, almost by default (there being no rivals in academia), his paradigm of universal cultural evolution held center stage, and was heavily relied upon by all manner of theorists, from Karl Marx to Sigmund Freud, to buttress their own theories of social development.

The three-age system of archeology, and hence cultural evolution, has the concept of *progress* built into it. Evolution in other academic spheres does not imply that the processes under study which are evolving are also progressing. For example, Modern English is not better than Middle English or Old English in any absolute sense. Each form of the language suits the needs of the people using them in their respective eras. Likewise, marble is not better than limestone even though it is the metamorphosed form of limestone. Both rocks serve their purposes. The verbs "evolve" and "change" in these contexts are almost exact synonyms, and the scholars in biology, geology, and linguistics understood this idea. Unfortunately, the three-age system of Old World archeology, when transformed into a full-blown theory of cultural evolution, can carry with it the notion that cultures advance or improve as they move through the stages.

Not surprisingly, in the nineteenth century the idea of “progress” in both archeology and sociocultural anthropology was equated with technological advancements, as befits a world that was rapidly changing (“advancing” in their terms) under the powerful thrust of the Industrial Revolution, thus placing technology at the center of cultural analysis in anthropology.

The nineteenth century also saw the pinnacle of European colonialism across the globe. This colonialism was fueled by greed, and supported by racist assumptions that colonized peoples were inferior to their colonial masters and, therefore, could be exploited with impunity. To be profitable, industries in Europe, especially Britain, needed cheap raw materials and cheap labor, which colonialism provided in abundance. Slavery was an outgrowth of the colonial system, justified by a stance that non-European peoples were inferior, racially and culturally to Europeans. Civilizing them was the “White man’s burden.” The anthropology that developed in Europe in the early twentieth century was profoundly shaped by this colonialist mentality in complex ways, especially within the imperial holdings of European nations.

Off the Verandah and into the Colonies

At the beginning of the twentieth century, British anthropologists started to emulate their colleagues in North America by journeying well away from home to conduct fieldwork. The turning point for British anthropology was the Torres Straits expedition of 1898 led by psychologist-turned-anthropologist, W.H.R. Rivers, and including C.G. Seligman, who later taught Bronislaw Malinowski, E.E. Evans-Pritchard, and Meyer Fortes. The Torres Straits lie between the northern tip of Queensland in Australia and the southeastern shores of Papua New Guinea, and their indigenous island peoples are culturally and linguistically distinct from either, and from each other.

The six members of the expedition were an assortment of psychologists, linguists, and ethnologists, and their preferred method of fieldwork was interviewing as well as taking physical measurements. In the process, Rivers discovered that the people he interviewed had the same visual abilities as Europeans, yet had no word for the color blue, and used the same word for blue things as for black things. Likewise, they did not have words to distinguish biological siblings and cousins. Inspired by his work in the Torres Straits, Rivers spent several months in 1901–2 among the Todas of the Nilgiri Hills of southern India.

At the time the Todas numbered about 700 individuals living in relative seclusion from other south Indian cultures. They were a **polyandrous** (one woman with multiple husbands) society divided into **moieties** who lived primarily on buffalo pastoralism and dairying – which involved a number of complex religious ceremonies, all meticulously noted by Rivers and eventually published in *The Todas* (1906). In the preface he writes that his work is “not merely the record of the customs and beliefs of a people, but also the demonstration of anthropological method” (Rivers 1906: v). The first 11 chapters are an extremely detailed description of the dairy cult and its priests among the Todas, but then it trails off into generalities concerning gods, magic, kinship, clanship, crime, and so on, and does not integrate these descriptions with his analysis of pastoralism. Furthermore, he failed to document the existence of

matrilineal clans alongside the patrilineal ones. The problem was that he had to use an interpreter to communicate with the Todas, and he lived in a hotel the entire time – leading to what is sometimes called “verandah ethnography” – the replacement for armchair anthropology. That is, anthropologists now journeyed away from their cloistered libraries to far-flung locations and peoples, but then interviewed them on hotel verandahs rather than living with them in their own villages, one-to-one. Bronislaw Malinowski changed all of that.

According to Malinowski’s later recounting of the story of how he developed **participant-observation** fieldwork, his inspiration began while attending Jagiellonian University in Kraków in Poland, pursuing a doctorate in physics and mathematics. He became bedridden with an illness, and during his convalescence he began to read Frazer’s *Golden Bough*. Taken by the ideas in the work, he abandoned his current track (after the doctorate) and went to the University of Leipzig in Germany, where he studied economics and psychology, and then in 1910 went to England to study anthropology at the London School of Economics under C.G. Seligman.

Malinowski was in Australia attending a conference in 1914 when World War I broke out, which left him in a precarious position because he was an Austro-Hungarian citizen and, therefore, an enemy noncombatant alien as far as the British were concerned. The Australian government was sympathetic to his plight, however, and gave him funds first to do fieldwork on Mailu Island in Papua, and then in the Trobriand Islands. The particulars are a little obscure, but the received wisdom (based in part on his diary) is that Malinowski was not comfortable with his first placement in Papua. He did not speak the local languages and did not have the ability to live with the people he was investigating, so he felt limited in his ability to understand the culture. What is a little perplexing in hindsight is why these early attempts at fieldwork seemed so limiting to him. What Malinowski was undertaking at first in Papua – verandah fieldwork – was the norm for social anthropology at the time. Thus, in June 1915 he started again in the Trobriand Islands off the northeast coast of New Guinea and continued, off and on, for several years, this time living among the Trobrianders and learning about their culture by being thoroughly immersed in it, in isolation, for long periods.

Malinowski’s contribution to anthropological data collecting cannot be underestimated, and the range of his subject matter is astounding. His method is sometimes referred to now as an “off the verandah” technique, meaning that he broke with the custom of interviewing indigenous people on a hotel verandah, and, instead, lived and worked with them in their local villages – learning the local language rather than relying on an interpreter. The methodological point that Malinowski stresses in *Argonauts of the Western Pacific*, his best-selling, compendious account of island to island exchange (kula ring) and ocean-going canoe travel in Melanesia, is that the participant observer is both intimately involved in the culture under study while, at the same time, scientifically detached. For example, at one point he writes that the goal of the ethnographer in the field is “to grasp the native’s point of view, his relation to life, to realize his vision of his world” (Malinowski 1961 [1922]:25).

Today, anthropologists often use the term “**emic**” to denote discussion of social situations in local terms (Malinowski’s “native’s point of view”) and “**etic**” to refer to the anthropologist’s perspective. These terms are derived from Kenneth Pike’s analysis of language. In his *Language in Relation to a Unified Theory of the Structure of Human Behavior* (Pike [1954, 1955, 1960] 1967), Pike draws a technical distinction between a

“phonetic” and a “phonemic” analysis of the sounds of a language. Phonetic differences are any differences in the sound of a word that can be detected, and phonemic differences are *differences that make a difference* to the semantic meaning of the word in that language. For example, English speakers can detect the difference between an aspirated and unaspirated /p/ when it is pointed out to them (phonetic difference), but the difference makes no difference to the meanings of words in English (phonemic difference). If you hold your hand close to your mouth and say the word “paper,” you should feel a puff of air on the first /p/ and not on the second. The first /p/ is aspirated, the second is not. You can experiment to make them both aspirated or both unaspirated, but the meaning of the word does not change whichever way you say it. Linguists say that there is a phonetic difference (you can detect the difference), but not a phonemic difference (the meaning of the word does not change). In languages such as Khmer, aspirated and unaspirated /p/ are both phonetically *and* phonemically different. Using one versus the other changes the meanings of words.

Strip off the /phon/ component of Pike’s terminology (“phone” = “sound”) and you have “etic” and “emic.” You can add new prefixes to get such concepts as “proxemics” – documenting the use of interpersonal space to infer what different distances mean locally (see Chapter 6) – or “aesthemics” (as opposed to “aesthetics”) – understanding value judgments concerning beauty in indigenous terms. Or, you can simply use the suffixes “emic” and “etic” as analytic words in their own right (pronounced /eemic/ (long e) and /etic/ (short e) – same is in “phonemic” and “phonetic”). The emic/etic distinction is of considerable importance in anthropology in general and in fieldwork methodology in particular. It often, simplistically, gets translated into: ways of viewing things from an “insider” (emic) versus “outsider” (etic) perspective in anthropological discourse, but such a translation is rather misleading, although not entirely inaccurate.¹ Pike, as a linguist, frequently used the phrase “differences that make a difference” to describe an emic approach, which I can illustrate with a simple example.

In Cambodia it is considered disrespectful, as a general rule, to show bare skin on shoulders or knees in public. Some men get away with going shirtless if the weather is hot and they are involved in heavy labor, but otherwise everyone is expected to cover their shoulders. This rule is rigidly enforced in sacred places, such as pagodas. People are denied entry if their bare shoulders are visible. But, covering one’s shoulders is not straightforward. One cannot simply drape a scarf, shawl, or other loose piece of cloth over bare shoulders and expect to be admitted to a pagoda. In European terms, shoulders with skin visible are bare, but shoulders covered with a shawl are not bare. The emics of Khmer culture are different. For your shoulders to be considered “covered” you must be wearing a fitted garment that has sleeves, such as a shirt or jacket, so that the skin on your shoulders is not visible. That is, simple visibility is not the issue. *How* the skin on your shoulders is covered matters. In Khmer emics: shoulders with skin showing and shoulders covered by loose material = bare; shoulders covered with a fitted garment = not bare. In this case, bare skin versus shawl on skin is a difference that does *not* make a difference. That is, it is an etic difference, not an emic one, to Khmer people.

1 See https://www.researchgate.net/publication/246054591_Emics_and_Etics_The_InsiderOutsider_Debate for further details.

Malinowski valued probing Trobriand Islanders' emic view of the world, but he was also interested in an etic approach. He writes:

Not even the most intelligent native has any clear idea of the Kula as a big, organised social construction, still less of its sociological function and implications ... The integration of all the details observed, the achievement of a sociological synthesis of all the various, relevant symptoms, is the task of the Ethnographer ... the Ethnographer has to construct the picture of the big institution, very much as the physicist constructs his theory from the experimental data, which always have been within reach of everybody, but needed a consistent interpretation.

(Malinowski 1961 [1922]: 83–84)

Malinowski's early history as a physicist is clear here, and his comments raise questions about fieldwork methods and their ultimate purpose. Here he is saying that fieldwork data are akin to experimental data in physical science in that in both cases the observer gathers the welter of observations together and is able to abstract overarching principles from all the myriad details. This is a point of view that we must ponder very carefully. Is it the goal of ethnographer as social *scientist* to take a mass of field observations and reduce them down to social laws in the way that Isaac Newton took a mass of experimental observations and reduced them to the laws of motion, or is there a different goal in anthropology? This debate has swung back and forth for some time with no clear answers, although at present the overwhelming majority of cultural anthropologists reject the scientific model (see Part II).

Post-Colonial Anthropology

The step from hotel verandah to village hut was a major leap forward for ethnographic fieldwork, but it contained some baggage that was rarely acknowledged. No matter how much Malinowski wanted to be a “participant” in local activities, he was always going to be perceived as an outsider, and, as importantly, he was going to be categorized as a member of the colonial elite. This status cannot avoid coloring the relationship between ethnographer and people being recorded. Whether in interviews, participant-observation, or both, there always exists a power dynamic informed by the status of the parties involved.

It is not an accident that all prewar ethnographic fieldwork conducted by British social anthropologists took place among peoples who had been colonized and become part of the British empire. Nor is it a coincidence that many of their works, such as *African Political Systems*, edited by Meyer Fortes and E.E. Evans-Pritchard (1940), and *Political Systems of Highland Burma* by Edmund Leach (1954), spoke directly to the political organization of subject populations. Colonial offices in Britain funded such research because they wanted to devise strategies for controlling local people by using their own power structures rather than simply imposing British law arbitrarily. Whether consciously or unconsciously, such a situation inevitably leads to a skewed power dynamic between ethnographer and people being analyzed.

During the mid-twentieth century, many European colonies, particularly those controlled by Britain and France, pushed for independence. When that independence was achieved, European anthropologists no longer had easy access to what many previously saw as their “natural laboratories” – especially in Africa and Asia. Once the colonial powers gave way to indigenously controlled governments, the continued role of anthropology came under scrutiny both in the former colonies and at home in anthropology departments. This time period is now conventionally thought of within anthropology as “post-colonialist” with a mindset which seeks to redress the longstanding imbalance of power and authority between the person conducting fieldwork and the people being analyzed. Post-colonial anthropology also seeks to change the very framework of investigation, including the continuing assessment of the relative value of various lines of inquiry both for the inquirer and for the community in the spotlight. It also includes the restructuring of inquiry so that the people under analysis are able to conduct the analysis themselves.

The process of transforming ethnography from an outsider enterprise to an insider one meant that both methods and theory came under deep examination and were forced to change because of it. On the one hand, if a member of a group of people whose cultural history lies well outside of mainstream Euro-American practices takes on the job of ethnographic inquiry, the intellectual norms of Euro-American anthropology get imported into that inquiry (including its goals as well as its procedures). Thus, the insider may (perhaps unwittingly) take on the role of an outsider. On the other hand, an indigenous ethnographer may have the desire, and ability, to change the methods and analysis of anthropology to suit local needs and expectations, although such changes bring their own problems (see, for example, Hannoum 2011; Hurston 1935; Mingming 2002).

The underlying colonialist sympathies of anthropologists such as Malinowski and Evans-Pritchard are now reasonably well understood, although their implications are still being explored and debated. In Malinowski’s case, his private diaries confirm his many ethnic and gender biases explicitly. It is just as easy to identify inherent prejudices in the fieldwork of the earliest practitioners, even in the absence of confidential personal data. We should also acknowledge attempts by anthropologists, from the mid-twentieth century on, to reflect on the nature of their own fieldwork and its inherent limitations (e.g. Briggs 1970; Dumont 1978; Marriott 1953; Powdermaker 1967, Rabinow 1977 (& 2007)). In light of this complicated history of the development of anthropological fieldwork, today, it is ever more important for fieldworkers to be hyper-vigilant concerning the power dynamics that exist between the ethnographer and the people providing ethnographic information (e.g. Ayi et al. 2007; Bester et al. 2003; Cerwonka and Malkki 2007; Gardner 2006; Siegel 2011); I will discuss this issue in more detail throughout this book, beginning with Self-Study (Chapter 5).

Another change in anthropology during the post-colonial era is an increasing emphasis on the need for fieldwork to do *something* beyond the collecting and analysis of field data for pure research purposes only. This had led to the development of the specialty of **applied anthropology**, including **medical anthropology** (see in particular Kedia and Van Willigen 2005). Applied anthropology required that anthropologists become more directly engaged with the people under investigation (see e.g. Albro et al. 2011; Armbruster and Laerke 2008; Price 2004; Sanford and Angel-Ajani 2006),

such that activism and **engaged anthropology** became field methodologies in their own right and are now mainstream within the discipline (see Chapter 10). Today, applied anthropologists work in a myriad of areas, including in business, IT, health-care, environmental science, archaeology, and other fields. There are numerous benefits, as well as drawbacks, involved in applied and **activist anthropology**, including ethical concerns created by the deliberate intervention with communities. These issues are directly addressed in relevant chapters.

In the process of coming of age, qualitative fieldwork went through a great deal of methodological and philosophical soul searching concerning the nature of the data being collected and the ways they were analyzed and presented, as anthropologists became more and more skeptical about the validity of the ethnographies of the past. Could field data ever be considered objective, and, even if such a goal was possible, was it desirable? Should fieldwork follow the basic models of hypothesis testing that dominate the physical sciences? How do we ensure that qualitative methods are legitimately rigorous? These, and many other, questions were posed repeatedly, beginning in the postwar years and they continue to this day. Therefore, before beginning actual projects we need to take a step back and review some key methodological issues to bear in mind when embarking on fieldwork.

Part II Analytic Strategies

Fieldwork Vs Ethnography (or Ethnology)

Analytically, there is a difference between doing fieldwork and writing ethnography (or “doing” sociocultural anthropology), although the edges between the two are fuzzy. Rather loosely, we can think of fieldwork as the process of data collection, and ethnography as the next step: writing up field notes into finished form that analyzes the field data. But things are not that simple. There is a necessary synergy between data collection and its analysis. Etymologically, “ethnography” is a blend of two Greek roots (ethnos = a people + graphein = to write). The older, more generic, term for the study of cultures was “**ethnology**” where the suffix logos (= knowledge) was preferred – akin to biology (life knowledge), geology (earth knowledge), theology (god knowledge), and anthropology (humankind knowledge). Today, “ethnology” is considered a somewhat dated term, although it is still used, and the notion that we are *writing* about a culture seems to sit better philosophically with anthropologists than the thought that we definitively *know* something about them. After all, writing about cultures always entails analysis even if only implicitly. There is no such thing as a simple description of a culture without some kind of analysis. The bias of the observer is inevitable.

Terminology aside, curiosity about “other” cultures has a long history (see e.g. Malefijt 1974; Honigsmann 1976). Not only did ancient and medieval scholars, such as Plutarch and Ibn Khaldun, describe the cultures of other lands, they also developed theories concerning why people in those lands behaved in the ways that they did. These classical theorists had numerous insights, some of which we can still debate today (does climate affect personality and/or behavior, for example), but they lacked rigorous fieldwork methodologies. Well-designed fieldwork is what sets apart the

modern study of ethnography from these older sources. But “rigor” in the context of qualitative analysis is a thorny issue. Controlled experiments are not ethically viable in sociocultural anthropology, although certain kinds of quantitative analysis used in anthropology (such as studies of nutrition or life expectancy cross-culturally) are able to borrow rigorous methodologies in emulation of physical science. This point raises the objective versus subjective dilemma within social science.

Objective Vs Subjective (and Anti-Objective)

The distinction between objective and subjective data (which is sometimes used to distinguish between the physical sciences and the humanities – with the social sciences hovering somewhere in the middle) is popularly believed to be an easily defined dichotomy. Often, people view objective reality as being independent of any observer, whereas subjective reality is filtered through the biases of a human spectator. For instance, “This coffee is 95°C” (objective) versus “This coffee is too hot” (subjective). This commonsense dichotomy has been under scrutiny by scholars for centuries because it is deeply problematic when held up to the light. Can there be a physical “objective” reality that is independent of the senses needed to appreciate its composition? **Postmodern** philosophers have further questioned the seemingly “obvious” distinction between objective and subjective realities and led anthropologists to question whether objectivity (however defined) is possible at all – especially in the social sciences. Can a fieldworker *objectively* study a social situation? Should they even try?

The critique of objectivity in anthropology began in the postwar years, reaching one kind of climax in the essays in Dell Hymes’s collection *Reinventing Anthropology* (Hymes 1972). The last two sections of this anthology focus on different facets of this critique (see especially Jay 1972; Scholte 1972; 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, Goodall 2000; Hertz 1997; Herzfeld 1987; Marcus 1998; Richardson 1990; Robben and Sluka 2012).

While the critique of objectivity in the social sciences has taken many forms, what one should carefully consider before embarking on fieldwork is less the notion that the data obtained through fieldwork are *unobjective*, or incapable of being made objective, but rather the idea that they can be deliberately conceived of as *anti-objective*; that is, they can stand in conscious *opposition* to objectifiable data. In the case of life histories, for example, certain parts of them may be objectified (in the conventional sense) 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; Zeitlyn 2008). Such meanings cannot, by their very nature, be made “objective.”

It may be that this anti-objective approach to field data always requires some kind of conversion experience on the part of the fieldworker mediated by the people they are engaged with in the field. An illustration of this concept comes from my dissertation fieldwork in Tidewater, North Carolina, which I discuss in greater detail in Chapters 7

and 16. The experience helped me move away from attempting to objectify the data that constituted the daily lives of my research participants, but it was a tough lesson to learn (Forrest 1988a; 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 as a brand-new fieldworker I was eagerly intent on getting the "facts" (objectively) 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 [in 1978].

FI: Uh-huh.

It took me over six years of listening to this recording 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. Of course, it is 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 system – as it relates to the quilt. Two responses in FI's discourse make this clear. The first is the command, "... so you work it out." In essence, what she is really (politely) saying is something like the following: "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 I 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 tone and facial expression indicated that she had heard what I had said, but was not interested. *Her* value terms in relation to the quilt were:

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*

² I identified each of the people I interviewed with a pair of capital letters that kept their identity hidden from outsiders, but used a simple algorithm for me to be able to identify them easily. Meanwhile, in my transcriptions I used JF for myself.

What I later discovered was that for FI, 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 was then able 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 ...”

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 taken anthropology courses, because the discipline had inclined me to *study* them (that is, observe), rather than to *learn* from them (that is, engage). 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. That is, they rejected my attempts at etic analysis in favor of their own emic approach, which they considered to be more important. And, of equal importance, I recorded all the interviews, and therefore have been able to return to them year after year, deepening my understanding of the community. *Field notes are a permanent record.*

What I did not understand at the time was that interviewing as a fieldwork method can be surprisingly deceptive in that the material being produced may contain multiple layers of meaning that are not evident at the time that the interview is being conducted. One benefit of recording interviews is that you can return to them repeatedly and continually reassess what you have recorded, as in the example above. In fact, this kind of reassessment can begin almost immediately as you are transcribing, and can continue for years. Recorded interviews, by their very nature, are artificial events in the life of a community you are studying, but another significant benefit is that they are one method of entering into the subjective reality of the people you are working with. In this book there are numerous projects that employ interviews in part or as the whole exercise. Keep in mind that *different methods of investigation produce different kinds of data*. Not only is interviewing only one method of data collecting, the way that you approach recorded interviews determines the kind of data you receive. Most anthropologists today prefer to record interviews in *partnership* with the person being interviewed as opposed to seeing the interviewee as an object to be studied. Indeed, this approach is useful in multiple methods; not just interviews.

While the objective/subjective distinction can still serve a purpose in a generalized, **heuristic** manner, the best we can accomplish is what we can call “objectifiable” information, that is, collecting certain kinds of information in ways that allow them to be measured in standardized or quantized ways (see Thapan 1998). Certain objectifiable facts are always going to have their uses. When dealing with issues such as economics or nutrition or medicine, one might, of necessity, need to gather objectifiable data, such as the calorific value of foods commonly eaten or the analgesic properties of

locally made teas. But even such objectifiable data are only usable within a context which will inevitably be subjective. What the facts are, is objectifiable; why they matter (and which ones are important), is always subjective.

Idiographic Vs Nomothetic Approaches

Right from the start of the twentieth century, the status of anthropology as a science (even when qualified as a “social” science) was a topic of debate. Both Boas and Malinowski had pursued advanced degrees in the physical sciences before becoming anthropologists and undertaking ethnographic fieldwork and, therefore, brought an element of scientific inquiry to their fieldwork. Meanwhile, Boas’ student, Alfred Kroeber, characterized anthropology as “the most humanistic of the sciences and the most scientific of the humanities,” which was his way of softening the sense that ethnographic inquiry was a scientific endeavor akin to physics or chemistry, yet it was still a science.

We commonly call the methods of physical scientists, such as Galileo and Newton, “**reductionist**” because they take the complexity of observable reality and *reduce* all the details to simple principles that ultimately govern the seemingly endless details. The observer stands outside of what is being observed in order to uncover its mysteries. The philosopher Wilhelm Windelband (1848–1915) called this approach “**nomothetic**” and contrasted it with the “**idiographic**” approach of the humanities. The humanistic approach is the diametric opposite of reductionism because it is interested in exploring the rich contexts and diversity of cultural phenomena, rather than stripping them away in order to hypothesize simple, unifying principles.

For example, if you like romantic comedies, you will not be stopped from watching a new Netflix release by a friend telling you that the main characters in the movie fall in love near the beginning, face a difficulty that pulls them apart, but then find a way, by some twist of fate or other plot device, to be together in the end. You know this structure: *you like it*. You take this structure as a given, and you go to the movie because you want to see the *specifics*: the exact character of the principles, the jokes, the absurd misunderstandings, and so forth. It is the particulars that attract you, not the generalities (which you assume). We call the focus on the particulars of a situation, over the desire to reduce its specifics to general rules, an **idiographic approach**, and it is the hallmark of interpretive analysis in ethnography. This book takes the position that ethnographic analysis sits somewhere between the nomothetic and the idiographic. Through the projects in this book you will have ample opportunity to explore both approaches.

Undergraduate Fieldwork

One of the prime motivations in producing this text is to teach fieldwork methods to undergraduates (as well as other novice researchers). Yet undergraduate research has not always been encouraged. Since Malinowski pioneered the gathering of data through participant observation in the Trobriand Islands, fieldwork has been entrenched within the discipline and viewed as a “sacred” activity (see Sluka and Robben 2012). Indeed,

nearly all PhD programs in cultural anthropology today require a student to produce a dissertation based on intensive fieldwork (of a year or more). There are outliers who have managed to graduate without doing fieldwork, but they are rare indeed. Fieldwork is considered the quintessential rite of passage for entering the profession.

Allowing undergraduates into the sacred rites of the profession was at one time viewed with extreme caution, yet increasingly it is seen as an important, if not essential, experience for students (see Ingold 1991; Sharma 1989; Sharma and Wright 1989; Thorn and Wright 1990; Watson 1995). Starting in the 1980s and 1990s, many social and cultural anthropology programs in Britain and the United States embraced undergraduate fieldwork as a core curricular component, but this trend met with loud resistance in 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 in this debate I would simply say that adequate advanced preparation and constant supervision by trained faculty mitigate the potential dangers that can arise, and that, in my experience, the value of undergraduate projects has been immense. If it were not, I would not be writing this text. Now, field methods requirements for undergraduates are relatively common, and quite popular because of the enduring effects it has on the students. Fieldwork is like that. It changes you.

While not getting bogged down in the intense quarrels within the discipline concerning the validity of our field research, you should familiarize yourself at some point with the continued re-evaluation of the overarching legitimacy of our methods. I recommend dipping into some of the following:

Anthropological Practice: Fieldwork and the Ethnographic Method by J. Okely
(Berg Publishers, 2006)

Ethnographic Fieldwork: An Anthropological Reader Edited by C.G. Antonius,
M. Robben and Jeffrey A. Sulka
(Wiley-Blackwell, 2006)

Being There: Fieldwork in Anthropology (Anthropology, Culture and Society) by
C. W. Watson,
(Pluto Press, 1999)

*Fieldwork Is Not What It Used to Be: Learning Anthropology's Method in a Time
of Transition* by James D. Faubion and George E. Marcus eds.,
(Cornell UP 2009)

Ethnography: Understanding Qualitative Research by Anthony Kwame Harrison.,
(Oxford University Press, 2009)

You might also want to look at how fieldwork on communities that interest you is conducted by anthropologists who have a particular/personal slant on their data, and how this work gets translated into ethnography. Instead of pretending to be presenting “objective” data about individuals and communities, or to being “objective” about their research findings, ethnographers now routinely embrace their own cultural

identities, as well as their sympathies with marginalized and oppressed peoples, and use those identities to encourage nuanced or multifaceted ways of writing ethnography. The following is a small example of the types of ethnography that explore marginalized identities. You should review at least one such ethnography to get a sense of ways in which you can move away from traditional, objectified writing. Your instructor can help you find more.

The Body Silent: The Different World of the Disabled by Robert F. Murphy (Holt 1987)

A Coincidence of Desires: Anthropology, Queer Studies, Indonesia by Tom Boellstorff (Duke University Press, 2007)

Veiled sentiments: honor and poetry in a Bedouin society, by Lila Abu-Lughod. (University of California Press 1986).

High Tech and High Heels: Women, Work, and Pink-Collar Identities in the Caribbean by Carla Freeman. (Duke University Press, 2000).

Encounters with Aging: mythologies of menopause in Japan and North America. Margaret Lock. (University of California Press 1993)

Citizen Outsider Children of North African Immigrants in France by Jean Beaman (University of California Press 2017)

Burning at Europe's Borders: An Ethnography on the African Migrant Experience in Morocco by Isabella Alexander-Nathani. (Oxford University Press 2021)

Borders of Belonging: Struggle and Solidarity in Mixed-Status Immigrant Families by Heidi Catañeda. (Stanford University Press: 2019)

Decolonizing Extinction: The Work of Care in Orangutan Rehabilitation by Juno Salazar Parreñas (Duke University Press 2018)

Out of roughly 3,000 students I have taught fieldwork to (including some 400 anthropology majors), only 4 have pursued postgraduate training in anthropology. The rest are employed as social workers, activists, doctors, lawyers, and professionals of various stripes. But, nearly all retain a strong sense of the anthropological method, and look 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.

Many of my former students mentioned their experience with the various skills learned in fieldwork in job applications and interviews, and some of them continue to use these skills in their work environment. Qualitative research methods are poorly understood by the general public, as well as by employers. Nonetheless, savvy employers can be convinced of the benefits of hiring someone with field methods skills. I won't go so far as to say that having field methods skills will land you a job, but having them is another arrow in your quiver.

At minimum, training in anthropological field methods develops a way of observing and interacting with the world in general – all the time. Of course, it will be up to you to assimilate the lessons learned in field methods into your life as a whole. This process should not be difficult because intensive and analytic engagement with people is potentially life altering anyway. On the other side of the coin, it is important to bear in

mind – always – that intense engagement with people carries a moral and ethical burden. I take up this issue in a subsequent chapter on ethics which is mandatory reading before you begin any of the projects (Chapter 3).

Most of the projects in this book are self-contained, but a few rely on methods developed in others, and, where this is the case, I cross-reference the projects. I will be giving examples of projects that my students developed to give you ideas; you might find some of them worth emulating. But I also encourage you to be creative in your choice of field situations. When I taught a methods course I always had one class per project that was a practicum in which I walked my students through an example project, and usually involved them in some form of observation and documentation so that they had a little practical experience of the method before they embarked on their own projects. From time to time in the project instructions in this book I discuss salient practicums I used as additional aids in your learning process. Your instructors will undoubtedly have experiences of their own to share with you in a similar vein. You should use their knowledge as you proceed, and be guided by their preferences for the precise execution of projects and their presentation. The projects proposed here may be used as is, or they can be tailored to various instructional needs. Fieldwork is, by nature, a disciplined process that, while rigorous, allows for spontaneous flexibility.

Vocabulary and Writing

Ethnographic writing is its own species of writing, and has come under critical scrutiny in recent decades for its capacity to mislead and misrepresent people. *How* you write about people is as important as *what* you say about them, and it is possible to offend the people you are writing about, or to give the wrong impression about them, by using inappropriate vocabulary or phrasing. This is a topic that you should discuss at length with your instructor; here I will simply point out a few cautions to bear in mind when presenting your data.

Anthropologists have been sensitive for some time to the fact that groups of people are commonly identified in multiple ways – the term(s) that outsiders use to name them, and the term(s) they use to name themselves. “Eskimo,” for example, is an outsider term, whereas “Inuit” (and variants) is an insider term. In reasonably straightforward cases, anthropologists opt for using the insider term. But things are not always straightforward. Take the case of people from predominantly Spanish-speaking countries in North, Central, and South America (and the Caribbean) living in the United States. What do you call them as a group? Are they a definable group? Do they have an insider term?

The problem with finding a term for Spanish-speaking peoples from the Americas living in the United States is that this is a multicultural demographic when viewed from the inside, but homogeneous when considered by outsiders. Any single term that identifies them as a group is going to entail treating them with some sense of unity as an “other” group (other than dominant, white, US culture). That is, the supposed unity of the group exists only in the minds of outsiders. It is common to hear Spanish-speaking immigrants in the United States being called “Mexicans” regardless of their country of origin, and in this case “Mexican” is often used as a derogatory term. The desire for such people to adopt a collective term for themselves is an overt admission

that they form a group in the eyes of an oppressing class and that they need a collective term to use in their struggle to fight back against oppression.

The problem with finding a collective term is that every one of them carries negative connotations for some or all of the people. “La Raza” had a currency for decades – originating in political struggles in Mexico in the 1920s. The problem is that “raza” is a loaded, potentially racial/racist, term that has, among other things, the implication not just of being non-white, but of being actively anti-white. In Argentina we have a specific classification, “rubio/a,” meaning something like the English term “white” and includes a substantial percentage of the population in urban areas. Rubios of Argentine origin (myself included) have major issues with being lumped into the general category of La Raza (which defaults in our minds to what we call “moreno/a” (brown) or “mestizo/a” (mixed) – also heavily loaded terms). In the 1960s, “Chicano” had widespread popularity among some groups but not with others. It had originally been a derogatory outsider term, but, as is quite common with such slurs, was picked up by insiders and used as a badge of honor. In turn, “Hispano/a,” “Latino/a,” etc. have all been adopted and discarded.

One of the additional problems with Spanish is that many words have genders. The suffix “-o” is typically masculine, and the suffix -a is feminine. When you have an unidentified group of people who could be male or female or both, the traditional linguistic convention has been to use the masculine plural. This usage is now changing. For example, “Latino/a” where gender is unclear or generic, or, more commonly nowadays, “Latinx” where the “x” indicates an undefined gender. In English, similar problems have been addressed in an analogous manner. At one time, the correct usage would have been to default to the masculine for pronouns, as in the sentence, “When a doctor is unsure of a diagnosis, he will ...” Nowadays there are two choices to replace “he” in that sentence: one being the composite “s/he” (or he/she) and the other being to use the plural “they” – which is becoming more common usage. Gendered terms for occupations, such as “actress,” “poetess,” and “sculptress,” have all but disappeared from contemporary discourse.

There are numerous adjectives and nouns in common use in English that can be classified as dead metaphors (that is, words that carry metaphorical meanings either etymologically or historically, but no longer do so in contemporary speech) which can be culturally offensive even though their historic meanings have long since passed away. Anthropologists try to err on the side of caution in this regard. Take, for example, the old British slang word “clodhopper” (sometimes shortened to “clod”) meaning someone who is either physically or mentally clumsy. A “clod” refers to a lump of dirt such as you would find on a farm or in rural areas, so a clodhopper, that is, someone who has to hop over clods of dirt, is a farmer or rural person in general, implying that a clumsy person is like a (stupid) farmer. The word “clown” has exactly the same history. In the sixteenth century it was a term for a rustic or peasant, shifting in meaning over time to mean anyone whose actions were foolish. Where can we draw the line? Is it acceptable to call an argument “myopic” when the term refers specifically to a visual impairment? Probably not, even though the word is in common usage. Caution is always needed.

Terms such as “philistine,” which are derived from ethnic slurs, raise the issue of how to refer to ethnic and geographic designations. It is impossible to refer neutrally

to the region of the world that was once the homeland of a group of Semitic peoples that became known Biblically as the Israelites, because any term you use for the region as a whole – Palestine, Israel, Canaan, Levant, et al. – will offend someone. Political designations have shifted over time, but whichever one you choose, it is a **hegemonic** label and, therefore, problematic. By that same token, “American” as an adjective for people and things that originate in the United States is offensive to me as it is to many people from Argentina, and much of South and Central America as a whole. Aren’t we Americans? In Spanish we use the term “estadounidense” (person or thing from the United States) or, more generally, “norteamericano” (North American) as adjectives in place of “americano” (American) which is too broad. But if you were born and raised in the United States you probably use “American” as a self-designation without thinking. The American Anthropological Association does. It can be challenging, but eye-opening, to examine your language usage critically.

Even terms that were at one time in common usage in anthropology, such as “tribe,” have come under suspicion even though they may have insider approval (as in parts of the first nations of the United States). Such terms tend to get lumped into the category of colonialist terminology, and are, indeed, vague when applied indiscriminately. But sometimes the alternate choices are either confusing or unhelpful. Take, for example, the adjective “indigenous.” It comes from Latin and its original meaning was “born in (a place),” but this meaning has been supplanted by a meaning of “original peoples” (synonymous with “aborigine” – that is, “ab origine” meaning “from the beginning”). In contemporary usage, however, “indigenous” is generally used to mean “the people who were living in a place when European colonists arrived.” You cannot legitimately refer to the Lenape as the *original* inhabitants of New York or the Inca as the *original* inhabitants of Peru. Peoples came and went continuously before Europeans arrived. But, the arrival of European colonists in a region put a timestamp on who was there when they arrived. Thus, even “indigenous” is a colonial term (meaning “descendants of people who were here at the time that the first colonists arrived”). There is no clear solution, but it is common in ethnographic writing nowadays to designate a group using the term for them that they use in their local language for themselves. Thus, for example, “Navajo” was the name given by Spanish missionaries to a group of people living in North America. They refer to themselves as *Diné* (“the people”), which is how anthropologists typically refer to them now.

At one time, anthropologists were in the habit of using the term “informant” to describe a person giving information to the ethnographer. The term has a rather dark undertone, as in “police informant,” but its more general usage in anthropology has habitually been less sinister: an informant “informs” the fieldworker, but thereby implies a certain kind of distance between speaker and listener. Because of the negative connotations of the term, contemporary anthropologists are more inclined to work with either more neutral or more indicative (and inclusive) terms such as “participant,” “interlocutor,” “partner,” or “interviewee.” Sometimes, however, using “informant” is the generic choice that fits best. Not all fieldwork situations are partnerships or symmetrical relationships by any means, and it can be misleading to represent them with vocabulary suggesting that they are.

Likewise, I am mindful that even the “field” and “fieldwork” can be loaded terms. When you interview someone in his/her home, are you “in the field”? Yes and no. The

“field,” once conceived of as an “other” place where the “other” were studied, is now no longer a viable term, and certainly not when it comes to the projects in this book which are likely going to be conducted in situations with which you are reasonably familiar (even though some components will necessarily be new to you). The term “fieldwork” is somewhat less troublesome. Fieldwork is the process of gathering ethnographic data. Doing fieldwork has more to do with a certain mindset rather than with the people or places involved. That mindset has different facets, but it is always more than simply looking on or even “being there” as fieldwork is sometimes described (Watson 1999; Bradburd 1998).

Even if you go to live in a country that is alien to you, for a year or more, so that you are forced to learn a new language, meet new people, engage in forms of daily life that are foreign to you, and participate in strange customs, you are not necessarily doing fieldwork – even though some of the behaviors overlap. If you move to a strange location, your primary interest in learning about the place is likely to be pragmatic – where to live, how to get food, where to work, and so forth. Fieldworkers have to learn these things also, but they are not primary. Fieldwork has an ethnographic purpose as its focus, and, therefore, you must always maintain an ethnographic mindset, even if that mindset is not always front and center.

The purpose of fieldwork is to gather data to write ethnography. Therefore, you are “in the field” whenever you are gathering ethnographic data. The “field” is not a place, it is an attitude (or group of attitudes), or what we can call a mindset. The best way to establish this point is to think of a variety of people who are gathering data and what their purposes are: detective, tourist, journalist, and fieldworker. They are all concerned with useful data, but what can be considered “useful” is determined by the purposes for which the data are gathered, and how they will be used subsequently.

So, what is an ethnographic mindset? There is no easy answer to that question, and the exercises in this book provide a hands-on approach to grasping what it is that fieldworkers do. It is not one thing. Sometimes they observe, sometimes they engage, sometimes they participate, sometimes they take action. Sometimes they take notes, or make voice recordings, or take photographs, or make videos, or draw maps, or play computer games, or combinations of all of these and more. After you have done some of the exercises here we can return to the question, and I have some additional thoughts in my concluding chapter. Meanwhile, you can keep the salient question in the back of your mind, “What am I seeking to achieve?” Fieldwork is never a passive enterprise; it must always be actively reflective and reflexive (at some level).

Likewise, instruction in fieldwork can also be reflexive. This book is a fair example in places. In many projects I introduce instructions and suggestions in a reflexive manner. That is, I reflect on the kinds of practices that worked well in my classes, and also on what I might have done to improve them. This practice developed out of my general desire to see teaching as a *partnership* with my students. When I was a classroom lecturer, there came a point in most classes when I would talk directly about my life, beginning with the observation that I had a life outside the classroom. I told my students that I had a wife and son, I owned a house, had bills to pay, had heartaches and joys, just like everyone else. I did not just magically appear for 90 minutes twice a week, dispense objective information, and then disappear into some nameless void. In that sense, teaching anthropology is not like teaching chemistry (not for me, at least).

I have taught chemistry without injecting myself into my lessons; I have never taught anthropology that way.

My students often used the expression “the real world” to describe life outside the university, and I frequently pointed out to them that the expression implied that the university was not real. It is real. It is part of the world. You can talk about the office world or the factory world or the university world, but one is not more or less real than any other. They all have their rules and they all have their rewards and punishments. What matters is how well you know the rules (which is one of the issues to be delved ethnographically). I am embedded in many chapters in this book to show you how I derived my methods, and you (and your instructor) are free to question them, and alter them, instead of treating them as absolute instructions to be obeyed without demur. What I do works for me (usually); it may not work for you. All I can do is tell you *why* my methods work for me. That is why the book is (minimally) reflexive.

This is a book about learning how to do fieldwork and how to present your findings to your instructor or some other relatively self-contained, or private, audience. You are not being asked to create a polished finished product to be widely disseminated. That enterprise is a step beyond what is required of you here. Organizing your field data and presenting it to your instructor is an intermediate step between gathering the data in the first place, and honing it into professional-level output. That final step is not within the purview of this book. If you have an interest in pursuing this subject more, specifically as it relates to both the gathering of data and the conversion of data to output for public consumption, you can consult the anthology, *Fieldnotes, The Makings of Anthropology* edited by Roger Sanjek (1990) or *Writing Ethnographic Fieldnotes* by Robert Emerson et al. (2011). The latter book is primarily concerned with how you go about taking notes during fieldwork and should also be consulted in this regard, because the process is not as obvious as may seem at first, and there are many wrinkles that you may not have thought of. Although in its second edition, the book is somewhat dated given the technologies now available to fieldworkers. When I present the individual projects in this book, I give different strategies for note taking depending on the nature of the project.

Individual projects in this book offer a wide array of methods for presenting your data including standard written reports, PowerPoint slide shows, videos, blogs, and so on, and which one you choose will depend on the parameters of the project, your instructor’s requirements, and your own preferences.