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From Car to House (Del coche a la casa)

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DURING THE COURSE of our collaborative research we have been led to reflect again and again on the division between method and theory in anthropology. Endorsed

by anthropologists from positivists to postmodernists, the duality holds an established place in anthropology's training programs and theory of itself. In recent years, with the elaboration of methods and the proliferation of theories, the separation has broadened as well. But we think the division is unfortunate, because it signs away anthropology's unique ability to join two moments of life—"being there" and "being here"—and it relinquishes the opportu-

nity to link these to a theory of culture and learning.¹ We have also come to doubt the accepted wisdom that the ethnographer is both a participant and an observer. This conceit—which must raise questions about what anthropologists actually are doing, because each task appears to exclude the other—is closely tied to a post-Cartesian theory of knowledge which is dominant only in certain cultures.² We would replace the two dualities by seeing anthropologists as artisans fashioning ethnographies by forming communities and making conversations.

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We have worked together in Colombia and Guatemala. This joint work was first tried in 1977, but not until 1984 were we able to undertake it on a continuing basis. Our collaboration began more by accident than by design. Born and educated in Colombia, Rivera came to the University of Minnesota for postgraduate education in linguistics. After completing his master's degree, he shifted to anthropology and undertook fieldwork among the Wayuu of northern Colombia. Gudeman, who was born in the United States and educated there and in England, teaches at the University of Minnesota where he served as Rivera's adviser. Our joint research started after both of us had independently undertaken fieldwork, alone and with our spouses. The experience gained from our separate histories has enriched our collaborative work, for each of us draws on past ethnographic knowledge to help formulate and interpret the joint research. We also had learned that the stresses caused by intensive fieldwork can rupture a relationship. In fact, we have never quarreled during the research (or outside it). At the outset, we also agreed that if the collaboration did not prove stimulating and enjoyable, we would separate and each do something else. This frankness about the content and purpose of our collaboration, the fact that the partnership was never a necessary commitment for either of us, and the ever-present possibility of continuing or terminating it created a context of freedom and affirmation that we retain even today. Certainly, our interaction began within a university setting where it was marked by a power difference, but our tacit ground rules have allowed us to build an equalitarian community and to evolve our own style of research. Through time, we have built an interdependent relationship and an independent dialogue that is valued for itself. Freud described the task of the analyst and patient as that of unburying the historical past (like archaeologists); but in recent years, post-Freudians have redescribed this task as that of creating narrative truths within a dialogue (Spence 1982). Much the same pragmatic stance characterizes our collaboration. Given our past personal and intellectual histories, we each undoubtedly brought a pragmatic perspective to the collaboration. One of us was raised in a heartland of business and

theoretical pragmatism (Chicago); the other grew up in a center of everyday pragmatism (Bogotá). But it was in our working relationship that the pragmatism emerged and flowered, reinforced by the rural people from whom we also learned it.

In our collaboration, considerable learning occurs that has little to do with fieldwork or anthropological theory. Only a small portion of this learning will reach printed form, but it plays an important role in sustaining our relationship. Each of us has become willing to try out any idea, nascent or absurd, knowing it will be received and discussed—if only because this helps to pass the time on a journey or while waiting for someone to meet us. Segments of these conversations have emerged in print, but we value them for themselves; they are the “commons” or shared reserve of our community. We seldom claim “independent authorship” of an idea in this community, for ideas emerge and are fashioned in conversations. Between us we certainly acknowledge the conceptual and practical contributions of one another; and we well know that each of us has provided distinctive donations to our commons. But not only are we shy of keeping accounts, we are wary of giving a public accounting, for nothing would more quickly shatter an interdependent relationship than competitive claims of personal authorship. This mutuality does have limits, however, and we often mock it ourselves. The slightest misstep, such as falling into a stream or mud but preserving precious field notes, contracting dysentery, or ruining the gears of a car, is carefully noted and never forgotten, because the other's misfortunes can always be invoked to critique his arguments and justify one's own. When something beyond our joint control goes awry, each blames the other in language that often ends with a double entendre invoking a gross mistranslation between Spanish and English. Our conversation shifts rapidly between the serious and the jocular, but this keeps us engaged in the research task.

Our first research in Colombia was carried out during the 1980s. Focused on peasant life in the Andes, it led us to formulate several ways of thinking about the Spanish-speaking margin and collaborative work (Gudeman and Rivera 1990). We used the metaphors of cars and of conversations to speak about “field techniques” and the processes in which we were engaged. By conversation we meant both verbal actions and nonverbal practices. We emphasized the practice side as opposed to texts and tried to explain how anthropologists are caught up in “long conversations” involving their talk and recollections as well as a people's conversation and memory. We argued that texts and voices become intertwined over time as they appropriate from and combine with one another. A few commentators disagreed with the multiplicity of meaning and practical significance that we attribute to “conversations” (Hoekstra 1991; Rappaport 1992). Our sometimes ironic use of the figure of a car to explain how

we interacted in fieldwork also was misunderstood (Roseberry 1992). The car was a metaphor for discussing our research interactions and for suggesting that the anthropologist is only a guest when in the field: in fact, conversations between the two of us also took place while we sat in rural houses, climbed to potato fields, drank coffee, and relieved ourselves; conversations with rural folk took place in multiple locales too, wherever they were engaged in an activity such as stringing tobacco leaves or cooking potatoes. In 1989, as the Colombia work drew to a close, we began new research in Guatemala, on a different topic and under different conditions. But this later work drew on our earlier conversation, and it has led us to consider more thoroughly the implications of our research methods.³

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Fieldwork is a process, an education, and a theory in action. Through working together we have come to see field research not as a set of techniques—such as deploying and refining the definitions of *Notes and Queries on Anthropology* (1951), collecting genealogies, or carrying out household surveys of wealth—but as *un modo de hacer* (a way of doing), to use a Latin American phrase. Much the same holds for our understanding of culture. We did not try to collect unimpeachable data to assemble into an ethnography because we did not think that human culture can be conceived as a set of rules, collection of roles, or series of values. Culture does not await discovery by application of a technique. And because culture is not an entity to be found, we did not try to make ourselves invisible during fieldwork so that the objective situation might better be seen. Only the belief that there is a true culture, which the anthropologist is obliged to find, compels the ethnographer to try to be invisible and efface her subjectivity in fieldwork.

Culture is an act of artisanship. Learned in the company of master practitioners, culture is an accumulation of experience. Anthropology is similar. The anthropologist should not strive to reproduce a culture in texts, not because verbal performance, music, dance, and ritual are transformed by the act of writing them, but because the idea of reproduction is misleading. Ward Goodenough once defined the complete ethnography as containing all the information one had to know in order to behave acceptably among a people studied.⁴ But the anthropologist is not a passive, objective observer of another formed life. Fieldwork is an encounter, and the anthropologist participates in making ethnography.

Consider an example. Elsewhere we have described the economy of the rural folk in the Andes of Colombia (Gudeman and Rivera 1990). We argued that people use the metaphor of a house with doors, walls, and a foundation to enact and talk about their material activities. Each

term of the metaphor refers to both a physical and an economic entity. For the people, “the base” (*la base*) of the house designates the underpinnings of the dwelling and the wealth of the householders. When we first explored this part of their metaphor, we became puzzled, because every item of wealth surely has to be made by somebody using something. Each base presumes a prior one. One afternoon of a fiesta day, as we stood with a group of females and males eating hot bread that was emerging from an outdoor oven, we posed the question. More exactly, we took turns trying to frame it, for we had not until that moment fully understood what the question was or how to ask it. After several tries from one of us and then the other, and after incomprehension from the group, Rivera finally framed the question in a way they understood: how did the first base start? Moments of silence followed before one man laughed and said that our question made them feel as if they had no base, because they could not answer us! Then, the question was slowly restated among the people, and eventually one man said that the first base must have been the Garden of Eden to which a woman responded that another base had to be formed by the children of Adam and Eve, and again after the Flood. There were murmurs of agreement.

Consider this process and these responses as part of a long conversation. Our question, directed to understanding them, actually was constructed by drawing an analogy between our term, *capital*, and their term, *base*. The point that amassing material capital requires a prior accumulation on which to live had once been made by Joan Robinson (1962), so in trying to understand their practices and verbalizations, and in posing the question, we were drawing on a developed, textual conversation. The Colombian folk found us equally hard to comprehend, with the difference that they assumed we had powerful, superior knowledge, as suggested by the comment that we made them feel as if they had no base. For their contribution, they too drew upon a long conversation, but one that was grounded in religious imagery and the Bible. Only after leaving the field did we realize that their response provided a special commentary upon ours, for Joan Robinson, in explaining the accumulation of capital—by linking it to the development of a class structure and appropriation of surplus—had cited a peasant ditty from Europe (Robinson 1970:55):

When Adam delved and Eve span
Who was then the gentleman?

But there is a larger question: Whose culture was produced in this conversation about the base? We certainly would not claim that this gathering of people on an afternoon in the 1980s produced the “right” answer to our question, for we do not know if their response had been given before or if the question had even been asked. More

important, our question to them was formulated with reference to a long conversation of ours so that their response was not so much an answer to a question "about" their culture as a contribution to a passing discussion engaged in by themselves and two anthropologists who also came from different cultures.

In this sort of fieldwork, ethnography is produced, but it is not only about the culture of "the other." In fieldwork, the anthropologist is not journeying to another culture, learning it, and translating it in order to bring it home. In anthropology the "translation problem" was posed by Evans-Pritchard and developed as a theoretical issue by his students (Beidelman 1971; Crick 1976). According to this view, the anthropologist's task is to translate in the broadest sense between two separate and incommensurate worlds. We would sidestep the translation problem, because it is founded on the modernist dichotomy between the supposed certainty of knowledge that we can possess only about the self and the uncertainty of knowledge that we must always entertain about the other. The translation problem is part of an epistemology in which discovery, replication, testing, verification, falsification, and the objective presentation of findings are thought to be key moments in the quest for knowledge. For anthropologists, the translation problem also implies a distinction between observer and observed which the ethnographer must strive to overcome through participant-observation so she can warrant her findings and the veracity of the ethnography. But fieldwork is not like this. The anthropologist produces ethnography *with* a people, and the anthropologist is as much (or more or less) a part of the ethnography as they. Doing ethnography is joint work, teamwork, not a discovery, and not an interpretation or translation of a preexistent, fixed culture.

Doing fieldwork, which we think is the crux of anthropology, is a way of learning and conversing. The anthropologist brings to the field experiences and expressions, only some of which come from anthropological texts; these are the components with which a research style is developed. "Theories" and "knowledge" in anthropology—about kinship, godparenthood, patterns of religion, or forms of social organization—do not make up anthropology, for anthropology is not a bounded knowledge system. And the methods of anthropology can never be encompassed by knowledge about how to do surveys, draw maps, take photographs, make recordings, transcribe myths, weigh harvests, make videotapes, or sample the amount of time a people spend working. These are only instruments for doing ethnography, which is a form of artanship.

This returns us to a "theory of culture." In the understanding of fieldwork that we advocate, there is no gulf between a theory of culture and the method for studying it, for by our theory, culture is "a way of doing" as rural people in many parts of Latin America continually told us.

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Given this perspective on anthropological method and theory, collaborative research assumes special importance. As a single worker, the anthropologist is like a solitary craftsperson, but culture is made by a community of people. Rural inhabitants of Colombia often said the conjugal pair is like a team of oxen pulling a cart together (*jalar la carreta juntos*), because to sustain their house the two must work jointly, with each providing the effort of which she or he is capable. Culture also is the teamwork of many, and so is ethnography, for it involves the cultures of several. Collaborative fieldwork is one way of making apparent that ethnography is a joint making and that anthropologists are embedded in multiple, destabilizing conversations.

For most of this century, anthropologists have been burdened with a myth, at once misleading and distracting. After the example of Malinowski, we have come to honor the lone, solitary fieldworker who undergoes a rite of passage (from "us" to "them") in the field and collects erudite information not otherwise obtainable. The intent is to be invisible among a people so as not to disturb what they ordinarily would do; we do not want to break up their routine, because otherwise we shall never know what it would have been. The anthropologist should be sanitized and enshrouded in a white lab coat, otherwise the report will be impure and contaminated. Anthropology has been living with a disease metaphor of itself: anthropology as an infection, if not an agent of imperialism. Joint fieldwork is all the worse, because a team of researchers will intrude and fail all the more to become participants in others' ways of life.

But the "great man" myth of fieldwork is an ironic commentary by ourselves on ourselves, for it does not fit the discipline's own major themes: that humans are embedded in a play of reciprocity, that human society is not reducible to the acts of individuals, and that human collectivities develop distinct cultures. Anthropologists study people in groups and resist the idea that social patterns derive from individual volition, but they have adopted a very different idea about fieldwork, which they claim should be undertaken by a single person working alone over a long period of time.

In our view, anthropology is done within a community of inquiry, and this collectivity is multiply defined: it is a community "at home" and "out there" at once. Sometimes the anthropologist is physically located "there" and sometimes "here," but the two together locate her. Joint fieldwork makes manifest this multiple location and alters the experience. According to the accepted paradigm, the anthropologist goes to "the field" for one or two years and then returns to digest his notes and write his findings into a text. The two moments are separated, and the entire process may take 20 years or more, as Marcel Mauss once

warned Meyer Fortes (Fortes 1987:247). By this spatial separation, two forms of knowledge are distinguished: raw, empirical learning in the field, and critical reflection within a theoretical community at home. This division has also been accepted by postmodernist writers who see fieldwork as the “dumb” recording of others’ voices (Clifford 1988:45; Rosaldo 1980:16, 42–43) or as part of the two-stage process of “being there” and “being here” (Clifford 1983, 1988). By their construction of fieldwork as a form of Lockean experience or writing upon a blank slate, however, postmodernist critics deploy the very epistemology they seek to overturn. Our emphasis upon joint research helps to collapse the distinction between fieldwork and analysis, participant and observer, ethnography and ethnology, because field research itself becomes group learning.

Collaborative research certainly quickens work in the field, through the mutual sustaining of effort, but it especially promotes collective learning. We found that as we tried to articulate to one another what we had learned, our verbalizations and “generalizations” were often divergent, and this forced us to consider knowledge not only as a mental abstraction fitted to a conceptual framework but also as experience in the fingertips. New knowledge is not always gained through reading or listening to summaries of practices. Knowledge of how to farm, seed, harvest, practice a ritual, or act in the family is an accretion of learnings. The rural people learn these things by doing them over time, and this is how we learned from them. We were hardly passengers in a car, passively viewing the landscape. But as we learned from them, we learned from each other, for we practiced our nascent knowledge on one another, and it altered in these trials. We also learned by listening to the questions and puzzlements of each other, and by watching as the other learned. Learning in the field was shared and could be labeled not “what I know” but “what *we* know,” because what each of us learned was dependent upon the other as well as on the people. We have practiced on one another the ways of scooping boiling sugarcane juice, stripping a vine, and cooking potatoes; we have talked about ways that a market depends on a social community for its continuance; we have listened to each other be confused; and we have taken our learning back to rural and urban people for their commentaries. Our personal histories and knowledges certainly helped frame this sort of learning, but they compose only part of the totality. So, whose knowledge is this? Their knowledge? Each of ours? The two of ours? Or, all of ours? Some folk metaphors, again, proved useful in formulating our activity. Together we were a team of oxen. In the rural economic model, everyone must aid or help in the common project of supporting the house; by contrast, in the market model everyone works competitively and receives a return that is proportionate to the value of her efforts. In the Latin American house model,

no one keeps “account” of separate contributions but the work of everyone “counts.” In the modern model of the corporation, work does not “count” unless it is “accounted.” The house model of joint effort, in which sustenance is achieved as a mutual product, provided a useful figure for conceptualizing the community of learning that involved them, us, and our own interactions.

Project research militates against the production of “autobiographical anthropology”—constructing and using the other in order to say something about the self (see, for example, Rabinow 1977). Collaborative fieldwork (especially when team members are from different cultures) also helps prevent constructing the other as exotic in order to portray the self as experientially open. Such exoticism of the other combined with personal reflection creates a genre of self-absorbed anthropology that is as objectionable as it is undisciplined. Project research that emphasizes group learning is very different from the normal atomism of the higher learning in the discipline.

A research group builds a joint memory; over time this accumulation of shared knowledge also influenced our experiences. For example, one afternoon in a marketplace of Guatemala, we spoke with a man who had a small, family-run shoe store. At the time, we were exploring the local concept of profit. The man told us that he had no gains from the store, because he had to use all the returns to meet “the necessities” (*las necesidades*) of his house. We both registered his use of the word necessity, but upon casually discussing it afterward, we discovered that it stimulated different memories and reflections, and each of us attributed a different significance to his comments. Gudeman thought the man’s conversation implied that household needs are fixed in amount, but in reality they fluctuate and are socially determined. Therefore, the next question to be asked was, What set the level of household needs and what happened when store returns superseded this level? Did this signal the emergence of surplus or profit? And were these sums variably, even arbitrarily, defined? Rivera observed, however, that the man’s use of the expression “household necessity” was the same as Gudeman had recorded in rural Panama 25 years earlier, but it was being used in an urban, business context of Guatemala. For Rivera, this suggested that a rural house model was possibly brought to the city by the folk and initially used to construct the market context. If so, was this not support for our argument from our work in Colombia that a rural house model is widely shared in Latin America? When joined together, the separate pieces of communal thought and memory, which made sense only to the two of us and were stimulated by the shared experience, led us to return to the man. We found that he had grown up in a rural area of Guatemala, that he had recently moved to the city, that the level of household necessities does fluctuate, and that he was using other rural images in his conceptualization of market profit. More important,

this joint memory helped us to see how profit or gains in the house business may emerge through acts of household thrift—but that is another story.

Communal memory constructed and adduced in collaborative research destabilizes the position of the anthropologist. Postmodernist writers have raised the useful point that the anthropologist is a positioned observer and writer (Rosaldo 1989). The thesis is a critique of the Cartesian distinction between neutral observer and observed. But the situation is even more complex than postmodernists allow. The anthropologist brings several perspectives to the field, and these are not simply reflections of a personality, for they are fashioned within communities. The anthropologist, like the people she is privileged to study, is a member of several conversations. Undertaking fieldwork as a team heightens awareness of the multiple conversations on which anthropology depends. This is not to argue for interdisciplinary fieldwork, although that can have its uses, but to urge the need for the multiple voices of anthropologists to be joined in focused research, for this brings the dislocation of perspective directly into empirical work or the process of observation that is never separate from its theorization. This depiction of research, which counts upon the unaccountable, is different from standard narratives about scientists performing an experiment with controlled variables so that it can be replicated. The anthropological process demechnizes fieldwork and makes it into a true experiment. We have been told that this is an “experimental moment” in anthropology (Marcus and Fisher 1986), because ethnographies are being written in new ways. In our view, this is a one-sided representation of the field. The experimental moment is not only about how texts are being written but also about how ethnography is being done. We certainly agree that many dimensions of human life—such as the place of women, the elderly, and non-rule-governed acts—were “silenced” by earlier, nonreflexive forms of anthropology. But we strongly disagree with the postmodernist silencing of research as an activity whose course changes in the act. “Doing ethnography” is a complex project; as rural Colombians and Guatemalans say of life in the house, they do things *al tanteo*—by trying, feeling, adjusting, and experimenting in the process. Anthropology is fortunate in being home to the pragmatic inquiry of life that itself is pragmatic. The anthropologist certainly is positioned—as a socially placed artisan.

Our perspective, thus, lies some distance from views that assume the anthropologist’s position is fixed, even if multiply determined. For example, a postmodernist sometimes relies on reflexive and intellectual deconstruction of her positioning in order to achieve self-understanding. But this implies that the anthropologist occupies at least one position from which to deconstruct. It both reinstates the subject-object dichotomy or Cartesian ego and leaves out practice in the communal activity of fieldwork with its

role in dislocating the anthropologist. Seeing ethnographic inquiry as a form of pragmatism also does not rest easily with normal tales about science or even with the ideology of other social sciences, such as economics and psychology. But pragmatic research is what good ethnographers have been doing for three quarters of a century—following Malinowski’s example. Fieldwork is always an experimental moment but not a “scientific experiment.” Doing anthropology means participating in a complex, conversational community in which ethnographers learn and make by working “here” and “there” at once.

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We have used various metaphors from our research to convey this way of thinking about anthropology. From rural folk in Colombia and Guatemala we learned about teams of oxen who pull together. Drawing on our own experience, we used the image of a car to convey some of our practices in relation to people in the field, ourselves, and the past. We knew, of course, that mentioning use of a car in the field might despoil every image anthropologists have striven to build: the necessity of not being too Western; the importance of being as “native” as possible and not displaying one’s relative wealth and power; the need to deny oneself the possibility and luxury of leaving the field at will (in order not to turn anthropology into tourism); and the necessity of establishing empathy with the life of others. We spent, of course, an enormous amount of time in other field experiences, and we “suffered” the indignities to which all anthropologists are susceptible, but we knew that referring to the use of a car while also describing the relatively equalitarian way the two of us cooperated across different cultures, different ages, and different professional positions could be taken as a contradiction: our practical use of a car might undermine what we were trying to establish with the metaphor of it.

We used the car metaphor to try to talk about the way conversations are both bounded and unbounded and the way that memory helps constitute experience: we wanted to display how our method was linked to our theory. We also knew that metaphors are sometimes self-contradicting (Beardsley 1981[1962]), and so we used the car imagery in a self-mocking fashion, to make light of what we were claiming to have achieved in the field; to be honest, we were also mocking some members of the profession, because we thought that our work displayed new insights into the rural life of Latin America, and we were suggesting that despite the “disability” of using an automobile and conducting fieldwork in an unconventional fashion, we had been able to gain something not achieved by others. But there were other reasons for using the car imagery. If metaphors, in I. A. Richards’s (1981[1936]) strong phrase, are “vehicles” for communication, then surely an automo-

bile is an apt metaphor for speaking about the use of metaphors.

We were also conscious of using a car in areas of Colombia where intrusions of people, technologies, and goods have been experienced since the European invasion; in fact, the culture of this region is composed of intrusions, including the invasion itself. In this context it was appropriate to have and use a car, because what we were doing was little different from what had been experienced for hundreds of years. Rural areas are a shifting mix of people, objects, and practices. We stayed in, ate at, and visited houses that were made of mud-plastered walls and straw roofs, yet contained ten-speed bicycles, wall calendars with pictures of the Alps, and transistor radios. We talked with people who ate home-grown potatoes three times a day and wore designer-label jogging suits on Sunday; occasionally, we would see people farming with wooden implements while listening to pop music on headsets. The rural dwellers live in multiple communities and engage in complex conversations of which a car is a part; we were strangers using an automobile, part of ordinary experience.

This brings us, finally, to the title of our essay, "From Car to House" or "Del coche a la casa." In using the image of a car to talk about our research, we wanted to emphasize that anthropology is a many-sided conversation, open to new voices. It also seemed to us then, and even more so now, that undertaking collaborative research is a political and moral act. Because we ourselves made up a community, the emergence of political views between us was inevitable. But the political implications of collaborative research have more to do with method than content, because collaborative work builds an equalitarian and democratic community of inquiry (Bernstein 1991; Rorty 1989, 1991). In this type of community, everyone's assumptions are put at risk. No discourse can be hegemonic, for conversation continuously modifies the views of participants. The commitment to participate in this type of community is a moral act, because it is founded on trust, and a political act, because it implies a willingness to be persuaded by others. This way of making community characterized not only our interactions but our encounters in the field as well. For us, doing ethnography could never be like reading a text or recording and preserving the separate voices of others, as if these were pure echoes of history, uninfluenced by peers and the recorder. Fieldwork means engaging in conversations whose results are unpredictable; as rural folk in Latin America might say, fieldwork is *hacer conversación* (making conversation).

To convey this dimension of our research, metaphors about cars and conversations are not satisfactory because we need to encompass more completely the anthropological activity and its product. We think a useful image is provided by the people's metaphor or model of their house economy, which was the focus of our initial col-

laboration. Doing fieldwork and producing an ethnography are like making a rural house and its economy. One begins with a few resources and a general design, but the shape of the "house" changes as other resources are found and as one assesses the emerging "building," seeing weak spots, thinking about what others have done, and listening to reactions. Consider the people's metaphor and practices concerning the "base" of a house. This figure assumes great importance in many parts of Latin America. The idea has variant expressions; for example, in Panama it seems to have been more nearly expressed by the word *fundamento* (foundation or basis). In general, the base refers both to the physical understructure of a house and to the material wealth of a domestic group; it can also mean "money." The base has other implications as well. The person with a base has a strong and able character; she has a secure foundation like a well-built house. The base of the house economy also includes a variety of materials. We often found that rural houses preserve and keep items that seem to have no use; lying about in the patio or loft are discarded cans and tools or old pieces of iron and plastic. City folk read this as a sign of peasant carelessness and disorder (*muy descuidado/muy desordenado*), but these bits and pieces are tangible signs of past activities, and they compose part of the base or reserve. Like the anthropologist's ad hoc collection of methods and theories, they may someday find their uses. Reminders of past accomplishments, they are objects of play for the practical imagination (Lévi-Strauss 1966). Every base contains materials unspecified by or to be constituted by the practices of humans. The base is, as the people did suggest, the Garden of Eden, the undesignated world that we still have and can draw upon today. A base is the cornerstone of a rural economy and rural living. We suggest that undertaking ethnographic research is like gathering a base and building a house with its economy.

Domestic economies and anthropology are connected in another way as well. A base can be started with the smallest of savings, such as a chicken or hog. A hog is often known as a "coche," from the word *cochino*; but a normal term for car is also *coche*. Rural folk often play upon the words and conflate the two by homophony so that a car (*coche*) is a hog (*coche*); thus, our car was our hog—for them and for us—and it was the base for our research. Still, while hogs (*coches*) are part of the base and are maintained at the house, they are not members of it and are kept outside the living and eating quarters. The term *cochino*, in fact, may stand for the undomesticated or uncultured—that is, the "pig"—which is sometimes an apt description of the anthropologist. For us, then, ethnographic work began with a coche—a car of "uncultured" persons—but our intention was to make this "pig" into a base for a house, or even a great house, which is an *hacienda*. Like an hacienda, every ethnography has a different shape and content, depending on the meander-

ing course of the making; and an hacienda, as a place and expression of a life, has no finality. Just as the word *hacienda* (from *hacer*, "to do," "to make") signals something in the making, so an ethnography is never finished.

Ethnographic fieldwork, at once method and theory, has never been the passive reading of a social text. Field research, especially collaborative work, is an active conversation. When we judge fieldwork, we judge what was done: we may like or appreciate what was produced; we may find it tasteful or shocking; it may fit our expectations or it may stimulate us to think in new directions. We may also consider how the artisan gathered her materials, worked with them, and developed her craft. Certainly, in making ethnography, we can become more accomplished artisans by drawing on known skills and developing new ones. But we do not say that a house or chair is true or false, and this should not be the criterion by which we assess ethnographies. Because the leading edge of anthropology will always be field research, perhaps it is now time to consider new ways and different communities for judging and doing it.

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Notes

Acknowledgments. For those who would keep account, we first discussed the ideas for this essay one afternoon in highland Guatemala while we walked a freshly seeded field and then sat in a sweat bath. The title was suggested the next morning over coffee by Gudeman, who liked the sound in Spanish and had in mind the shift in metaphors that we might use to describe our changing perspective on research. Amused, Rivera connected the two Spanish metaphors by pointing out their mutually "pig-gish" meanings, then we jointly elaborated on their entailments. After the essay was drafted, we presented a version at the 1993 American Anthropological Association meetings in Washington, DC, and later we discussed it both there and in Minneapolis. Still later, it passed between us by Internet. During this process, Timothy Dunnigan, Alma Gottlieb, and Andrew Strathern provided helpful comments.

1. Our use of the expression "being there" is meant to evoke Clifford's (1983, 1988) provocative essay on Malinowski and the "authority" of the ethnographer; as we shall elaborate, we do not think that Clifford actually addresses the question of "being there." D'Amico-Samuels 1991 provides a recent and different discussion of this issue.

2. The angst about the possibility of being both a neutral observer and a committed participant—about being a theoretician and a practitioner—seems to lie beneath the surface of Geertz's 1988 discussion of ethnographers and their writing.

3. For a different and interesting use of conversations in collaborative research, see Pálsson and Durrenberger 1992. Pálsson provides a more thorough discussion and treats the

linked issues of translation and "the other" in a later publication (1993). His notion of "enskilment," especially of the anthropologist, is similar to what we mean by artisanship (Pálsson 1994). In an interesting analysis of ethnography's history, Sanjek (1993) shows how collaborations between ethnographers and assistants often have been hidden from view. His conclusion that "ethnographers and assistants together made anthropology" (1993:16) fits our argument. For an enlightening discussion of changing ethnographic methods in relation to political considerations and global anthropology, see Kuper 1994. More recently, Gottlieb (1995) discusses several interesting forms of fieldwork collaboration that are different from our own.

4. See Goodenough (1957:167, 1970:111). In fact, Goodenough initially defined culture—not ethnography—in this way. In Frake's formulation of this thesis, an ethnography "should properly specify what it is that a stranger to a society would have to know in order appropriately to perform any role in any scene staged by the society" (Frake 1964:127).

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