Traditions and Transitions:
From Market Women in the Andes to
Adoptive Families in the United States

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BECOMING AN ANTHROPOLOGIST

In 1974, as a junior in college, I wondered if the anthropology major I was pursuing was a good fit. The 1950s ethnographies that dominated my course readings had a weighty flatness that numbed my genuine curiosity about cultural differences and similarities. I had pursued anthropology as a major because I had grown up outside the United States. While born in Washington, DC, at the age of fifteen months I had been taken by my Euro-American parents to Japan, where I lived for almost eight years.[1] I lived in a Japanese neighborhood, went to a Japanese nursery school, and had Japanese friends. I spent close to another three years in Thailand at an international school and then returned to the United States just in time to be an awkward seventh grader, with almost no knowledge of American popular culture. This was intriguing, if painful. I was "American" but knew not what that meant. Hence, my informal interest in anthropology and, subsequently, my pursuit of it as a major in college.

I finally shed my ambivalence about whether or not I wanted to be an anthropology major after a semester of field research in Peru. As part of a "study abroad" group, I went to a monolingual, Quechua-speaking region in the southern Andean highlands, where we worked on independent as well as collaborative projects with students and faculty from the National University of San Antonio Abad in Cuzco. I stayed on in Peru another three months after the semester ended, immersing myself in life there. Thus serendipity mostly

1. My father was a career foreign service officer and Japanese language specialist.

explains why I became a Latin Americanist, specializing in the Andean region of Peru, rather than a scholar of, say, East Asia, where I'd been raised.

Over the years, I returned time and again to the Andes, to multiple sites, working on many different projects—including the symbolism of textile motifs as a visual language (Seligmann 1978), the history of textile production and exchange (Seligmann and Zorn 1981), and the intersection of oral traditions and the changing environmental knowledge embedded in irrigation systems (Seligmann 1987; Seligmann and Bunker 1994). At one point, I embarked on pilot field research in Ecuador, where a major movement to revolutionize education in indigenous highland communities was occurring, in conjunction with a pan-Indian wave of struggles to transform the institutionalized political landscape of Ecuador. However, the combination of political upheaval, whose signs I could not read well, and the challenges of learning a new variant of Quechua, persuaded me to return to the Peruvian Andean highlands.[2] Nevertheless, I learned a surprising amount from my brief foray into Ecuadorian culture and politics. Not least was my growing awareness of why most Andean countries had emerging indigenous movements, whereas Peru did not. The value of comparative research became more apparent in a way it had not been to me before, steeped as I was in an area studies approach to anthropology.[3]

CAREER AND LIFE TRAJECTORIES

My first book was based on my doctoral research on political struggles in the Peruvian Andes and the relationship of a radical agrarian reform to Peru's violent civil war in the 1980s and 1990s (Seligmann 1995). I conducted most of that research in the countryside. I then made a major shift, documenting the lives of market women who themselves straddled city and countryside (Seligmann 2001). In *Peruvian Street Lives*, I wrote a series of vignettes, embedding theory in stories, in order to reach a broader public (Seligmann 2004).

2. There are four variants of Quechua. While in some cases the roots of words are the same, the diacritics that make the difference in the grammar of the variant differ markedly. For those fluent in Cuzco or Cochabamba Quechua, which are more or less mutually intelligible, acquiring fluency in Imbabura Quichua or Ayacucho Quechua would be akin to learning a new language.

3. I had chosen to pursue an area studies approach in my graduate studies, first attending the Institute of Latin American Studies at the University of Texas–Austin for my master's degree, with concentrations in anthropology and Spanish-American literature, and then pursuing a doctorate in anthropology at the University of Illinois–Urbana, whose anthropology department had a renowned strength in Andean studies.

After having spent close to twenty years participating in the professional activities of the discipline, I had achieved a level of respect I found gratifying as a Latin Americanist. Within the Andes, I had painstakingly cultivated rapport, and although I still felt ignorant about some things, I grasped subtleties that characterized the complex political and social terrain of Peruvian society, within and outside academic circles. That familiarity formed a critical backdrop for all the diverse research projects I had undertaken. I had also made very good friends in Peru, with whom I still maintain contact.

After my family expanded to include my husband (who works outside the academy) and our daughter, whom we adopted from China, life cycle demands compelled me to reflect on the course of my career. There were other catalysts as well. I felt a need to refuel. Continuing to crank out papers and articles on the subject matter I was familiar with would be relatively easy, but my heart was not in it. I was also older, with less tolerance for harsh conditions that sometimes left me severely ill in the field; moreover, as a woman who had become one of the few full professors in my department, I had myriad administrative responsibilities.

One day, a surprising letter arrived in the campus mail from the dean. I had been selected as the recipient of the tenth annual faculty scholarship award and asked to give a major lecture as my reward. The dean and most of my colleagues expected I would deliver a talk on my work in the Andes. Yet I felt unwilling to talk about material that, while of interest to others, concerned issues about which I was no longer passionate. In fact, I found myself in a liminal state. Growing out of my husband's and my decision to adopt a girl from China, I had, over the previous three years, been doing informal fieldwork on a new set of issues . . . but I had not consciously acknowledged it. The dean's invitation inspired me to declare my new interests as a formal research project: comparing and contrasting the experiences of several types of families—those who had adopted children from China and from Russia; non-adoptive family members; and transracial families in the United States who had adopted African-American children. And so I took the scholarship lecture as an occasion to embark on my new project: an examination of competing models of family formation in the United States.

My choice of subject matter for that campus lecture compelled me to acknowledge something else. Perhaps partly as a result of personality, my research is propelled as much by passion, curiosity, and autobiographical experience as by attentiveness to the quality of the fieldwork and analysis of it. While these factors might have been more obvious with my new research project on adoption, they had also been relevant to my earlier experiences

in the Andes, especially with respect to my work on market women. I had traveled with Peruvian women traders time and again over the five years I had spent doing research in Peru on other topics. Although perhaps not immediately apparent, my interest in market women had been catalyzed by my own autobiographical trajectory—in that case, as a straddler of worlds, an American raised largely in Japan. As a transnationally adoptive mother myself, my new research interest in transnational and transracial adoption was even more obviously integral to my life history. The task was to bring anthropological knowledge to bear on what is, for me, clearly an intimate topic.

As Paul Stoller observes in the afterword to this volume, many of us may reconstruct the research experiences that constitute our career trajectory as a "straight highway" when in reality our journey is hardly linear. Often, we take side roads, sometimes major arteries, and occasionally a detour or path that leads to a dead end. These routes are indicative of both continuities and disjunctures. Perhaps more subtly, they are also a reflection of what one hopes research is—the outcome of a spark of curiosity, and the process and result of labor invested in a genuine quest for knowledge.

TRANSITIONS, UNKNOWNS

Like many anthropologists who are both inside and outside their culture, I knew that I could survive half out of my skin elsewhere. I read and thought about my informal research and experiences and synthesized them for the scholarship lecture. It was exhilarating to be working in a new area . . . but also more terrifying than I recall my first field experience to have been in the Andes. Ironically, I had no "safety net"—no familiar landmarks and faces, no mentors, no token books or keystone articles as fetishes, and few traces of memory to guide my way. Abstractly, the methods, the process of field research, and the memory that ethnography is at once process and product were principal commonalities uniting my two fieldsites. I knew how to ask questions and how to listen to silences; how to connect dots and follow flows in a nonlinear fashion; how to move from talk to practice, and from history and process to representation.

Yet this particular ethnographic project was alien in ways that brought me up short . . . but that also intrigued me. I experienced the excitement of not knowing, of learning, and of serving as translator and mediator. Working in my own culture on questions that entailed barging into the intimacy of family life and struggling to locate interlocutors, some of whom were dead-set on becoming invisible, was more difficult than any of my prior fieldwork experi-

ences. Once, at a group dinner at an annual anthropology meeting, I sat with a colleague who had known me a long time and respected my work on the Andes. When I told him about my new research, he responded, with some hostility, "Why would you want to do that? It's so private." It turned out that he himself was adopted. At the table, as well, were another colleague and his wife who had adopted three children and fostered several others. I had also adopted a child. Yet we did not talk about adoption at all after that initial exchange. Was it because the personal and private collided with the public and professional? Or was it because of our discomfort with the liminal interface between work and sociability, in which, rather than chatting as colleagues and friends, I would be cast as anthropologist and they as informants? In the latter role, they might find themselves thinking (out loud) anthropologically about their family lives—something they had not yet done. Or, perhaps, it was something to do with the topic itself?

I was working on subject matter that was as immediately significant and meaningful to me as it might be as a subject of scholarly importance. I was living it, but also trying to make sense of it anthropologically. My research was poised between extraction, dialogue, and autobiography. Too many such endeavors have gone awry or had negative consequences. Both my professional and personal identity and my "personal" life were up for grabs and were entangled rather densely. I wondered how to make my way. Should I return to the fold (of previous Andean research)? Sit on my modest laurels and let the river flow for a while? Or take the plunge? I took the plunge.

So what is there to report? Here, I address what I think are the most important differences and similarities between my prior field research in the Andes and my current project on transnational and transracial adoption in the United States in three areas: gaining a grounding, theoretically and substantively, in the research specialization; the process of doing field research and its analysis; and publication of findings. These areas necessarily overlap, but for purposes of this chapter, I address each separately. In the course of discussing each of these topics, I also suggest what might be common to all sociocultural anthropological projects.

MOORINGS

Despite some theoretical similarities between my past and present field research, the differences are greater. My prior research shares with this project an effort to pay keen attention to the practices, views, and voices of people, while heeding the historical, political, and socioeconomic contexts and insti-

tutions in which they materialize—and which people themselves may help to build, challenge, and transform. But I was originally trained in political economy as an anthropologist. My current work has stretched my mind in new theoretical directions, as I familiarize myself with literature on kinship, transnationalism, social geography, race in the United States, adoption studies, and popular religiosity. I often feel I am taking my doctoral exams all over again, drafting "field statements." My new project has required me to recognize starkly what I do not know and what I want to know, and to enter into an epistemological domain that incorporates humility and a sense of wonder.

Without making too much of the case, I would argue that shifts in field research sites may, on the one hand, be quite difficult for the anthropologist. Clearly, one is expected to do one's homework: learn a new language, if necessary, and certainly familiarize oneself with the ethnographic and historical literature and debates that constitute the field. On the other hand, field shifts may bring new perspectives and theoretical contributions to both an area of specialization and to general anthropological knowledge. Often, claustrophobia can beset fields of specialization. In terms of Pierre Bourdieu's ([1972] 1977) understanding of *habitus*, challenges to doxa—the taken-for-granted "rules" informing the patterning of practices and tastes of a particular field— may lead both to a shaking up of the field (heterodoxy), as well as to some scholars hunkering down and protecting the barricades (orthodoxy), so to speak. Anthropology is characterized by the poly-paradigmatic status of theories in play, such that revolutionary upheaval is exceedingly uncommon, even more so than in the natural sciences where, eventually, the burden of evidence forces institutions and bureaucracies to accept a new dominant paradigm (Kuhn 1962). Nevertheless, we can all point to moments when our foundational knowledge has been shaken. My point is that, as difficult as shifting fieldsites might be for the anthropologist, it is often healthy for the discipline itself. And it may be healthy for the anthropologist as well.

INSTITUTIONS AND NETWORKS

I came into my new research area and fieldsite without preconceived ideas about what it should be. I was also less familiar with the well-established history of the issues surrounding adoption, let alone of the principal figures engaging them. The act of establishing senior-junior relationships may be intimidating but is expected, one of the established cultural schemata and tropes within the hierarchical structure of academia. Senior-senior mentoring, in which one party behaves as a "junior," is more awkward. The networks

already in place tend to be entrenched. Building a professional identity under these conditions feels a little like ritual hazing—sometimes hard to stomach, and certainly humbling. Yet newcomers bring novelties now and then, and that has been the case with my late-in-life entrée into a new research area. I have approached the subject matter with some humility, some naïveté, but also from an angle that has not yet received much attention. Each critique, each polite closed door, draws forth a renewed effort on my part to understand the institutional and anthropological reasons for those practices. It is not something I could have tolerated or easily undertaken twenty-five years ago, but my new project forces me to reembrace what originally animated my research: discovery, puzzle-solving, a commitment to intercultural communication, and, not least, sharing that process with a wider public.

Why "reembrace"? I do not think I am alone in acknowledging that in the course of my two decades, to date, in the academy, I have been unduly affected by the status jockeying and small power ploys of university life. I recall the words of my dissertation advisor frequently: "Never has the pie been so small and the fights so big." Hence, it is no small achievement to put power struggles on the back burner while recognizing how they erode some of the best-laid plans of scholars.

METHODOLOGICAL AND ANALYTICAL UNDERTAKINGS

My fieldsite—or should I write, fieldsites?—lends itself to the use of new methodologies and technologies. Before this project, for example, I had never imagined that, in addition to participant observation, I would do in-depth interviews via telephone and digital recorder, or rely as heavily as I do on modes of information, communication, and data processing via the Internet. Moreover, multisited field research has become one of the key tropes of our discipline. My work on market women in the Andes was multisited, but the multisidedness of my current project on adoption is qualitatively different. It requires participant observation across many different locales and entails a keen attention to the interaction of space and place in processes of identity formation and the constitution of shared cultural values and practices. An equally important, and problematic, artifact of this interview method is the individuation of interlocutors (Balasescu 2007). It is harder to track the interlinked networks and their dynamics that constitute adoption communities, which operate more like what Arturo Escobar (2004: 352), citing the Mexican philosopher Manuel de Landa, calls "meshworks." Yet these meshworks are one of the most important dimensions for an ethnographer to apprehend

in order to understand the competing modes, as well as the malleability, of family formation in the United States (see also Latour's 2007 conceptualization of actor-network theory).[4]

Conversational interviews in which power differentials are not an issue create refreshing yet awkward opportunities for confrontation and commitment. In my current project, the adults with whom I speak are more certain about whether or not they desire to trust me than were *campesinos* (or bureaucrats or scholars, for that matter) in the Andes. If they decide they trust me, they have few qualms about asking questions, and making demands on me and the products of my work. It is much easier for them than it was for people in the Andes to decide whether or not they want to participate in my research, and to feel comfortable telling me their decision. I feel an urgency to comply with the promises and commitments I make, which range from providing book lists and copies of my publications and papers to offering workshops. Collaboration has always been part of my *modus operandi*, but it occupies a more prominent and deliberate place in this research (see Lassiter 2005). I also feel more pressure on me to define my position. Am I "against" transracial adoption or not? Are people who adopt internationally rather than domestically racist? Do I think all adoptions should be viewed as gifts from God? Don't I think being as American as apple pie in a great melting pot is healthier for adopted children than all this attention to the child's language and culture? And so forth.

The observations above point not only to what is involved in my shifting fieldsites, but also to significant differences in shifting from working elsewhere to working at home. It is difficult to judge which has more impact on how my research unfolds. Further, while all ethnographers—myself among them—struggle with ethical concerns, this struggle penetrates intensely and deeply in my current project because of my autobiographical engagement. I have always placed high value on making sure I tell and interpret stories with empathy. Some might call this "remaining balanced." The interpretation of stories is integral to my analyses, and inevitably I have a point of view—but,

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4. As conceptualized by Escobar (2004), meshworks result from the "meshing" of networks, especially in the vastness of cyberspace, and encourage heterogeneity, decentralization, self-organization, mobility, and growth in unplanned directions in response to real-life situations. Some single networks may behave similarly to meshworks; others, as De Landa argues, are more hierarchical and centralized and operate at economies of scale. Because meshworks are emergent, it is difficult for ethnographers to track them simultaneously and to tack back and forth between meshworks and "real life," on-the-ground, communities, ascertaining their relationship.

more, I would like my interlocutors to be able to see themselves without being blinded by anger, resentment, or indignation at my interpretations. They may not agree with my analysis, but I want them to know that I follow their logic and story line, that I understand their concerns and objectives, even when I critique some of their assumptions.

Still, it is harder to achieve this sort of balancing act in one's own milieu. And the clamor for reciprocity is not intangible, distant, or romantic. It is right here, now; these interlocutors generally know how to claim their rights, and they network among themselves. So do all interlocutors, but physical distance combined with center-periphery power relations often make it difficult for them to act on or enforce their claims when they are a hemisphere away from the scholar who writes about them. There is an immediacy to the kind of pragmatic knowledge that is emerging from my current research. Coevality, pushing back against power differentials, and creating space for dialogical contestation, accountability, and collaboration all come to the fore in this study.

ROOTS AND TEMPORALITY

Reflecting on an article about his longitudinal field research project, James Watson noted:

> Anthropologists do not have the luxury of drawing a line in the sands of time and declaring a closure date for our research. Ethnography never ends. Even the demise of the original field-worker does not conclude the enterprise, given the inevitability of re-studies (usually conducted by younger scholars eager to overthrow past paradigms). (2004:893)

Watson commented that his article "was a product of contemporary ethnography; it describes a project that has a beginning but no clear end." In a similar vein, my new project has compelled me to track the families I have been working with through time. Of course, one can do this with all kinds of fieldwork, but the relative physical proximity of the families to my home life makes it easier.

Another impact of time involves how the age and status of the anthropologist shape the research process—whom she most interacts with in the field, and what she learns from them. Not only is all field research a product of intersubjectivity, but age and experience intervene in the way that intersubjectivity unfolds. Over time, this process occurs in a single site as well, but

how we interpret what we participate in and observe, especially after it has acquired a familiar backdrop in ongoing work, may acquire a qualitatively distinct valence if we begin research in an entirely different fieldsite at a different age.

My new project also involves me in a more systematic mode of analysis. In my previous work in Peru, I was always able to "command" my data, coding, classifying, and interpreting them in the intense way that most ethnographers do when working with qualitative fieldnotes. But the nature of my new project on adoption cries out for more systematic analysis, hence I have learned how to use a sophisticated yet flexible qualitative data analysis program (NVivo) that has allowed me to make sense of the information I collect without flattening or reducing it, as many ethnographic software packages do. Learning new modes of analysis takes time, and time is at a premium at this stage of my life because of competing demands, but I have found it worthwhile to familiarize myself with this new methodology.

THE NOVELTY OF SHIFTING FIELDSITES?

The history of archaeology as well as sociocultural anthropology includes a long tradition of shifting from one fieldsite to another. What merits a closer look are the motivations and goals that anthropologists have had for shifting fieldsites, as understood within a historical context. There are many permutations of how and why anthropologists have made such moves. For earlier generations, three principal theoretical orientations prevailed: salvage anthropology; comparative field research (looking at the same phenomenon/structure in different cultures); and universals (Susan Trencher, personal communication).

In the early to mid-twentieth century, American anthropologists such as Alfred Kroeber and Robert Lowie, influenced by the Boasian paradigm of "salvage" research with its emphasis on history, environment, and psychology as principal forces shaping culture, conducted fieldwork among different peoples in the same part of the world, especially among different Native American Indian groups.[5] Boas's personal commitment to puncturing a model

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5. Boas's *The Mind of Primitive Man* (1938) and his compendium of articles, *Race, Language and Culture* (1940), Kroeber's *Configurations of Culture Growth* (1944) and masterful *Handbook of the Indians of California* (1925), and Robert Lowie's *Primitive Society* (1920) exemplify these approaches, though it is important to recognize that Kroeber and Lowie, especially, would shift their views over time. Interestingly, Lowie returned to his roots toward the

of evolutionary hierarchy and racial supremacy, and to demonstrating cultural relativism, was also paramount to his own research in multiple sites in North America.

More comparative field research in different fieldsites has been motivated by several factors over the past century. One was explicitly political. In the early to mid-twentieth century, some national governments employed anthropologists in order to ascertain how best to achieve colonial order; this was especially true for the UK in ruling the British empire. This period of colonial anthropology coincided with the emergence of structural-functionalism as a theoretical paradigm in social anthropology and produced comparative research by such (largely British) anthropologists as Edward Evans-Pritchard, Max Gluckman, Monica Wilson, and others. In Germany, by contrast, museum studies, folklore, philology, and a particular brand of linguistic anthropology encouraged a decidedly apolitical form of comparative research.

Different theoretical orientations underlay another set of midcentury comparativists: structuralism and structural Marxism propelled anthropologists such as Claude Lévi-Strauss, Maurice Bloch, Jonathan Friedman, and Maurice Godelier, all of whom engaged in comparative research, though none of them did in-depth fieldwork in multiple sites, relying heavily instead on secondary ethnographic sources.

Without such an explicitly comparative agenda, some anthropologists have simply worked in different parts of the world, pursuing research on different topics (e.g., in the mid-twentieth century, Cora Du Bois in Indonesia, India, and the Netherlands; Fred Eggan in the Philippines and North America; and, more recently, Frederik Barth in China, Pakistan, New Guinea, and Indonesia, and Nancy Scheper-Hughes in Brazil, Ireland, and South Africa). More recently still, a number of scholars have moved from research in distant spaces to ethnography "at home" (e.g., Christine Ward Gailey, Rena Lederman, Emily Martin, Sherry Ortner, Paul Stoller, and Toby Volkman). Nowadays, in many textbooks that introduce anthropology to undergraduates, ethnography is defined as implicitly comparative, whereas ethnology is said to be explicitly comparative and usually focused on a single topic, such as socialization practices in different societies. That may be, but the deliberate and active shift to a new field research site, accompanied by a parallel shift in both theories and methods used by the anthropologist, is a qualitatively distinct undertaking.

For one thing, at this time—the beginning of the twenty-first century—

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end of his life, doing field research in, and publishing cultural analyses on, Germany, Austria, and Switzerland (1954).

the assumption that fieldsites correspond to spatially defined territories has been thoroughly deconstructed.[6] Even area studies paradigms, though they may remain prevalent in doctoral training in major universities, have changed. Within the discipline of anthropology itself, many anthropologists still speak of "going to the field" or "doing field research," not as a neocolonial enterprise (in the sense of "othering"), but rather as traversing some distance (whether physical or "just" conceptual) to get to somewhere else.

This predominant discourse is counterbalanced, though, by the practice of not exactly getting to somewhere else, but rather coming and going, and being present from afar. That is, once they do field research, anthropologists realize that they do not simply enter, leave, or return to it but rather engage it as part of their ongoing lives in a range of locations that constitute both a part of field research itself and a part of the lives of their interlocutors. Hence nowadays, "shifting fieldsites" entails not only the sort of dramatic shifts that a previous generation of anthropologists experienced, but also more mundane and subtle ones, such that shifts themselves are not wholly alien to the normative practice either of the discipline or of anthropologists' daily lives. Methodologically and theoretically, we have some skills available to us that we can draw upon and more consciously apply to these dramatic shifts. Eventually, the discourse itself may change.

One caveat is in order, however. A danger in the blithe embrace of either multisited field research or shifting fieldsites is a superficiality in the fieldwork itself. Depth is one of the hallmarks of anthropology. Deep interaction, deep cultural knowledge, and deep understanding do not come easily. They require sustained participant observation and a valorization of the social and institutional relationships that constitute each site or node.

PROFESSIONAL PRODUCTION

The fact of shifting fieldsites does not in itself explain the difficulties I have encountered establishing myself professionally. As Margaret Dorsey (2006:17) points out, in general, agencies that fund anthropological research, few though they are, remain more interested in funding research based outside of the United States. Interestingly, though, as she also notes, "the publishing world is moving faster than the funding world." Perhaps partaking of this

6. See, e.g., Amit (2000); Appadurai (1991); Bamford and Robbins (1997); Boellstorff (2008); Dresch, James, and Parkin (2000); Gupta and Ferguson (1997); Hannerz (1996).

trend, several presses have already expressed interest in publishing the product of my US-based research on the basis of a prospectus that they themselves requested.[7]

Coupled with the struggles of establishing a new professional identity is a nagging question I find myself asking: When I became a Latin Americanist anthropologist working in the Andes, did I take careful steps to build that identity? Or is this a memory narrative I have constructed *post hoc* that conveniently erases all the diversions I encountered along the way—stumbling, pleasant, and otherwise? I cannot answer the question. I think (perhaps erroneously) that, had I then wished to pursue my current field research, I would likely have selected a different place to do graduate work and built different networks to improve my chances of reaching my goal. There would have been moments of good and bad luck, but it would have been possible to control more of the variables that constitute the professionalization of scholarship. And, of course, time appeared to stretch before me, then.

IS ANYTHING NEW IN THE PRACTICE OF MY MÉTIER?

What makes the contemporary scenario I have sketched out here any different from the experiences of anthropologists in the past who moved from one fieldsite to another, and why? And what makes it similar? Most broadly, I conjecture that the growth of transnational interconnections creates more options for anthropologists who seek to do field research "at home." The "place" in which my research occurs has no clearly delimited boundaries. That would appear to make it easier to "do" field research, but it also means that it is harder for the anthropologist to draw boundaries between fieldwork and personal obligations. As a caveat, it may also lead to a propensity for greater abstraction and less attention to immersing oneself and then making vivid for readers or some other audience the nitty-grittiness of daily life that characterizes going "somewhere else." This is somewhat ironic, given that in the past, the tendency toward abstraction that sometimes accompanied the ability to engage in fieldwork *in situ* followed closely upon the recognition

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7. This represents a general trend, linked to an interest on the part of presses, not so much in ethnography conducted in the United States, as in ethnographic projects that treat topics that are accessible and meaningful to the general public. Robert Borofsky's Public Anthropology series for the University of California Press is a move in this direction, as are the numerous trade press publications and journalistic articles that rely heavily on anthropological knowledge but are not written by anthropologists.

that fieldsites, and the people who lived in them and served as anthropologists' informants, had so often been objectified and drained of life.

Technological advances now allow anthropologists to use a wider array of field research methodologies. Moreover, family structures and dynamics have changed in the lives of anthropologists themselves. In earlier generations, the "tradition" of shifting fieldsites was made somewhat easier by the acceptability of tag-along spouses (read: wives) and far less onerous expectations on the part of middle-class parent-anthropologists in the way of their children's formal education.

Clearly the intimate and the less personal are mutually constitutive, each making its mark on the structuring and substance of both domains. Women, in particular, after entering the workplace, have encountered challenges, such as accommodating familial responsibilities with the embrace of shifting fieldsites. At the same time, the changing milieux in which anthropology unfolds has obligated anthropologists to reflect on the value of their research to both the academy and civil society. This combination of conditions has served as a catalyst for many anthropologists to shift fieldsites. In my case, the research on adoption I have undertaken may, indirectly, have policy implications, and is of abiding interest not only to me but also to the people with whom I am conducting my research. It is thus a kind of "public anthropology" in ways that my earlier research in the Andes was not.

Some of the anthropologists I mentioned earlier contested the position of their government through their ethnographic research and publications. Margaret Mead saw herself as both anthropologist and public persona, sometimes eagerly supporting government initiatives (World War II), at other times passionately explaining the cultural underpinnings of American socialization practices and intergenerational strife. Nevertheless, many of these anthropologists made choices about the fieldsites where they would do research as a consequence of their association with government projects, which also provided them with a way to make an income. My research, in contrast, is not linked to objectives defined partially or wholly by government objectives.

The shift from Andean market women to transnational adoption in the United States crosses treacherous terrain. Geographically, my current work takes place on more level ground, but the territory is less familiar and there is no "other" to specify. It is impossible to disengage and "go do field research," as I did by embarking for the Andes. Perhaps as a result, the public face of my current project is far more exposed. Within the edifices of the academy, it is a steep climb, but one that is remarkably eye-opening.

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Time does not stand still. I presented the first iteration of this article as a talk at the American Anthropological Association meetings in 2007. As I worked on subsequent drafts, I realized that the movement from one field project to another illuminated for me not only the hurdles I needed to overcome, but also unexpected continuities between research in the Andes on market women, and research in the United States on the changing faces of families and adoption. These continuities are apparent in two recent publications, an article on the cultural and political economies of adoption in Latin America (Seligmann 2009a), and another on the life story of Lucre, a market woman in Peru, in the context of transference—a recognition that my interaction with Lucre entailed bonds between mother and child, the experiences of feeling orphaned, and longings to escape status ambivalence on both our parts (Seligmann 2009b). At the same time, and most importantly for me, the shift from one fieldsite to another has sparked a new intellectual curiosity; challenged my assumptions about matters I had worked on in Latin America, such as gender and kinship relationships in the context of globalization; and convinced me that as teachers—both in the classroom and in public settings—we should be making the dynamics of shifting fieldsites central to our conversations.

When Alma Gottlieb organized the conference session at which five of this book's chapters were first presented, I looked forward to tentatively setting out some of my ideas and hearing others' perspectives. After we presented our respective papers, I was not prepared for the sentiment that palpably rippled through the audience. To be sure, they responded to a multitude of points that we had raised. Nevertheless, I do not think it an overgeneralization to state there was a consensus among the audience that explicitly addressing this particular topic was long overdue.