Shadows in the Field
New Perspectives for Fieldwork in Ethnomusicology

SECOND EDITION

Gregory Barz and Timothy J. Cooley

Shadows in the Field

Second Edition

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*New Perspectives for Fieldwork
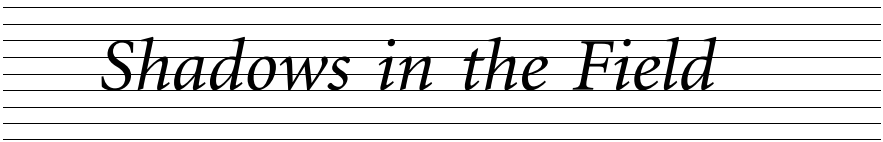in Ethnomusicology*

Second Edition

Edited by

Gregory Barz
& Timothy J. Cooley

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Foreword

Fieldworker's Progress

Shadows in the Field, in its first edition a varied collection of interesting, insightful essays about fieldwork, has now been significantly expanded and revised, becoming the first comprehensive book about fieldwork in ethnomusicology. Because ethnomusicologists think of fieldwork as the defining activity of their endeavor, one may be surprised to find, looking through our literature, not much that tells what it was really like to work in the "field," nor much about the methods employed in gathering data for any particular project in ethnomusicology. But one does get a sense that fieldwork meant—means—many different things to different scholars; many different things, indeed, in the career of any one scholar. As the history of ethnomusicology proceeded through the twentieth century, fieldwork changed radically, and many times, in its basic assumptions and execution; it has changed, as well, in my own several decades of attempts—and surely in the life of any of us who have been at it for several years.

In North America through the twentieth century (and, for that matter, in my own experience since 1950), the configuration, very, very roughly, went somewhat like this. Starting with simple "collecting"—we found an "informant" and asked him or her to sing for our recording devices, posing such questions as "What do you use this song for?" and "Where did you learn it?"—we proceeded to more general "hanging out" in a distant community, spending a summer, a year, attending events as they occurred and asking random questions. We began to engage in fieldwork by participating in the music we were studying—learning how to play and sing it—first often at our home institutions, then continuing in the culture's home ground, putting ourselves as pupils in the hands of competent teachers, joining local groups or classes. We moved on to the idea of projects to answer specific questions. For example, in my research, I tried to figure out how the minds

of improvisers of Persian music worked, by making and collecting many recordings of one *dastgah*, or "mode," and getting help from the musicians in analyzing how they had used the basic material of the *radif*.

We came to realize that we should do field research in our own communities, something that was both easier (it's our turf) and harder (be "objective" about one's own family and friends?) than working abroad. We began to question the role we were playing in the "field" communities, whether we were doing harm or good, and about our relationship to ethnomusicologists from those host communities. We worried that our very presence would result in significant culture change (and sometimes it did). It may have come as a bit of a surprise that the particular identity (nationality, ethnicity, gender) and the personality—shy, outgoing, quick on the uptake, contemplative—of the fieldworker makes a lot of difference in the research enterprise. We learned that fieldwork may include the gathering of ethnomusicological data from seemingly impersonal sources such as recordings and the Internet. And we have devoted quite a bit of energy to criticizing our discipline, largely in terms of the approaches and methods in the field. In its very comprehensiveness, this nutshell history of fieldwork hides dramatic events that become defining moments in one ethnomusicologist's progress.

Dramatic events for me: The Arapaho singer Bill Shakespear telling me in 1950 that two songs that sounded identical to me were different, and two that sounded very different were actually the same, "although very little difference in tone," teaching me that different cultures have very different conceptions of what makes a unit of musical thought. Calvin Boy, my Blackfoot teacher in 1966, telling me "the right Blackfoot way to do something is to sing the right song with it," putting the culture's conception of music into a single sentence. My teacher in Tehran telling me, perhaps with a bit of exasperation, that I'd never be a cultural insider and that any uneducated Persian would understand the music instinctively better than I ever could, with a little sermon in 1969 that began, "You know, Dr. Nettl, you will never understand this music." A Carnatic music lover in Chennai, to whom I was talking in 1981 about Mozart, exclaiming, "He is *your* Tyagaraja!"

Writing about Fieldwork

That's a précis of fieldwork—history and autobiography, in tandem. What, now, more specifically, about the history of the literature *about* fieldwork? Considering the centrality of fieldwork in the ethnomusicological enterprise, it's surprising that *Shadows in the Field* was really the first book devoted completely to this entire complex—and that there were few in the related disciplines of anthropology and folklore. (An early exception I'd draw to the reader's attention is Hortense Powdermaker's classic *Stranger and Friend* [1967], which lays out the similarities and differences of experience in four cultures in the author's lifetime career). And there were only very few chapter- or article-length extended discussions of fieldwork as

a whole. Or maybe it wasn't so strange, when we consider the small amount of attention given to the actual activities of fieldwork in the vast majority of the typical research studies, the ethnographic and musicological reports that make up the core of our recorded knowledge—most notably in those published before 1980. Many papers hardly tell us more than "this study is based on three months of fieldwork in . . ." If we compare these reports with those in the hard sciences, where everything—from number and grouping of subjects to precise times and detailed procedures of all activities—must be accounted for, we may wonder why ethnomusicologists, in describing their research, are so private about the fieldwork.

Here is one likely cause of this development: As most of the essays in this volume demonstrate, our informants-consultants-teachers become part of our family; or even more likely, we become part of theirs. I'm reminded of the joke about the structure of the Native American family—two parents, two children, one anthropologist. Talking about our fieldwork relationships would in some ways be like talking about family relationships. Our consultants and teachers do often treat us like wayward children (my elderly Persian music teacher scolding me: "Why do you go around Tehran talking to other musicians when you know I am the real authority?"); or like uncles or aunts (a Blackfoot dancer informing me, a bit condescendingly, "Things are very different from when you first came here"); or like siblings (we may help them with transportation or a bit of money; they often get us out of embarrassing social pickles). The fieldworker may relate to them as if they were parents, grandparents, lovers—the kinds of relationships that are difficult to write about, and especially to integrate into a scholarly, informative, and in some ways "objective" account. How we felt about them, emotionally, and perhaps how we think they saw us, may be virtually impossible to report on. As Helen Myers wrote, "In fieldwork we unveil the human face of ethnomusicology," and "Fieldwork is the most personal task required of the ethnomusicologist" (1992:21), suggesting that in contrast to the kinds of disciplines in which one may study manuscripts and texts, or statistically survey vast numbers of people through brief questionnaires, ethnomusicological data gathering is essentially a human exchange, and the quality of the human relationship between fieldworker and consultant, student and teacher, is at the heart of the endeavor.

But in contemplating the history of ethnomusicology from the perspective of fieldwork (rather than, say, analysis or interpretive theory), I am astonished at the large number of activities, as well as concepts, that fieldwork encompasses and thus should properly be included in its discussion, and at their interrelationships. The activity receiving the most attention in print has been the process of sound recording: selecting and learning to use (and maybe to repair) equipment and developing recording techniques, a profession by itself in modern musical life, but something ethnomusicologists had to absorb along with everything else. There are the associated problems of recording verbal information, making and organizing field notes (in the field, and later). But before all that should have come acquiring

linguistic and cultural competence; finding or selecting informants, consultants, and teachers, and dealing with the complex question of who is a proper spokesperson for the culture being studied; apprehending the culture- and community-specific methods needed for acquiring, as an ethnomusicologist, the three kinds of information that Malinowski (1935) specified for social anthropology—texts (maybe the songs and pieces); structures (the system of required behavior in musical activity, and the system of ideas underlying music); and the most intriguing, because it tells how these structures are actually observed in life, the "imponderabilia of everyday life" (who talks to whom, what kinds of things musicians actually talk about, what's the course of a lesson). Then come decisions: What does one do if one's consultants disagree? Is there unanimity? What is the distribution of beliefs? What are the subjects of local debate?—I'm just at the beginning of the list. Most important, the fieldworker needs to find a niche for himself or herself in the host society, where one is inevitably an outsider, but, if I can put it this way, an outsider of the insider sort.

There are so many things that are distinct about ethnomusicological fieldwork, one wonders why it hasn't received a lot more attention in the history of our literature. The question is particularly remarkable because this is a field which has, more than most, devoted a great deal of attention to its own methods and techniques, developing, indeed, a tradition of self-examination and critique. We would have expected some "how-to" books, textbooks for courses in field methods; works that theorize the problems of the interpersonal relations involved; books about the changing concept of "field"; and detailed accounts of individual experience. But most of our literature treats these matters at best as an essential step toward what we are trying to find out and not as a central activity. And yet, let me not neglect to mention some important surveys of fieldwork: Two massive chapters in Mantle Hood's *The Ethnomusicologist* (1982[1971]); two chapters in Helen Myers's edited compendium *Ethnomusicology: An Introduction* (1992); six short chapters in my own *Study of Ethnomusicology* (2005); and Herndon and McLeod's comprehensive and thoughtful *Field Manual for Ethnomusicology* (1983).

Shadows among the Landmarks

I have been complaining about the absence of literature about fieldwork in the last hundred years of ethnomusicological writing. But there has all along been a thin strand of such writing, and *Shadows in the Field,* while it is a unique contribution that fills an important niche, should also take its place among a number of important landmarks that go back to our earliest literature. A few words about the experience of collecting do appear in some of our earliest classics. Carl Stumpf (1886) gives us a fairly detailed (if sometimes curiously ethnocentric) account of his brief relationship with a member of the Bellakula, and his eliciting and transcribing sessions. Walter Fewkes (1890), writing about the earliest recording work, tells

something of what it was like. But it is somewhat baffling to read the many pioneering studies of George Herzog or the first book to attempt a comprehensive account of a small musical culture, by Alan Merriam (1967), and to find very little about the way this information was acquired. Later on, I must quickly add, Merriam produced two articles that qualify as classics in fieldwork literature—the unprecedentedly detailed account of the making of a drum among the Bala of Congo (1969) and the story of his revisit to the Basongye after fourteen years of absence, where it turned out that his earlier visit had come to be seen by his hosts as a defining moment in their music history (1977b).

And to be sure, beginning in the late 1970s and snowballing by the 1990s, authors of book-length ethnographies made the fieldwork process increasingly part of the discourse. Among the classics here are Paul Berliner's *The Soul of Mbira* (1993[1978]) and his descriptions of his interviews and lessons with prominent jazz artists in *Thinking in Jazz* (1994); and Steven Feld's work on the Kaluli in *Sound and Sentiment* (1990), with the intriguing attempt to have the result of his work critiqued by his teachers in a process he called "dialogic editing" (1987). Among the works I consider recent classics in their explanation and description of fieldwork, I wish to mention Anthony Seeger's *Why Suyá Sing* (1987b); Helen Myers's *Music of Hindu Trinidad* (1998); and Donna Buchanan's *Performing Democracy* (2005), which extends the subject to an urban society. These are outstanding examples, but there are now dozens of others, and they show that we have come a long way in understanding how much the process of fieldwork affects the final outcome and how important it is for the reader to get a sense of the relationships the author developed in the field. Everything that comes later—analysis, interpretation, theory—depends on what happened in the "field."

Aside from its primacy as a comprehensive book on fieldwork, *Shadows in the Field,* in its first edition, and even more, a decade later, in its second, concentrates on telling us how fieldwork affected the fieldworkers themselves. When first published, it was immediately seen as a book of great importance, and unsurprisingly it began quickly to be used as a text or required reading in seminars of students heading for the field. Many of its individual articles have been widely cited and it has become a mainstay of the central literature of the field. This second, expanded edition adds a new level of comprehensiveness. Preferring, in most instances, comprehensive works by individuals giving a personal synthesis in a unified perspective, I am persuaded in the work at hand that the diversity of fieldwork—the many kinds of attitudes and activities, the variety of host cultures and communities, and of relationships between fieldworker and teacher—could not be adequately represented by one author. We have here a plethora of presentations, most by well-established American, European, and Asian scholars with records of distinguished publications (among them, incidentally, six former or current presidents of the Society for Ethnomusicology), but also including, in the spirit of the first edition, voices of junior scholars. The authors have worked on all

continents and in villages and cities, telling us what it was like, what they tried to do, how they solved (or didn't solve) their central problems, how they related to their teachers, but also—and this strikes me as most significant—how the field experience changed them and their ideas, and how they as visitors changed their hosts.

Contents

Contributors

Carol M. Babiracki is associate professor of ethnomusicology in the Fine Arts Department of Syracuse University. Before joining Syracuse, she taught on the faculties of Brown and Harvard Universities. She holds a PhD in ethnomusicology from the University of Illinois at Urbana-Champaign. She has spent many years in India researching classical and folk music and dance, with a focus on folk and tribal music and dance in the state of Jharkhand over the past twenty-five years. Her research interests there include ethnicity, identity, gender, politics and cultural policy, oral epics, repertory studies, and flute performance. She is the recipient of Syracuse University's Meredith Teaching Recognition Award, and her publications have appeared in the journals *Ethnomusicology* and *Asian Music* and in the books *Women's Voices Across Musical Worlds, Shadows in the Field: New Perspectives on Field Research in Ethnomusicology*, *Comparative Musicology and the Anthropology of Music*, *Ethnomusicology and Modern Music History*, and *The Western Impact on World Music*.

Gregory F. Barz has engaged in field research in Uganda, Kenya, Tanzania, Rwanda, and South Africa for the past fifteen years. He received the PhD from Brown University and the MA from the University of Chicago. He is currently associate professor of ethnomusicology and anthropology at the Blair School of Music at Vanderbilt University. He is also the general editor of the *African Soundscapes* book series and served as African music editor for the *New Grove Dictionary of Music and Musicians* and as recording review editor for the journal *World of Music.* His latest book is titled *Singing for Life: HIV/AIDS and Music in Uganda* (2006). His book *Music in East Africa: Experiencing Music, Expressing Culture* was also published by Oxford University Press, and he has published an ethnography of East African choral communities, *Performing Religion: Negotiating Past and Present in Kwaya Music of Tanzania*. He is currently engaging collaborative medical

research regarding music and HIV/AIDS in Uganda, continuing his ongoing fieldwork as a Senior Fulbright Research Fellow in the African AIDS Research Program. His CD *Singing For Life: Songs of Hope, Healing, and HIV/AIDS in Uganda* was produced by Smithsonian FolkwaysHis topic for today's talk is "Singing for Life: Music, Medicine, and HIV/AIDS in East Africa" and draws on materials from his recently released CD with Smithsonian. He recently served as producer for the *God in Music City* CD produced in Nashville.

Nicole Beaudry teaches ethnomusicology at the Université du Québec à Montréal. She specializes in the musical traditions of North America's northern native cultures and is particularly interested in issues of performance and the relation among music, belief systems, and social behavior. She is currently working on the musical traditions of the Dene Indians in Canada's Northwest Territories.

Harris M. Berger is a scholar working at the intersection of ethnomusicology, folklore studies, popular music studies, and performance studies. An associate professor of music and associate head in the Department of Performance Studies at Texas A&M University, Berger and Annie J. Randall edit the Music/Culture book series, and he and Giovanna P. Del Negro edit the *Journal of American Folklore*. His books include *Metal, Rock, and Jazz: Perception and the Phenomenology of Musical Experience*, *Identity and Everyday Life: Essays in the Study of Folklore, Music, and Popular Culture* (co-authored by Giovanna P. Del Negro), and *Global Pop, Local Language* (co-edited by Michael T. Carroll). He has served as president of the US Branch of the International Association for the Study of Popular Music, and his current research projects include a monograph on affect, meaning, and style in expressive culture and an edited volume on heavy metal music around the world, co-edited by Jeremy Wallach and Paul D. Greene.

Philip V. Bohlman is the Mary Werkman Distinguished Service Professor of the Humanities and of Music at the University of Chicago. His fieldwork cuts across the boundaries of nations, genres, and musical practices, and currently he is engaged in fieldwork in European Muslim communities and in the virtual and ethnographic fields surrounding the Eurovision Song Contest. He is the author or editor of numerous books, most recently *The Music of European Nationalism* (2004) and *Jüdische Volksmusik—Eine mitteleuropäische Geistesgeschichte* (2005), as well as the Oxford University Press volumes *World Music: A Very Short Introduction* (2002), *Music in American Religious Experience* (edited with Edith Blumhofer and Maria Chow; 2006), and *Jewish Music and Modernity* (2008). He is artistic director of the cabaret, New Budapest Orpheum Society, with which he is recording its third CD. President of the Society for Ethnomusicology in 2005–2007, he received the Derek Allen Prize for Musicology from the British Academy in 2007.

CHOU CHIENER worked as a journalist and cultural administrator in Taiwan before undertaking graduate study in ethnomusicology at the Universities of Durham (1997–98) and Sheffield (PhD, 1998–2002). She has been a visiting lecturer at several universities in Britain and Taiwan. She has published widely in English and Chinese on the classical Chinese genre *nanguan* and its continuation and transformation in modern Taiwan, including articles in *Ethnomusicology*, *Ethnomusicology Forum*, and *Music in China* and a book entitled *Cong shisheng dao guojia de yinyue: Taiwan nanguan de chuantong yu bianqian* (2006). Her interests include ethnographic method, Taiwanese popular music, and English traditional dance. Currently, she is employed as postdoctoral research fellow at the University of Sheffield working on the study of daily musical life among the Bunun aboriginals of East Taiwan.

JUDAH M. COHEN is the Lou and Sybil Mervis Professor of Jewish Culture and an assistant professor of folklore and ethnomusicology at Indiana University. He has authored articles on American Jewish musical culture and history, as well as the book *Through the Sands of Time: a History of the Jewish Community of St. Thomas, US Virgin Islands* (2004).

TIMOTHY J. COOLEY has participated in fieldwork in Papua New Guinea, Central Europe with a focus on Poland, Midwestern America, and most recently coastal California and Hawaii. He is an associate professor of ethnomusicology at the University of California, Santa Barbara, in the Department of Music, and is affiliated faculty with the university's Global and International Studies Program. He teaches courses in Polish and in American vernacular, folk, and popular musics, among other things. Cooley earned the MM at Northwestern University and received the PhD in Ethnomusicology at Brown University. His book *Making Music in the Polish Tatras: Tourists, Ethnographers, and Mountain Musicians* won the 2006 Orbis Prize for Polish Studies, awarded by the American Association for the Advancement of Slavic Studies. He enjoys playing Polish mountain fiddle music, American old-time banjo, and guitar as well as singing in choirs. As this book is being published, Cooley is serving as the editor of *Ethnomusicology*, the journal of the Society for Ethnomusicology, and is the president of the Society for Ethnomusicology, Southern California Chapter. Whereas Cooley's work in Central Europe and Midwestern America focused on the musical articulation of ethnic groups, his recent research looks beyond ethnicity to "affinity groups." In particular he is asking how individuals associated with the surfing community in California musically express their ideas about the sport and related practices.

JAMES KIPPEN is professor of ethnomusicology at the University of Toronto. He specializes in the music of northern India, but also teaches courses on music in the Islamic world, music and colonialism, the Beatles, and various theory and method

courses including one on ethnomusicological fieldwork. His recent publications concern the social history of drumming in India, and include translations, transnotations, and analyses of nineteenth- and early twentieth-century indigenous writings on repertoire, rhythm, and meter. His latest book, *Gurudev's Drumming Legacy* (2006), examines one such work in the context of the Indian Nationalist movement.

Michelle Kisliuk is currently associate professor at the University of Virginia, in the Critical and Comparative Studies in Music program. Her doctoral training is in performance studies (NYU, 1991). Integrating theory and practice, she specializes in a performance approach to cultural studies, experiential field research, and ethnographic writing. Since 1986 she has researched the music, dance, daily life, and social politics among forest people (BaAka) from the Central African Republic, and has written about music/dance and modernity in Bangui (the capital city). In 2007 she began a research project focused on performance and identity in the House of Israel community in Western Ghana, and her earlier work has also extended to the socioesthetics of jam sessions at bluegrass festivals in the United States. Kisliuk directs the UVA African Music and Dance Ensemble and teaches graduate and undergraduate courses in ethnomusicology and performance theory. She has published numerous essays and articles, and her book *Seize the Dance! BaAka Musical Life and the Ethnography of Performance* (Oxford, 1998 and 2000) won the ASCSP Deems Taylor Special Recognition Award.

Katherine Meizel earned her PhD in ethnomusicology from the University of California, Santa Barbara, where her research focused on popular music. Her dissertation, supported by a University of Colorado fellowship in media, religion, and culture (Lilly Endowment), addresses identity politics in the televised singing competition *American Idol*. Her publications include articles in *Popular Music & Society*, the *Journal for the Scientific Study of Religion*, and the magazine *Slate*, and several chapters forthcoming in books on global music and media. Her paper " 'Be a Fan, Not a Hater': Identity Politics and the Audience in *American Idol*" won the Ki Mantle Hood prize at the 2005 Society for Ethnomusicology, Southern California Chapter, annual meeting. She also holds master's and doctoral degrees in vocal performance.

Bruno Nettl was born in Prague, received his PhD at Indiana University, and spent most of his career teaching at the University of Illinois, where he is now professor emeritus of music and anthropology. His main research interests are ethnomusicological theory and method, music of Native American cultures, and music of the Middle East, especially Iran. He has been concerned in recent years with the study of improvisatory musics, and with the intellectual history of ethnomusicology. Among his books, the most recent are *The Study of Ethnomusicology*

(1983), which after more than twenty years appeared in a revised edition in 2005; and *Encounters in Ethnomusicology* (2002), a professional memoir. He has served as president of the Society for Ethnomusicology and as editor of its journal, *Ethnomusicology*.

Timothy Rice, professor in the UCLA Department of Ethnomusicology, specializes in the traditional music of Bulgaria and Macedonia. He has written or co-edited *Cross-cultural Perspectives on Music* (1982); *May It Fill Your Soul: Experiencing Bulgarian Music* (1994); *The Garland Encyclopedia of World Music, Vol. 8: Europe* (2000), and *Music in Bulgaria* (2004). He edited the journal *Ethnomusicology* from 1981 to 1984 and served as president of the Society for Ethnomusicology from 2003 to 2005.

Anthony Seeger is an anthropologist, ethnomusicologist, audiovisual archivist, and record producer (BA Harvard in Social Relations, MA and PhD University of Chicago in Anthropology). He has combined university teaching and public ethnomusicology activities during his professional career as associate professor in the Department of Anthropology of the National Museum in Rio de Janeiro Brazil (1975–1982), associate professor of anthropology and director of the Indiana University Archives of Traditional Music (1982–1988), curator of the Folkways Collection and director of Smithsonian Folkways Recordings (1988–2000), and currently professor in the Department of Ethnomusicology at the University of California, Los Angeles (UCLA). He has served in professional societies (SEM, ICTM, IASA) in a variety of functions. He is the author of three books focusing on the Suya Indians of Brazil, with whom he and his wife have undertaken research and collaborations since 1971, and editor of two books on audiovisual archiving and has published more than sixty articles on a variety of topics in anthropology and ethnomusicology.

Kay Kaufman Shelemay is the G. Gordon Watts Professor of Music and Professor of African and African American Studies at Harvard University and a past president of the Society for Ethnomusicology. She is the author of many books and articles, including most recently the second revised edition of *Soundscapes: Exploring Music in a Changing World* (2006) and *Pain and Its Transformations:The Interface of Biology and Culture* (2007, co-edited with Sarah Coakley). She is currently carrying out ethnographic research with Ethiopian musicians who have immigrated to the United States and writing a book about Ethiopian cultural creativity in diaspora. She has recently received fellowships from the National Endowment for the Humanities, the Guggenheim Foundation, the Radcliffe Institute, and the John W. Kluge Center at the Library of Congress.

Jonathan P. J. Stock, editor of the journal *World of Music*, is professor in ethnomusicology at the University of Sheffield, where he directs the Centre for

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Nasir Syed is currently a PhD student at the University of California, Santa Barbara. He has been learning sitar for the past 10 years under Ustad Shujaat Khan. His research interests include Hindustani music, nationalism, and technologically mediated learning.

Jeff Todd Titon has served as professor of music and director of the PhD program in music at Brown University since 1986, where he teaches graduate seminars in fieldwork, in representing and interpreting people making music through hypertext and multimedia, in music and ethnographic film/video, and in the history of ethnomusicological thought, as well as undergraduate courses in blues, bluegrass, old-time, and country music. Brown's old-time Appalachian string band ensemble, which he founded and leads, recently celebrated its twentieth anniversary. Titon's fieldwork with a group of mountain Baptists in Appalachia's northern Blue Ridge spanned many years and resulted in the publication of an ethnographic book, documentary film, and sound recording, each titled *Powerhouse for God*. Since the 1990s, his fieldwork has involved Old Regular Baptists in eastern Kentucky, as well as old-time fiddlers and blues musicians, while his writing has turned more to theoretical issues involving fieldwork, phenomenology, applied ethnomusicology, and ethnomusicology's past and future. From 1990 through 1995 he was editor of *Ethnomusicology*, the journal of the Society for Ethnomusicology.

Deborah Wong teaches at the University of California, Riverside. A specialist in the musics of Asian America and Thailand, she holds an MA and PhD (1991) from the University of Michigan and a BA, magna cum laude (1982), in anthropology and music from the University of Pennsylvania. Her first book, *Sounding the Center: History and Aesthetics in Thai Buddhist Ritual* (2001), addressed musicians' rituals and their implications for the cultural politics of Thai court music and dance in late twentieth-century Bangkok. *Speak It Louder: Asian Americans Making Music* (2004) focused on music, race, and identity work in a series of case studies. She serves as president of the Society for Ethnomusicology for 2007–09 and pre-

viously served on the SEM Board of Directors from 1999 to 2005. She has taught at Pomona College and at the University of Pennsylvania, and as a visiting professor at Princeton University and the University of Chicago. She is a member of Satori Daiko, the performing group of the Taiko Center of Los Angeles, and her book in progress will address Japanese American drumming in California.

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Shadows in the Field

Second Edition

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TIMOTHY J. COOLEY
AND GREGORY BARZ

Casting Shadows: Fieldwork Is Dead! Long Live Fieldwork!

Introduction

Shadows in the Field: A Crisis for Fieldwork?

Music's ephemeral nature predisposes ethnomusicologists to embrace multiple realities. As Claude Lévi-Strauss suggests, "Music . . . bring[s] man face to face with potential objects of which only the shadows are actualized" (1969:17–18). Ethnomusicologists often feel as if they are chasing shadows in the field—striving to perceive and understand the liminal quality of musical meaning. The often ambiguous quality of musical meaning invites ethnomusicologists into a dialogue of multiple realities, a dialogue shared by social scientists endeavoring to understand other aspects of cultural practices. With a spirit of unboundedness, *Shadows in the Field* focuses on chasing shadows—on fieldwork—from a stance of ideological diversity to ask what it is that compels us toward fieldwork for methodological foundation. What is fieldwork, what does it accomplish, and how can we do it better? Long relegated to private conversations and hushed statements about what really happens in "the field," this book emerged out of a desire to provide a forum for making explicit contemporary theories involving fieldwork in ethnomusicology. Ethnomusicology enjoys the advantages of being an inherently interdisciplinary discipline, seemingly in a perpetual state of experimentation that gains strength from a diversity and plurality of approaches (Killick 2003; Rice 1987; 2003; A. Seeger 1987a:491–94; 1992:107; 1997). In this sense, ethnomusicologists are in a unique position to question established methods and goals of the social sciences, and to explore new perspectives. These new perspectives are not just for ethnomusicologists but also for all ethnographic disciplines.

Why do we focus on fieldwork when the liveliest debate among social sciences during the past several decades has been about the adequacy and legitimacy of our means for describing the cultural Other in writing? The reasons lie hidden in the sonic shadows of the musical practices we are privileged to study. The power of

music resides in its liminality, and this is best understood through engaging in the experimental method imperfectly called "fieldwork," a process that positions scholars as social actors within the very cultural phenomena they study. Ethnographic fieldwork requires meaningful face-to-face interaction with other individuals, and therein lie both the promise and challenge of our endeavors. In the first edition of *Shadows in the Field* we claimed that by actively taking part in a society's music-cultural practices, the ethnomusicologist had the potential for uniquely and truly participatory participation-observation. Jeff Titon phrased this succinctly as musically "being-in-the-world." We were perhaps overly naïve in our firm embrace of cultural relativism and ideological diversity, yet we coupled this stance with the adoption of feminist theories, phenomenology, and reflexive and dialogic ethnography, among other theoretical and methodological trends. Most boldly, we proposed that there was much to gain by shifting the emphasis away from *representation* (text) toward *experience*, a term that we believe encapsulates the essence of fieldwork. If this proposed focus on experience also results in better ethnographic monographs, all the better. In this second, revised edition, *Shadows in the Field* integrates new responses to recently revealed issues in ethnomusicology. The inclusion of these additional voices within the shadows of our discipline strengthens the original goals of this publication by maintaining an involvement with issues relevant to our discipline—both current and historical—as they relate to fieldwork.

Because of the potential for truly participatory participant-observation (Shelemay in this volume) through actively joining in a society's music cultural practices (including sounds, concepts, social interactions, materials—a society's total involvement with music [Slobin and Titon 1992:1]), we believe ethnomusicologists are well positioned to offer unique perspectives on postmodern fieldwork processes. By ethnography, we mean the observation of and the description (or representation) of cultural practices—in the case of ethnomusicologists the focus is on musical practices.[1] Fieldwork is the observational and experiential portion of the ethnographic process during which the ethnomusicologist engages living individuals as a means toward learning about a given music-cultural practice. Though the authors focus on the observational aspects of ethnography, they do not artificially ignore the textual imperatives of our academic field. For some, fieldwork is a process through which observation becomes inseparable from representation and interpretation (Babiracki; Barz; Kisliuk; Rice; Titon; and Wong in this volume).

Fieldwork distinguishes ethnographically based disciplines from other approaches to the humanities and social sciences. Ethnomusicologists derive from fieldwork their most significant contributions to scholarship in general. However, the critiques of the ethnographic enterprise that engendered the "crisis of representation" link ethnographic fieldwork, as well as representation, to colonial, imperial, and other repressive power structures (Asad 1973; Manganaro 1990:27–28; Sluka and Robben 2007:18; Willis 1972). While recognizing fieldwork as prob-

lematic and believing that fieldwork should be reconceptualized, even renamed (Kisliuk), the authors in this volume argue for fieldwork as an inherently valuable and extraordinarily human activity with the capacity of integrating scholar, scholarship, and life (Shelemay). Additionally, several authors suggest that the focus on performative aspects of culture, and our ability to engage music and individuals through substantive participation, increases both the value and necessity of ethnomusicological fieldwork for cultural understanding (Kippen; Kisliuk; Rice; Shelemay; Titon). Chapters new in this second edition of *Shadows in the Field* expand our responses to the postcolonial critique of ethnography. They include fresh looks at domestic fieldwork as opposed to the classic "exotic" fieldwork model (Stock and Chou; Wong; Cohen; Berger), issues in "virtual fieldwork" (Cooley, Meizel, and Syed), a critique of "ustād" model fieldwork (Kippen), studies of popular and vernacular musics (Berger; Cooley; Meizel), the challenges of a "native" fieldworker among transnational diasporic groups (Wong), and the need for well-conceived cultural advocacy (Seeger).

Why This Book Now? Five Centuries of Ethnomusicological Fieldwork

In the mid-twentieth century, fieldwork (re-)emerged as a common practice among ethnomusicologists, and fieldwork methodologies have since multiplied to an extent that a comprehensive history of ethnomusicological fieldwork in this century would alone fill a book. The late nineteenth century is often interpreted as the beginning of ethnomusicology, but this period was preceded by a long history of ethnographic inquiry into music—a history in which fieldwork was not unimportant.[2] This extended history contributes to our legacy as ethnomusicologists, and few of us have the opportunity to undertake fieldwork in any region of this small world where this legacy does not precede us. Conscious attempts by some ethnomusicologists to distinguish themselves from present and past colonial administrators, missionaries, tourists, and other ethnographers only serve to highlight our connection, for better or worse, with this legacy. As Kisliuk writes in this volume, we may be required by the people we study to enter into a role cultivated during a colonial period even if we actively work to define for ourselves a different field stance. As individual fieldworkers, our shadows join with others, past and present, in a web of histories: personal histories, the histories of our academic field, and the histories of those we study, for example. Interpreted within a broad framework of intellectual and political history, a brief (and in no means comprehensive) history of ethnomusicological fieldwork allows us to understand better the present condition of fieldwork and suggests why the issues addressed in this book are vital now.

A fieldwork model of collecting data for goals quite external to the field experience itself is strikingly common in the history of ethnographic inquiry. This

model of fieldwork is consistent with the science paradigm of the "modern era," which persisted into the twentieth century.[3] In this model, music was an objectively observable fact to be collected in the field and manipulated in the laboratory. However, a few early examples of ethnomusicological literature stand outside this science paradigm, and we begin this selected sampling of ethnomusicological literature with one such example.

Jean de Léry was perhaps the first European to describe the music of a non-European society (1578).[4] A Calvinist minister, de Léry traveled to an island in the Bay of Rio de Janeiro in 1557 to assist in the organization of a French settlement. After quarreling with the leader of the settlement, de Léry was in a real sense stranded on mainland Brazil for about ten months until he returned to France in 1558 (Harrison 1973:6). While in Brazil, De Léry took an interest in the indigenous people around him and sought opportunities to observe and document their cultural practices, including rituals involving music. In his descriptions of what he heard and saw, it is clear that he was convinced that their music and rituals were linked with pagan religious beliefs, but he does not allow this to interfere with the fascination he felt toward what he observed:

> But those ceremonies having lasted thus almost two hours, these five or six hundred savage men not ceasing at any time to dance and sing, there was a tune of such a kind that, given that they do not know what the art of music is, those who have not heard them would never believe that they could sing so well together. And in fact, whereas at the beginning of this sabbath (being as I have said in the women's house) I had been in some fear, I had now as recompense such a joy that, not only hearing the consonant sounds so well rhythmicised by such a multitude, and above all in the cadence and refrain of the dance-song, at each verse everyone drawing out their voices, giving forth in this way, I was altogether captivated; but also every time that I remember it with beating heart, it seems to me that I still have them in my ears. (De Léry, in Harrison 1973:22)

The clarity of de Léry's biases, his first-person prose, his stated fear-then-joy at the experience of unfamiliar music, and his expressed passion for the music he heard resemble recent reflexive ethnography. These qualities also distinguish de Léry's writing from later ethnographies in the science paradigm that, ostensibly in the service of scientific objectivity, do not admit passion. De Léry's Calvinist beliefs allowed him to be skeptical of the emerging scientific paradigm. He sought religious truth, not scientific objectivity, and though in his mind the native Brazilians were mistaken, de Léry seemed sensitive to their efforts to express belief systems in ritual forms.

In contrast, European writers of de Léry's era and for several centuries following typically replaced such enthusiasm for non-Western music with a pronounced bias for European music. For example, nearly two centuries later French Jesuit missionary and pioneer ethnographer among Canadian Indians, Joseph-François Lafitau, expressed surprise at de Léry's passion for Native American

music: "I have not felt at all such keen pleasure as Mr. de Léry did at our Indians' festivals. It is difficult for me to believe that everyone was as much impressed as he at those of the Brazilians. The music and dancing of the Americans have a very barbarous quality which is, at first, revolting and of which one can scarcely form an idea without witnessing them" (1974–1977:326 [1724:534]).

A century after de Léry's Brazilian encounter, Athanasius Kircher published a theory of music including a systematic comparative study of musics from around the world. He gathered together available information about musics, Western and otherwise, including musics in the Americas, to ponder the cosmological origins of music structures in his 1650 *Musurgia universalis, sive ars magna consoni et dissoni* (Bohlman 1991:144–146). This treatise can be considered the beginning of a European academic discipline of music scholarship that includes non-European musics. He maintained a conservative neo-Platonic theory of music as a numerical symbol of God's cosmic harmony, but he was forward-looking in his extension of the discussion to include music far removed from European practice (Buelow 1980:73–74). Yet Kircher did no fieldwork himself and based his comparative studies on the fieldwork of others—a model repeated by some comparative musicologists in the end of the nineteenth century.

Systematic early ethnomusicological praxis centered upon the scholar's personal fieldwork is represented in several musical ethnographies from the eighteenth century, notably those of Jean Joseph Marie Amiot and of Sir William Jones. A French Jesuit missionary, Amiot moved to Peking in 1751 and remained there until his death in 1793. His 1779 book *Mémoire sur la musique des chinois* (1779) is based on many years of firsthand observation of music practice and on older Chinese music treatises. Similarly, Jones's 1792 article "On the Musical Modes of the Hindus" draws from his experience in Calcutta where he was a colonial High Court judge for many years. Like Amiot, Jones benefited from ancient treatises and from the observation of current music practice, as well as from consultations with Hindu music experts (see Jones 1792:62 for a fair statement of his fieldwork methodology). Amiot and Jones had a great deal of respect for the music systems they described, and Amiot is exceptional for his efforts to convey Chinese music in a Chinese manner (Lieberman 1980:326). The methodologies of Amiot and Jones are not unlike those of present-day ethnomusicologists who study music systems that have ancient indigenous theoretical literatures—so-called classical traditions.

Like other colonialists who wrote about the music of colonized peoples, Amiot and Jones focused on description and explanation, not on understanding (Rice; Titon in this book). The asymmetrical relationships of fieldwork in colonial contexts make it unlikely that a fieldworker would understand or even be interested in, for example, the inner life of an Indian or Chinese musician. Asymmetrical relationships may have excluded the possibility of Amiot or Jones submitting themselves as apprentices to master musicians, a common learning technique today among ethnomusicologists studying classical traditions of Asia and the Far

East. We do not wish to question the quality and integrity of the pioneering work of these early scholars, only to historically and socially situate their work and to suggest how their shadows impact our own fieldwork. Several authors in this book, in fact, find that they must strive to define for themselves new roles as fieldworkers in the lingering shadows of colonialism (Babiracki; Kisliuk; Stock and Chou).

Technological advances in the following century contributed to the institutionalization of cross-cultural music studies using a methodology mirroring science: fieldwork and laboratory work. The establishment of "comparative musicology" as an academic field in the 1880s was facilitated by the invention of the gramophone in 1877 and the creation of a pitch and interval measurement system by A. J. Ellis (Ellis 1885; see also Krader 1980:275–77; Stock 2007). Mechanical audio recordings and measuring devices allowed for greatly improved objectivity in analysis of music objects, and could separate the scholar from the inherent subjectivity of fieldwork involving unpredictable human encounters. Reflecting the emphasis on sound objects, early leaders in the field of comparative musicology concentrated their work in newly established archives of sound recordings—the laboratory—and often did little or no fieldwork themselves—so-called armchair analysis (Merriam 1964:38–39; and see Marcus and Fischer 1986:17–18; Sluka and Robben 2007:10–13, for descriptions of contemporary phenomena in other ethnographic fields). Carl Stumpf, with the assistance of his student Erich M. von Hornbostel and medical doctor Otto Abraham, founded the Berlin Phonogramm-Archiv around 1901 (Christensen 1991:204; see also Schneider 1976), the first large archive of non-Western music field recordings, but Stumpf himself did not travel out of the West to make field recordings. Hornbostel, who was the director of the Phonogramm-Archiv from 1906 to 1933, did do fieldwork among the Pawnee Indians in North America, and in prison camps during World War I. Yet the conception of fieldwork as the collection of data to be analyzed in the laboratory and used in universal schemes—such as tracing the evolutionary origins of music or mapping global culture areas—is persistent throughout the work of comparative musicologists up to and including Alan Lomax's cantometrics project (1968; 1976). In the heyday of comparative musicology, the general attitude toward fieldwork was expressed by Jaap Kunst, who described fieldwork as a desirable and even exciting activity but held that true scientific work occurs in the laboratory (1959[1950]:19).

Fieldwork was better integrated into different early ethnomusicological models active in America and Europe at the same time that the Berlin school of comparative musicology emerged in the late 1800s. Motivated by a fear that native cultures were vanishing, the Bureau of American Ethnology sponsored massive fieldwork projects around the turn of the century that included the collection of American Indian musical sounds on wax cylinders and the documentation of musical practices in their cultural settings. J. Walter Fewkes, Frances Densmore, and German immigrant Franz Boas, scholars who worked for the Bureau of

American Ethnology at some point in their careers, all recorded songs, as well as other "cultural artifacts." Their emphasis on fieldwork and data collecting contrasts their approach with the Berlin comparativists, but at least in the case of Boas, they were no less theoretical. Boas was influenced by comparative linguistics and the Berlin comparativists, and by the evolutionary theories that underpinned much of their work, but he ultimately rejected evolutionary hierarchies as inherently racist. Through a rigorously empirical fieldwork-based methodology, he moved toward theories of cultural relativism (Stocking 1974:478–80; de Vale 1980:823).

Musical folklore—an additional early ethnomusicological model as practiced by Zoltán Kodály, Béla Bartók and Constantin Brailoiu in Eastern Europe, and Maud Karpeles and Cecil Sharp on the British Isles—shares with comparative musicology a science paradigm that conceives of music as a collectable, comparable, and ultimately explainable object within an observable cosmos. Contrasting with comparative musicology, musical folklore focuses on the folk music of the scholars' native country rather than on universal comparative schemes. Musical folklorists—like folksong collectors before them such as the person who coined the term *Volkslied*, Johann Gottfried Herder, in eighteenth-century Germany (Suppan 1976) and Oskar Kolberg in nineteenth-century Poland (1961)—were motivated by the concern that their national folk heritage was vanishing. Fieldwork was associated with romantic nationalism and a quest for the natural and the pure. Even musical folklorists, such as Bartók, who did significant fieldwork outside their native country tended to relate music from other countries (or other ethnic groups within their home country) to the folk music of their native country (e.g., Bartók 1976:146).

Nationalism motivated British and continental musical folklorists alike. Sharp endeavored to glean from folk music national (racial) traits of the English (Anglo) people (1932:xxiv–xxxvi; 1954:1). Bartók hoped to "scientifically demonstrate which [tunes] are pure Hungarian folk song types, and which are borrowed melodies or reflect foreign influence" (1976:157). The perception of "the common people" or "peasants" as cultural and national ancestors also linked the British and continental musical folklorists (Sharp 1954:xx, 16ff; Bartók 1976:71). Fieldwork within one's own country and among individuals who share the fieldworker's nationality might seem to exonerate the scholar from the critique of the ethnography that seeks to describe the Other. Yet musical folklorists invented an Other within their national borders by creating cultural and evolutionary development distinctions that separated the scholars from the individuals studied (for a musical folklorist's theory of evolution, see Sharp 1954:16–31). Situated historically, it is evident that musical folklorists are implicated in the oppressive policies of colonialism and imperialism. The British colonial empire was at its peak when Sharp was collecting folk songs and using them to promote an English Anglo racial identity (Harker 1985; Francmanis 2002). Perhaps he perceived a need to distinguish English colonialists from the subjects of the British Empire. Similarly, before the First

World War when Bartók was active searching for pure Hungarian folk music, Hungarians had authority over significant portions of the Austro-Hungarian Empire. In some cases this authority was used to suppress those not considered ethnic Hungarians, including Jews, Gypsies, and Slavs. Certainly Bartók's work was not free of troubling racial politics (Brown 2000; Trumpener 2000), though his own writings reveal a progressive move toward more pluralist views of society (Bartók 1976:29–31).

Two scholarly societies were founded shortly after the Second World War: the International Folk Music Council (IFMC) in 1947 (the International Council for Traditional Music [ICTM] after 1982) and the Society for Ethnomusicology (SEM) in 1955. The IFMC began as an organization of primarily European musical folk-lorists dominated by the English, and has grown over the decades into a truly international society that encompasses diverse approaches to the study of music. In a sense, the founding of the SEM replaced (though not entirely), especially in North America, a fairly unified academic discipline—comparative musicology— with a diverse academic field—ethnomusicology—that borrowed from compar- ative musicology as well as from historical musicology, anthropology, and folklore. By the end of the 1950s in North America two ideological trends within ethno- musicology emerged: anthropological, as represented by Alan Merriam, and mu- sicological, as represented by Mantle Hood (Nettl 1964:21–25; Myers 1993:7). Folklore studies were and continue to be a strong influence, as can be seen in the writings of Charles Seeger in the 1950s (i.e., 1953 in 1977:330–334), as well as in the more recent work of Jeff Titon (1988:xv), Philip Bohlman (1988b), Revell Carr (1999; 2004; 2007), and others. The field of performance studies is also influenced by folklorists (see Béhague 1984; Abrahams 1970; Bauman 1975) and shares many of the issues that motivate this volume, especially concerning the liminality of per- formance practices (Schechner 2003:57). Music departments and anthropology departments, however, presented the most viable academic homes in institutions for ethnomusicologists—a situation that remains today.

Both the anthropological and musicological approaches are significant from the perspective of fieldwork—fieldwork was an essential methodology to both, but fieldwork of different sorts reflecting different goals. Merriam's desire to "un- derstand music in the context of human behavior" (1964:42) or "music in culture" (1960:109) called for fieldwork that closely resembled anthropological methods, including extensive fieldwork by ethnomusicologists themselves. He believed ethnomusicology must be defined, not by *what* we study, but by *how* we study music. Like earlier science paradigm models, his methodological model still combined two separate components, field and laboratory work, but he envisioned a fusion of the two (1964:37–38), and he objected to comparative musicology's fixation on the music object (taxonomies, scales, melodic and pitch phenomena, etc.). Mantle Hood advocated a very different fieldwork method that reflected his training as a musician. Without denying the importance of studying music in its

cultural context, he shifted the focus back on the music sound object with his call for *bi-musicality*, a corollary to the anthropologist's bilingual challenge (1960; 1982:25–40). In the 1970s and 1980s the anthropological and musicological methodologies and theories were merged in the work of some ethnomusicologists (e.g., Berliner 1993[1978]; Feld 1990; A. Seeger 1987b) and continue to be fused today, as evidenced in the chapters of this book.

Mid-century Ethnomusicology (Post–World War, Early Cold War, Pre-Vietnam, Not-Yet-Ready-for-Postcolonial Ethnomusicology)

The American academy and society at large entered a period of change in the 1970s to which in many ways the original publication of *Shadows in the Field* was a response, and it is this period that we now are beginning to reassess. What we might call the "ethnographic" disciplines—including ethnomusicology, anthropology, to a certain extent folklore and sociology, and more recently performance studies (Schechner 2003)—were entering a postcolonial era ushered in by rather sweeping challenges to Western hegemonic thought made possible, in part, by the decolonization of European empires. A parallel phenomenon occurred with related challenges to totalizing scientific paradigms (Rosaldo 1993[1989]). In other words, Western economic, political, and intellectual hegemony were questioned after World War II. The perceived crisis of the era was as much about coming to terms with the colonial legacy inherent with many of our academic disciplines as about the interpretations and representations of cultural practices. In the ethnographic disciplines, this so-called crisis called for profound self-critical reflection in the face of a colonial legacy that led anthropologists and ethnomusicologists to fear that their interpretations were driven by paradigmatic theories that were on some level in support of cultural imperialism. There is ample evidence to suggest that often this was indeed the case (Willis 1972).

One response to this crisis by anthropologists in particular was to look to ethnography and a new-and-improved "fieldwork" as a solution. One might even wonder if fieldwork provided an escape for many in the face of the crisis. If so, in their escapist forays they encountered a different world out there in their longing for holistic theories of culture, and by extension a new world "at home" within the academy with requisite desires for absolutes. What they discovered in the crisis-of-representation quest was a postmodern—and later globalized—reality where distinctions between cultures, scholars, informants, subjects, objects, selves, and others were increasingly blurred (Clifford 1988; 1997). It was at the peak of this crisis that we originally raised the premise that drew the responses bound in the first edition of *Shadows in the Field*. On reflection, as social actors birthed in the early- to mid-1990s, we were perhaps merely picking up on anthropologists' escape to fieldwork, effectively (re-)presenting fieldwork as "an inherently valuable and

extra-ordinarily human activity with the capacity of integrating scholar, scholarship, and life" (Cooley 1997:5).

We still stand by this basic premise, with rather audacious fingers pointed directly at the essential epistemological assumption of 1950s fieldwork: The crisis is not representation, however; the crisis is experience, for it is only in experience that we know. But, pointing at a problem does not solve it. During the decade following the original publication of *Shadows in the Field*, fieldwork as the defining method of ethnomusicology once again must be reassessed. The motivations for this reassessment are both practical and ideological. Practically, funds for long-term overseas fieldwork appear to be diminishing, and international travel is becoming less attractive due to a series of wars and direct anti-American sentiment. Ideologically, the paradigm of "area studies" that encouraged location-specific research is directly challenged by issue-driven projects that focus on musical change, transnational and intranational musical fusions, polymorphic rather than circumscribed theories of identity, and ubiquitous commentaries on globalization.

Always leading the charge—as was true with the original *Shadows in the Field*—graduate students are good indicators of disciplinary change. In informal surveys of graduate programs in ethnomusicology there is currently an almost even split between students adopting traditional fieldwork-based projects involving extended periods away from home in the field and students designing alternative research methodologies, usually with some gesture toward fieldwork but of a wholly different sort. Many of these students—and interestingly in both categories—frequently challenge their predecessors' assumptions and expectations, indicating a distinct shift away from mid-century fieldwork methodologies. Those field research projects that retain what we uncomfortably call "traditional exotic fieldwork" tend to ask untraditional questions, often challenging reified "cultures" and considering instead the invention of cultural discourses. The assumption of non-European subjects is more often now unassumed, according to data collected on what students are doing. Interestingly (although not surprisingly), the academic job market has not caught up with this sea change, as recent job postings corroborate. Students now turn frequently to ethnographic studies of popular music, diasporic groups, and subaltern practices that challenge the tools for analysis that were so practical only a few years ago.

General assumptions about empiricism were frequently challenged in the anthropologically based crisis of representation. In recent ethnomusicological thought we have moved through various epistemological staging points or "-isms" prefaced with "post"—postmodernism, post-structuralism, and post-colonialism (which although overtly politically correct seems to have the most staying power, even though we are at the moment in a post–politically correct era). We remain in an epistemologically eclectic moment, and in this regard we are not in a much different place than a decade ago. We still have much to learn from feminist

theories, phenomenology, and reflexive and dialogic ethnography, for example. Reflexivity, defined most eloquently by Myerhoff and Ruby in 1982, was still a "trend" ten years ago, and is now an expectation. Phenomenological approaches to the study of music, advocated so convincingly by Titon and Rice in the first edition of *Shadows in the Field*, are appearing in an increasing array of publications (see, for example, Benson 2003, Berger 1999, Friedson 1996, and Porcello 1998). The concept of historical studies informed by fieldwork presented by Noll and Bohlman in the first edition is employed in new monographs (i.e., Cooley 2005:58–122; Carr 2006).

Ethnographic fieldwork is also experiencing fundamental structural changes. The classic model of the mid-twentieth century was a minimum of twelve months "away" in some remote locale—the more "exotic" the better. In the first decade of the twenty-first century we find a much broader spectrum of fieldwork situations. For example, today many ethnomusicologists stay at home and study their own community or travel within their own home country to research other communities in our increasingly multicultural society. These fieldwork paradigm shifts were foreshadowed by Beaudry's, Titon's, and Shelemay's chapters in the first edition of *Shadows*, and they are augmented here. Specifically urban-focused domestic fieldwork was pioneered in ethnomusicology by Adelaida Reyes (2007), and is developed here by Wong and Cohen. The original edition of *Shadows* also challenged the very notion of insider/outsider, subject/object research, and here we take this farther by including new chapters that address various degrees of "indigenous" fieldwork (Chou and Stock; Wong; Cohen; and Nasir). Finally, as we ethnomusicologists become more confident in our disciplinary methods and with our ever-changing role as fieldworkers, we are moving from a concern about the potential negative impact on those we study and toward active advocacy for those same individuals and their communities (Seeger this volume; Hellier-Tinoco 2003). In light of the harsh, if ill-researched, critique of anthropological fieldwork by Tierney (2000), we feel it is imperative that these changes and new developments be addressed in the chapters that follow.

Perhaps the pressing questions is, is fieldwork in fact dead? Fieldwork as culture shock leading to a rite of passage is clearly no longer part of mainstream ethnomusicology. Yes, the old fieldwork with all of its assumptions and expectations is dead. Yet the epistemological efficacy of experience has lost none of its luster. The face-to-face interaction with other individuals and some level of participation in the music-cultural practices we hope to understand no less lends itself to meaningful musical "being-in-the-world" today than it did ten years ago. The crisis is still experience. What we need to do as ethnomusicologists—and as music scholars in general—is to toss out older assumptions about fieldwork in order to adjust our expectations. Yes, the *old* fieldwork is certainly dead. Long live the *new* fieldwork!

The New Fieldwork

What are the characteristics of the new fieldwork? Face-to-face interaction remains a cornerstone of fieldwork. The "ethno" in ethnomusicology has always referred to people—any person, any group of people, however they are constituted. Ultimately, if not only and always, we judge our disciplinary success by our ability to "write-across-culture" (ethnography). Music is our path toward people, and if anything distinguishes contemporary ethnomusicology from previous eras of the discipline, it is our practice of talking with, playing music with, experiencing life with the people about whose musical practices we write. The returning and new authors of the second edition of *Shadows in the Field* reiterate the book's initial central theme: fieldwork is experience, and the experience of people making music is at the core of ethnomusicological method and theory.[5]

The normative twelve-month fieldwork model has exploded in several ways. Even with projects that reflect traditional exotic fieldwork, the parameters have changed, often becoming temporally ambiguous. Not only do we extend our time "in the field" digitally, but we tend to go to the field more often; first for "feasibility" trips, later for follow-up trips. Travel to Papua New Guinea and Uganda has become easier over time. Other field research projects lend themselves to frequent short trips, in this way not unlike Boas's pioneering fieldwork of a century ago, but in contrast to Levi-Strauss's famous voyages and Malinowski's years during the First World War in the south Pacific.

What student in the twenty-first century does not begin a research project with an Internet search? When does digital information, accessed with a computer portal, become fieldwork? In his book *Global Pop: World Musics, World Markets* (1997:xvii), Timothy Taylor asked if use of the Internet might constitute a new form of ethnography. René Lysloff compared his traditional fieldwork in Java with what he also called fieldwork conducted on-line with a musical community that exists on the Internet. He never met the community members face-to-face (2003:234–235). Kiri Miller's study of the *Grand Theft Auto* video game series included some of the trappings of traditional fieldwork (surveys, interviews), but as with Lysloff's study, there was no community that gathers in a physical place (Miller 2007). Lysloff and Miller did engage in participation/observation with their research communities, but it was virtual participation/observation. While this clearly represents a radical redefinition of ethnography, today's fieldwork must take into account the benefits of what living in the digital world can afford us all. Indeed, our face-to-face fieldwork in the Polish Tatra Mountains (Cooley 2005) and in remote communities in Uganda and Rwanda (Barz 2006) is today extended with email conversations and queries, including the digital transfer of visual and audio files. Is this fieldwork? The Internet and email are not mentioned in the first edition of *Shadows in the Field*, but in this second edition several chapters rely and draw on Internet ethnography as part of their overall research method. The chapter co-

authored by Cooley, Meizel, and Syed, for example, addresses and expands what we might normally consider virtual fieldwork beyond just the digital.

A new ethnomusicological truism notes that what is local is global, and the inverse, one must understand the local implications of those things that seem to be global. Cooley's research, for example, proposes a "California Vernacular" music associated with surfing and what "indigenous" literature calls the surfing tribe (i.e., Malloy 2006:3). This regional music was first Pacific Rim music: Californians imitating Hawaiian music, and Hawaiian musicians in California. The inscribed popular genre named "surf music" in 1961 still serves as a sonic icon for California, but it was first exported across the United States and then globally. Today there are classic-style surf bands in Finland, Slovenia, and Japan, as well as in America's midwest. To understand California Vernacular beach music, one must think globally. Barz's recent research on musical decisions made by popular musicians in post-genocide Rwanda cracks open a culture of pain and suffering. How can musical choices that contributed significantly to the loss of countless lives be repackaged and reperformed to heal the nation on both local and global levels. Choices about adapting local scale systems and local languages frequently involve decisions to reach out to broader audiences with broader expectations of contemporary African popular musicians at home and abroad. Juggling the local and the global is a political, emotional, *and* musical aspect of fieldwork in contemporary Rwanda.

The new fieldwork is assertively global in its subjects. No musical genre, tradition, or related activity is off limits for contemporary ethnomusicologists. This was clear from surveys taken of graduate programs in which student projects are shifting toward popular music genres, on the one hand, and domestic musical practices, on the other. Henry Kingsbury's ethnography of the New England Conservatory of Music is an important early example of fieldwork of Western Art Music (1988); Kay Shelemay's ethnography of the early music movement is a more recent example (2001).

Shadows in the Field—Contributions

The essays in *Shadows in the Field* are reflections on the state of fieldwork in ethnomusicological thought. Several authors evaluate their own fieldwork and challenge readers to reconsider what it is they do when they do fieldwork, or "field research," a phrase preferred by Barz, Babiracki, and Kisliuk. Greg Barz wrote his contribution to this volume while "in the field" of Tanzania, East Africa. By looking self-critically at his practice of taking field notes, he realized that the activity of writing about experience was actually affecting his experience. The field journal, therefore, not only stands between experience and interpretation but is also interrelated with experience and interpretation. Michelle Kisliuk considers the relationship between experience and writing. In her essay based on field research

among BaAka pygmies in the Central African Republic, Kisliuk calls for writing that fully evokes experience. She notes that research focusing on the ethnography of musical performance stands to bring other ethnographic disciplines closer to more effective ways of writing about and understanding research and cultural processes. Nicole Beaudry believes fieldwork is first of all an extraordinarily human research methodology—after all, it is humans that fieldwork brings together. In a candid and personal essay based on her fieldwork among Inuit, Yupik, and Dene communities in Canada, Beaudry describes how the humanity of the fieldwork enterprise caused her to question classic field techniques (participation-observation, interviewing and translation) and to develop her own non-model approach. Deborah Wong's essay encourages ethnomusicologists to consider ways we can engage the practice of ethnography more critically. She draws on her rich experiences as a researcher and performer of North American *taiko*. By offering a creative methodology that draws directly on her performative epistemology, Wong encourages a positioning of autoethnography in the toolbox of ethnomusicologists.

A group of authors highlight the diversity of roles, identities, and self-reflexive experiences in ethnomusicological fieldwork. Jeff Todd Titon proposes an epistemology for ethnomusicology in which fieldwork is defined as "knowing people making music," an experiential, dialogic, participatory way of knowing and being in the world. This musical way of being in the world and knowing differs from models of ethnomusicology that emphasize the contemplation in the laboratory (library, sound archive, study) of a text collected in the field. Timothy Rice adapts the phenomenological hermeneutics of philosopher Paul Ricoeur to mediate between field experience and field method. In the process, Rice challenges categories of "insider" and "outsider," "emic" and "etic," and the metaphorical notion of the "field." He bases his views on transformative moments in his experience of Bulgarian music culture, and his long transformation into a *gaidar* (bagpipe player). Carol Babiracki critically considers both reflexive theories and feminist theories of ethnography as she seeks to develop research methodologies and writing strategies that bridge the chasm between the field experience and writing about the field experience. In her essay, she investigates the impact of her identities and gender roles on her own research in village India, which began in 1981 and was renewed in 1993. The influence of these different gender identities and roles is manifest both when doing fieldwork in India and when writing in an American academic setting. In his new essay, Harris Berger approaches the significant issue of fieldwork and rock by offering a perspective on what ethnomusicological ethnography can offer popular music studies. By focusing on ethnography (old and new forms), he demonstrates a dialogic approach to his field research with "extreme metal" in Ohio rock communities. By offering a critical phenomenological approach, he concludes that practice and experience are the most significant study objects of ethnomusicology.

Several authors comprise quite different challenges to the notion of synchronic fieldwork—the "ethnographic present." Philip V. Bohlman used fieldwork methodologies during the summers of 1990 and 1991 to reconstruct the musical landscape of Jewish musical life in the Austrian border province of Burgenland, where Jewish religious life flourished from the late seventeenth century until 1939 when Jews were expelled or deported as a first stage in the Holocaust. Based on these experiences, Bohlman suggests that the past not only lends itself to the fieldwork process, but that certain historical conditions require the fieldworker's approaches. Relying on the memory of present-day residents of Burgenland and on the imagined constructions of oral history, he was able to enter the "ethnomusicological past," which is not "the past as it really was," but rather the past that is recognizable only through the filters of memory and through the tragic disjuncture of the Holocaust. Bohlman uses fieldwork to understand the ethnomusicological past and Kay Kaufman Shelemay examines the role of ethnomusicologists in the transmission of the music they study to the ethnomusicological future. The ethnomusicologist, while seeking to document the transmission process of musical practices, becomes a part of those practices. An event during Shelemay's research with Jews of Syrian descent living in Brooklyn, New York, pushed her toward recognizing the ways in which ethnomusicologists are implicated in the process of transmission. In a fascinating addition to the second edition, Judah Cohen explores issues of long-term field relationships established by his teacher and mentor, Kay Shelemay. The discovery and maintenance of links with specific communities leads Cohen to inhabit inherited fieldwork spaces that bridge time and place and to question the authority and role of the academy in local community outreach efforts.

Several essays new to the second edition introduce significant perspectives on emergent issues in ethnomusicology. Anthony Seeger's essay problematizes the distinction some might make between "applied" and "theoretical" forms of ethnomusicology. He suggests that the two can actually serve to strengthen each other. Drawing on his long-standing work with the Suyá of Brazil, Seeger concludes that the communities we live with and study (and represent) should be considered when we record, document, and release archival and commercial recordings. Engaging public projects, he suggests, is not only a legitimate form of ethnomusicology, but the accompanying field research documentation can very well serve important functions for both the communities engaged and for the discipline of ethnomusicology. Jonathan Stock and Chou Chiener raise issues related to ethnomusicologists studying at "home" and "abroad." They suggest that the significance of home-based ethnography will surely increase with the rise of ethnomusicology in academic institutions throughout the world in the last two generations. Both authors tease out the intricate nuances of "home" by providing case studies that ultimately call into question dialectical ways (emic/etic, insider/outsider) of differentiating field roles. For Stock and Chou, engaging fieldwork at

home represents opportunities to experience a more rounded ethnomusicology (even if it means documenting the introduction of mad-cow disease into our disciplinary discourse). Fieldwork at home bridges, explains, and domesticates as it enhances the traditional academic roles of the researcher.

James Kippen introduces the so-called *ustād* model of engaging field research to the second edition. He writes about his journey to India to study in the Lucknow tablā tradition with Ustād Afaq Husain Khan. The role of the ethnomusicologist in field relationships (disciple, student, son, etc.) is complicated, according to Kippen, who suggests that conflicting loyalties may affect how we engage fieldwork and write ethnography. In a jointly authored essay on virtual fieldwork, Timothy Cooley, Katherine Meizel, and Nasir Syed explore the potentiality of virtual fieldwork by raising critical issues regarding the technological divide that affects our research. For these three authors (two of them graduate students at the time of writing), newer technologies that offer new modes of communication beg questions of dependence and responsibility on the part of the fieldworker. Three case studies introduce us to detailed dimensions of the *American Idol* reality show, vernacular musics related to surfing culture in California, and North Indian Hindustani music in order to tease out ways in which virtual fieldwork manipulates, influences, and facilitates experiences of ethnographic contact.

The essays of the second edition stand independent of each other, but are woven together with the thread of issues and concepts. For example, if the authors represented in this book believe fieldwork is an important and central feature of present-day ethnomusicology, they do not all concur about what constitutes fieldwork and "the field." When Carol Babiracki first traveled to India, she carried with her a common conception of fieldwork as clearly bounded by time, space, culture, and language. Babiracki wonders if she can avoid the disjuncture between the field experience and the rest of life, including life hours spent writing about experience. Titon raises a similar issue related to his domestic fieldwork and writing. He tries to achieve integration by circling "hermeneutically back and forth between texts and experience, musical knowing and musical being." In a complementary process, Shelemay experiences an integration of life and scholarship through fieldwork. Judah Cohen simultaneous feels a stranger and at home in an inherited fieldwork community. Timothy Rice challenges the boundary between the field and home, suggesting that the field is a metaphorical creation of the researcher. Michelle Kisliuk questions whether "fieldwork" is a construction to distance us from "real life," creating an artificial boundary between here and there, home and field, us and them. She prefers the phrase "field research" and defines the "field" as a "broad conceptual zone united by a chain of inquiry." We have already written about Philip V. Bohlman's extensions of fieldwork into areas of inquiry normally left to historians. For Jeff Titon, fieldwork need not involve travel to a distant place—"fieldwork" can be playing music with other individuals and the "field" that shared experience.

For Deborah Wong, the field is both everywhere and nowhere, and anyone and everyone is an insider. She envisions a world for the ethnographic encounter in which social change is not only embraced but also expected. Fieldwork still excites Anthony Seeger. Not knowing what is going to happen next in field research leads Seeger to recognize how easy it is for ethnomusicologists to lead and direct the products of their research to meet the needs of others (NGOs, government agencies, etc.). Engaging lived experience with the "on-the-ground world of people" is the greatest product of fieldwork for Harris Berger. This emphasis on experience allows for a strong phenomenological approach for ethnography in which field colleagues move beyond their historic roles as cultural actors playing out a script to being fully engaged cultural participants actively engaged in their experiences. Jonathan Stock and Chou Chiener explode open the processes engaged by native fieldworkers. By focusing on the act of engaging in fieldwork at home, Stock and Chou each describe ways in which domestic fieldwork is more effective and linguistically economical. They raise important issues, not the least of which is the political potential for aggressively engaging fieldwork projects that easily respond to activistic interventions "at home." James Kippen teases out issues related to the politics involved in the *ustād* fieldwork methodology. He suggests that in all field relationships conflicts in loyalty easily occur that mold our experiences with informants. In addition, he points to the irony of fieldwork situations where the fieldworker must embrace established ethics protocols while the informant may choose to ignore the same code. Virtual fieldwork, according to Timothy Cooley, Katherine Meizel, and Nasir Syed, is permeating contemporary ethnomusicological fieldwork methodologies, and has been for longer than we realize. Each of these three authors clearly articulates the need to embrace virtual worlds as means to an end, namely, the goal of studying real people making real music. Each uses virtual methods as a component of his or her fieldwork methodologies, not as the dominant modality. Cooley, Meizel, and Syed challenge us all to adapt to and re-imagine our field situations, ultimately embracing technological advances in the virtual world to meet the changes in ways in which people are musically in the world.

Reflexive ethnomusicology is a pervasive theme in *Shadows in the Field*. No longer are ethnomusicologists content to record music in the field—to collect data for later analysis in the laboratory. The shift in interest away from music as an object toward music as culture and then as cultural practice has renewed emphasis on "reflexive, nonobjectivist scholarship." Reflexive ethnography responds to two related aspects of our ethnomusicological heritage. First, it works to redress the insufficiencies of colonial ethnography that positions the ethnographer outside the study community in an Archimedian vantage point from which to view and represent the Other, resulting in what Gourlay called "the missing ethnomusicologist" (1978:3). Second, reflexive ethnography rejects the modern-era science paradigm that conceives of human culture as wholly objectively observable

(see also Myerhoff and Ruby 1982:15; Clifford 1986b:22). Instead, ethnographers attempt reflexively to understand their positions in the cultures being studied and to represent these positions in ethnographies, including their epistemological stances, their relations to the cultural practices and individuals studied, and their relationships to their own cultural practices. Reflexive ethnography is keenly aware of experience and of the personal context of experience. Though one objective of ethnography is to understand others, reflexive fieldworkers realize that "we get to know other people by making *ourselves* known to *them*, and through them to know ourselves again, in a continuous cycle," as Kisliuk describes the process. Timothy Rice constructs an epistemology mediated with experience that positions all understanding within the realization of self-understanding. The challenge is to avoid self-indulgent and "confessional" ethnography, and to focus on the ethnographically relevant (for critiques of reflexive ethnography, see Babiracki, Barz, Kisliuk, and Wong in this volume). Locating the reflexive moment in shared music performance is one method pioneered by ethnomusicologists for achieving understanding, but focused on meaningful activity (see Titon, Berger, and Rice in this volume).

As ethnomusicologists, we have much to offer other ethnographic disciplines when it comes to the ethnography of experience. As Kisliuk writes in her essay, "Because of our participation in performance, ethnomusicologists are especially aware that there is much one can only know by doing." Shelemay describes ethnomusicologists' tendency to participate in music culture as "extraordinarily participatory participant-observation." The methodological advantages of participating in the music practice one is studying were first advocated by Mantle Hood (1960), positioning ethnomusicologists as leaders in the ethnography of performance, or performance practice. However, sharing music with the people one is studying is a practice that preceded Hood. Indeed, de Léry sang for the native Brazilian's in 1557, a reciprocal act that seems to have been greatly appreciated (Harrison 1973:6). More recently, David McAllester wrote eloquently about the advantages of sharing songs with the Navahos from whom he was learning songs (1973[1954]:84–85), and Paul Berliner found his skills at playing an African mbira gave him entrée to a music culture of Zimbabwe (1993[1978]). Music participation is not simply a means for gaining access to cultural information. Anthony Seeger discovered that it was part of his duty to reciprocate with songs when working among the Amazonian Suyá—he was called on to share music from his own culture, not simply record and document Suyá music (this volume and 1987b:19–23). Seeger, like de Léry, practiced participant-observation, not only by "doing what the Other does" but also by opening a window for the Other on the musical world outside the field. Apprenticing oneself to a master musician is a well-established ethnomusicological fieldwork technique, especially among scholars of Asian art musics. The experience of knowing people through "musical being" is central to Titon's epistemology of musical knowledge. Taking feminist episte-

mology as a working model, Titon grounds musical knowledge in "musical being," rather than in introspective self-reflection. However, performance participation is not a panacea: it does not produce perfect musical ethnographies. In her work in village India, Carol Babiracki also participated in music and dance, but believes her performance resulted in skewed interpretations of music culture, and performance certainly affected her gender roles and identities. Using their own fieldwork experience for negative as well as positive examples, the authors of this book demonstrate what ethnomusicologists in particular can contribute to a better understanding of fieldwork practices for ethnographic disciplines in general.

Chasing Shadows—Conclusion

What do we see when we acknowledge the shadows we cast in the field? What do we hear, smell, and taste? In their forward and backward glances, each author in this volume explores the distinct roles he or she acts out while performing and reconstructing field research. Will field research continue as an integral part of ethnomusicological theory and method? Or will distinct theories emerge that will drive future ethnomusicological discourse? If so, where does field research of the past fit into these theories? What are the new directions that ethnomusicological field research is taking? What good can come of it all? Such questions are unique to the new fieldwork, and have not yet been addressed in a significant way in any of the standard guides to ethnomusicological or anthropological field research theory and method. As they interact with contemporary and historical field research models, ethnomusicologists define individual ways of interacting, often improvising and inventing new methods and theories in the field.

A degree of separation between experience and representation has been a traditional feature of ethnographic field research in the social sciences. This feature persists in many contemporary studies despite the recent "crisis of representation." The two quotations that follow demonstrate a historical and contemporary reassertion of the bifurcation of field research from ethnography—of experience from representation no longer embraced in the new fieldwork. The first selection is from the field guide *Notes and Queries on Anthropology*, first published in 1874:

> [The investigator] must assume the attitude of a learner, not of a teacher . . . . The music of every people, whether vocal or instrumental, has its own characteristics, and can be estimated rightly only on the evidence supplied by accurate records. General impressions—even those of a trained European musician—are of little value unless the sounds and phrases which they describe can be reproduced. Music may be recorded either in writing, or by means of the phonograph or other recording instrument. (Frake 1964[1874]:33, 315)

This dictum indicates that one of the main objectives of the field researcher of musical performance is to document by means of "accurate records" before any

observation can be of "value." The second quotation, similar in spirit to the first, appears in the field manual compiled by the Society for Ethnomusicology, in which the notion of documentation or writing as separable from ethnography or representation is reaffirmed:

> Respect your informants' beliefs and traditions. You may object to attitudes or behaviors on a personal level, but in your role as researcher, do not pass judgment . . . . Ethnomusicologists are part of a process whereby musical traditions all over the world are recorded, documented, studied, written about, and made accessible to new audiences . . . . Effective documentation can make data valuable not only for the researcher returning from the field with a large body of information to organize, analyze, and interpret, but also for future generations of scholars. (Post et al. 1994:54, 5–6)

The Society for Ethnomusicology manual (recently updated [Fargion 2001]) reiterates the prominent position of documentation in ethnomusicological field research; the manual asserts that field research relationships—certainly a form of human relationships—are perhaps not authentic interactions in and of themselves.

The authors in *Shadows in the Field* address issues of documentation, but not as a field guide or manual for the next generation of field researchers; the essays offer few prescriptive recommendations concerning what to do while engaged in field research. Specifically avoided are questions one should ask in the field, specific methods to employ, definitions of what music is and what it is not, and identification of what musical facts are and are not. Rather, the authors engage in a forum, openly questioning the position of field researcher in the new fieldwork.

All the authors of *Shadows in the Field* approach their fieldwork experiences as a way to confront questions concerning who they are as field researchers and what they signify to the people with whom they work (and to themselves). In addition, each author attempts to reach beyond traditional academic discourse to redefine the way we read, ultimately suggesting changes in representation that involve an acceptance of multiple truths, multiple epistemologies that would lead to the "creative diversification" of ethnographic writing that James Fernandez has called for (1993:180). Ethnography in this sense becomes an integral part of the translation of experience, an extension of the field performance, and ultimately a form of performative writing. For most field researchers the period of "translation" is frustrating, where nothing, including the self, is as it seems, and many are now beginning to realize that field research itself is just such a period of "translation."

Shadows in the Field does not present a complete story; the authors intentionally leave doors open, ideas unfinished, and research still in process. It is an important time for these essays to emerge as researchers and informants, scholars and friends, stand close enough to each other so that the outlines of their shadows merge, blurring the definition of self and other in field research. The negotiation of individual and communal experience, the processes of forming relationships, the

representation of musical ethnography—contemporary ethnomusicology is challenged in many unique ways: most important, to listen, feel, question, understand, and represent in ways true to one's own experiences.

Shadows in the Field was never conceptualized as a how-to manual for fieldworkers, but is motivated by issues in fieldwork that are essential for the continued contributions of ethnomusicology and related fields. Rather than an intermediary step toward an ethnographic monograph in which culture is represented, fieldwork is potentially an inherently valuable model for being in the world. Doing fieldwork, we weave ourselves (or are woven by others) into the communities we study, becoming cultural actors in the very dramas of society we endeavor to understand, and vice versa. At this "experimental moment" in the field of ethnomusicology, we have unique opportunities to reconsider the theories, methods, and epistemologies of fieldwork. Toward this end, the essays in this volume offer diverse approaches for chasing the shadows of music cultures. We hope they will ignite in readers a passion for knowing and a desire for understanding that can be achieved only through the experience of human interaction.

As a closing gesture, we suggest that to open up the topic of musical study in this way calls for yet another layer of decolonization, the decolonization of our departments and schools of music, and of the entire university structure. This could very well be the direction in which this moment of shifting methodologies is leading us. This is where the not-so-new (anymore) musicology was heading as the twentieth century came to a close. Gary Tomlinson proclaimed at the 1999 American Musicological Society conference that music has deliberately been used to construct a sense of self, and a sense of the Other since the eighteenth century. Surely there is no conceiving of Europe without considering that non-European Other. Tomlinson concluded that there can be no musicology without ethnomusicology, and we would quickly add, no ethnomusicology without musicology (anthropology, sociology, performance studies, and so forth). Perhaps the final cross-cultural passage will be across the disciplinary divides within universities, and fieldwork could very well show us the way by interpreting liberally the call of fieldwork to experience our fellow humans through any and all means possible, including archival research, Shenkerian analysis, singing in a choir, and learning to play sitar.

We stand now at a critical moment in the field of ethnomusicology. The new fieldwork has become a reality for many field researchers, for both experienced and younger scholars, as we continue to listen and learn from those we engage in our field research. This second edition of *Shadows in the Field* refocuses our efforts to highlight and explore significant issues related to the new fieldwork. Each of the ethnomusicologists in this volume issues a call for the integration of field research experience with the representation or communication of that experience. Each author acknowledges and approaches an understanding of the agency of the individual field researcher both in and out of the field, chasing whatever is hidden

behind the shadows he or she casts in the field. Ideally, ethnomusicologists will be encouraged to take these ideas, thoughts, and methodologies into classrooms, seminars, and conversations with colleagues at home and in field research situations, and explore them in ethnographic writing.

If globalization theories have taught us anything, it is that we already are social actors within just about any cultural phenomena that we might wish to study. Another way to look at this is that we are all already in the field. We are all fieldworkers.

Notes

1. See for different but not incompatible definitions of ethnography Marcus and Fischer (1986:18) and Feld (1990:x).

2. As a field obsessed with self-definition, ethnomusicology has numerous histories, all generally interpreting the 1880s and comparative musicology as the beginning of the field. For example, see Merriam 1960; 1964:3–16; 1977a; Nettl 1964:12–24; 1986; Krader 1980; Myers 1993:3–15; and Shelemay in this volume. In this introduction, we join Bohlman (1988a,b,c; 1991), Harrison (1973), and Shiloah (1995:ix) to suggest a longer history for ethnomusicology.

3. By the "modern era" we refer to the time initiated by the scientific revolution in the sixteenth century and extending to the disillusionment with the hegemony of Western scientific thought in the twentieth century. This use of the root "modern" differs from twentieth-century literary and aesthetic use of the words "modern*ism*," "modern*ity*," and "modern*ist*;" terms about which there is little agreement (Manganaro 1990:4–7). In other words, we distinguish between "the modern era" and "modernism" (unlike Miller 1994:60–64). However, our interpretation of "postmodern" as that which reflects the changes in how experience is perceived (primarily by Westerners) after the modern era is not incompatible with other attempts to characterize "postmodernism."

4. Jeff Titon first brought to our attention the unusual qualities of de Léry's writing about the experience of music.

5. This should not, however, eliminate the possibility of historic ethnomusicology. Philip Bohlman's and William Noll's chapters in the first edition of *Shadows in the Field* stand as examples of effective uses of ethnographic fieldwork when researching essentially historical questions. These projects index history's effect in contemporary cultural practices necessitating the need for ethnography.

Knowing Fieldwork

Epistemology is that field of inquiry whose subject is the origins, nature, and limits of human knowing (see Rorty 1979:140). An epistemology for ethnomusicology is therefore concerned with the origins, nature, and limits of human knowledge concerning music in human life. An epistemology for ethnomusicology attempts to answer two basic questions: What can we know about music, and how can we know it?[1]

Not long ago, musical transcription was the distinguishing mark of our discipline, not only as a passage rite (Hood 1982[1971]; McAllester 1989) but as a generative practice. Transcription told us what we could know about music and how we could know it. Music was objectified, collected, and recorded in order to be transcribed; and transcription enabled analysis and comparison. Transcription— that is, listening to a piece of music and writing it down in Western notation—not only became a guild skill but also "wrote across" lived experience, eliminated the life-world, and transformed what was left (sound) into a representation that could be analyzed systematically and then compared with other transcriptions so as to generate and test hypotheses concerning music's origin and evolution. Today it is not transcription but fieldwork that constitutes ethnomusicology. Fieldwork is no longer viewed principally as observing and collecting (although it surely involves that) but as experiencing and understanding music (see Titon 1992[1984]:xvi). The new fieldwork leads us to ask what it is like for a person (ourselves included) to make and to know music as lived experience.

As it did most, if not all, ethnomusicologists, music caught hold of me before ethnomusicology did. In the late 1960s when I began formal study of ethnomusicology, at the University of Minnesota, I was already part of a blues musical community centered on Lazy Bill Lucas, an African American who was born in Arkansas and had a career as a blues singer in St. Louis and Chicago before moving to the Twin Cities in the early 1960s. Harmonica player Mojo Buford, who had

been with Muddy Waters's band, bass player JoJo Williams, guitarist Sonny Boy Rogers, and pianist and singer Leonard "Baby Doo" Caston also visited Bill's apartment, the hub of this community; and we played music together, ate Bill's fried chicken dinners (he was a superb cook), drank Fox Deluxe beer, and became friends. I got to know them, their wives, and their girlfriends, and we passed time together. Later, in Alan Kagan's seminar in ethnomusicology, I learned about fieldwork. Then, fieldwork relied on in-person observation and on data gathering through structured interviews, a method derived from the Trobriand Island practice of anthropologist Bronislaw Malinowski during World War I.

Thinking about my blues musician friends, I wondered whether to do field-work with them. Why not? I thought, and I proceeded to interview Bill for a class project. Of course, I had already "observed" him for a long time (and vice versa). I had no difficulty speaking with them about their lives and careers, particularly because they felt that it might result in useful publicity—and it did. The publi-cation of Bill's interview, for example, led a French blues enthusiast to produce two LP recordings of Bill's music (Titon 1969; Lucas 1971, 1972). In those interviews I asked questions such as when and where they were born, what kind of work their families did, when they first learned music, how their musical careers progressed, and so forth; and they answered them. I was doing oral history and was interested in obtaining facts of their lives. In a word, I was data gathering. As a result, my relationship with them added a dimension: I became someone who might be able to promote them, to help them in their careers, instead of just a young man hanging around older ones and trying to learn music from them. Besides friend-ship I now had a tacit contract with them.

I had discovered that my fieldwork thrust me into thinking about relation-ships; it wasn't just about surveying and collecting. Later, I also realized that structured interviews did not always result in my best understanding. Blues singer-guitarist Son House had come to the Twin Cities to do a concert, and I was able to get an hour alone with him and a tape recorder. I had my oral history questions ready, but I had decided to begin by playing him a tape of a blues recording from the 1920s by his friend Charley Patton, hoping to enlist his help in deciphering Patton's lyrics. (House later told me that you could sit at Patton's feet and not understand a word he was singing.) House listened to the tape, and I was ready to start asking questions, but before I could do so, he began to speak and reminisce about "Papa Charley" and those days. I forgot about my questions and listened to what he wanted to say. He told me a long and detailed story about how he "got religion" when he lived in the Mississippi Delta. He also spoke about the old times, and the bad whiskey they made and drank, and he acted out a story about how he got put in jail one night because he was so drunk he wouldn't let a Greyhound bus pass him while he was driving home. He told me how his white landlord had interceded with the sheriff and the judge to free him, but added a fine of indebt-edness to his sharecropping arrangement. In telling the story he played the parts of

the boss and sheriff. Boss (House whispers): "You got to let him out of there; he's so good with the tractor. I need him Monday morning." Judge (House whispers): "Well, all right, we'll tell him he had to pay such and such a fine." House (normal voice): "See, that's how they stepped in with each other" (Titon 1976).

I sat there raptly listening, wanting more. When House stopped telling stories from his life, I steered him through a series of oral history questions, hoping to get more stories; but now I was directing it by the questions I asked, and House no longer felt free to move in his own direction. And so began a long process in which I pondered the different kinds of knowing that arose from the structured interviews that were a part of the old fieldwork, versus those life stories told to sympathetic listeners or friends in a "real life" situation that could not, then, be described as fieldwork, but whose resultant texts I maintained ought to be valued, not as a form of data gathering, but as a means toward understanding (Titon 1980).

Continental European philosophy since the nineteenth century regularly distinguishes between two kinds of knowledge: explanation and understanding (Dallmayr and McCarthy 1977). Explanation is typical in the sciences, and understanding typifies knowledge in the humanities. We are all familiar with the scientific method of inference, hypothesis, and experiment; scientific explanations in their strongest form are expressed as universal laws of nature, such as the law of gravitation. Explanation gives us the kind of knowledge that enables prediction and control (Carnap 1966). Understanding, on the other hand, represents a different kind of knowledge. If explanation is directed toward objects, understanding is directed toward people. If explanation drives toward law, understanding drives toward agreement, sometimes, though not always, through lived experience (Gadamer 1992(1975); Schutz 1962). Explanation proceeds through analysis, understanding through interpretation. Explanation is a type of "knowledge-that," whereas understanding is a type of "knowledge-of." "Knowledge-that" is a typical concern of British and American positivist philosophers in this century, because in their view all meaningful knowledge-propositions can be expressed in prepositional form as "I know that. . . . " (Of course, not all "knowledge-that" propositions are meaningful in a positivist sense.) Understanding's "knowledge-of," on the other hand, is more characteristic of an earlier view: knowledge of subjects, expressed in statements like "I know my friend William" or "He knows plumbing" or "You know ethnomusicology" (Rorty 1979:141).

Most writings about ethnomusicology as an academic discipline favor explanation theories of knowledge in which music is considered a type of language (see, e.g., Nettl 1964; Hood 1982[1971]; Kunst 1959; Myers 1992). Ethnomusicology is said to have begun in the 1880s when it became a scientific project. At the time it was not called ethnomusicology but comparative musicology, reflecting its close kinship with similar disciplines such as philology (comparative linguistics). The person generally regarded as its founder, Alexander Ellis, set out to measure the musical intervals in selected non-Western musics. Most Europeans thought that

these musics were more or less "out of tune." Ellis, representing the best tradition of ethnomusicological relativism, had another hypothesis: that the modes and scales of other nations had their own patterns, different from those of Western Europe, but coherent in their own terms. Measuring the intervals confirmed his hypothesis. Significantly, Ellis was tone-deaf and employed an assistant to make the measurements. That is, Ellis could not experience the musical intervals and had to rely on an external instrument to do so.

The most obvious application of explanation theories of knowledge to ethnomusicology came via linguistically based theories of music. Comparative musicology and musical folklore both rely on philology (comparative linguistics) for their methods. In this century linguistics has changed, but whether in the systematic musicology of Charles Seeger, the transformational ethnomusicology of John Blacking, the cognitive ethnomusicology of many of our European colleagues, or the semiotic ethnomusicology of Jean-Jacques Nattiez (1990), the notion that music behaves and ought to be studied as a system like language continues to have a profound and shaping influence on our discipline, one that has affected my work as well as others'. Ethnomusicology, as a paradigm, owes a great deal to anthropology—after all, the Society for Ethnomusicology was originally planned at meetings of the American Anthropological Association—and anthropological linguistics is one of the four fields of traditional American anthropology. (Archaeology, physical anthropology, and cultural anthropology are the other three.)

Theories of knowledge based on understanding rather than explanation, on the other hand, find their philosophical defenders in a continental philosophical tradition that begins with Dilthey and includes Husserl, Sartre, Heidegger, Schutz, Merleau-Ponty, Gadamer, and Ricoeur. This tradition, an alternative both to Anglo-American positivism and to European structuralism, involves mainly two kinds of activities: phenomenology and hermeneutics. Phenomenology emphasizes the immediate, concrete, sensory lifeworld, and it attempts to ground knowledge in the world of lived experience (see Ihde 1986[1977]). Hermeneutics originated as a way of interpreting the Bible but has come to be a method for interpreting texts in general. In recent years, Paul Ricoeur has attempted to integrate the two into what he calls hermeneutic phenomenology. For Ricoeur, any meaningful action can be considered, or read, as a text; thus, a musical performance, for example, can be understood as the equivalent of a text (1981b). Clifford Geertz took up this formulation, likening cultures to "an assemblage of texts," and his work has been enormously influential in American ethnomusicology in the past fifteen years or so. Although much of my work from the 1980s is based in hermeneutic phenomenology, I have more recently become critical of the poststructuralist tendency to textualize everything, musical experience included; and I have proposed that we stand Ricoeur on his head, that meaningful actions be experienced as music, not read as text (Titon 1995). In other words, I suggest that we change the metaphor we

use for our interpretive acts. The world is not like a text to be read but like a musical performance to be experienced. But I must leave that for a future essay.

Ethnomusicology, in my view, has made use of four paradigms, or bedrock sets of assumptions, during the current century, of which comparative musicology was the first. The English translation of the second is musical folklore. It is typical in Eastern Europe, and was until recently in Britain. Although musical folklore involves collecting, transcribing, analyzing, and comparing, it adds four other features: an ideology of nationalism, an ethnographic emphasis on surveying social context, an ethical dimension that involves the preservation of music thought to be traditional and endangered throughout the world, and an educational aspect in which the music becomes part of the public school curriculum and is offered to adults as well. The collecting, classifying, and analytical works of Béla Bartók and of Constantin Brailoiu are representative of musical folklore.

The third paradigm is ethnomusicology itself, associated with the birth of the Society for Ethnomusicology in the 1950s, which grafted in American anthropology, with its emphasis on fieldwork and cultural immersion, rather than survey work; in addition, ethnomusicologists tend to distrust broad comparative generalizations and produce, instead, monographs based on detailed studies of particular music-cultures. Ethnomusicologists also distrust nationalism, rejecting it as ethnocentric, and they do not, by and large, emphasize preservation; rather, their focus is on acculturation and change. Nor do ethnomusicologists find much enthusiasm for public school music education; they think of themselves as scholars. (The late Alan Merriam used to dismiss the efforts of his world music colleagues in music education as "sandbox ethnomusicology.") The "native point of view" is important to ethnomusicologists, many of whom adopt in one form or another Merriam's three-part feedback model of music in culture: ideas, behavior, and sound (1964). For Merriam, and most of the ethnomusicologists of his generation, ethnomusicology nevertheless was about data, while the personal experiences of the ethnomusicologist, including all the relations with others in the field that not merely affected but constituted the meaningfulness of the data, were absent; ethnomusicology was to be, in his memorable phrase, "sciencing about music" (Merriam 1964:25).

The seeds of the fourth phase, for which we do not yet have a single name, were sown by those ethnomusicologists who brought master artists to American universities, where they led non-Western ensembles in which some graduate students found their most profound musical experiences. I have called this new paradigm the study of people making music (Titon 1989, 1992[1984]),[2] but it might also be called the study of people experiencing music. In retrospect it is apparent, also, that this fourth paradigm came from a generation transformed by the politics of the 1960s: the women's movement, the peace movement, and the Civil Rights movement. Because it is still emergent, this fourth phase is difficult to describe systematically, but some of its consequences are evident. An emphasis on

understanding (rather than explaining) the lived experience of people making music (ourselves included) is paramount. Other emphases involve reflexivity and an increase in narrative representation that is descriptive, interpretive, and evocative (see, e.g., Kisliuk 1991); sharing authority and authorship with "informants" (who are now considered teachers, consultants, friends, or all three) (see von Rosen and Francis 1992; Guilbault 1993); a concern for history and with issues of power relationships, ethics, identity, and belief; a deconstructing approach to boundary concepts such as race and ethnicity; close attention to how class and gender operate within music-cultures;[3] skepticism toward the culture of science and engagement with feminist and third world perspectives; a willingness to explore various media, such as museum exhibits, festivals, film, video, and hypertext, to represent people making music; and an active involvement as musical and cultural advocates trying to help people in the music-cultures with whom we work have better lives in which their music can flourish (Sheehy 1992). All of these emphases are implicated in "the new fieldwork" and many are generated by its emphasis on human relationships rather than on collecting information. The new fieldwork does not abandon musical sounds and structures, it just repositions them as "texts" (subjects of interpretation) in a hermeneutic circle (Ricoeur 1981a). Musical sound is still documented, and if musical structure is an important aspect of the musical experience, as it so often is, then it is analyzed and interpreted as part of the matrix of meaning. Nor does the new fieldworker abandon documentation; if anything, documentation increases. But documentation, too, is repositioned, and is now considered reflexively, as an inter-subjective product, rather than as the report and analysis of a witness.

If we enlarge the history of our discipline to include understanding-type theories, then we will be sure to attend to some of the writings of the early world travelers and missionaries whose understanding of native music took the form of an encounter with it. Jean de Léry, for example, a sixteenth-century missionary, narrated an account in which he told how he was "captivated" by native American music, and in doing so he weighted his narrative toward experience (Harrison 1973). In the revised histories we will emphasize "bimusicality" (Hood 1960; 1963; 1982[1971]), and ponder the nature of knowledge that comes through the human relationships developed through fieldwork. David McAllester's early work with the Navajo and cultural values will take on profound importance (1973[1954]; see also Mitchell 1978), and Kenneth Gourlay's articles on the ethnomusicological researcher become key early theoretical statements (1978; 1982).

Our approach, whether we favor explanation or understanding, will obviously depend on what we think music is. In my view, music is a socially constructed, cultural phenomenon. The various cultural constructions enable people to experience it as patterned sounds, aesthetic objects, ritual substance, even as a thing-in-itself. But to say that music is a culturally constructed phenomenon does not mean that it has no existence in the world, for like everyone I know, I experience

my world through my consciousness, and I experience music as a part of my lifeworld.

In the rest of this chapter 1 offer phenomenological and reflexive answers to questions concerning what we can know about music, and how we can know it. I begin by examining experiences of music as they are presented to my consciousness. I proceed by examining experiences of fieldwork. Finally, I discuss some interactive strategies for representing these experiences so as to enlarge our understanding of music. Of course, there is no single phenomenology. Husserl's transcendental phenomenology is significantly different from Heidegger's existential phenomenology, which is different from Ricoeur's hermeneutic phenomenology. Nevertheless, they constitute a tradition and have certain common assumptions and emphases. In what follows, I will draw from this tradition without attempting to represent any single version of phenomenology. Indeed, I do not find any single version of it wholly satisfactory.

"Phenomenology insists that phenomena be investigated as they present themselves to consciousness" (Stewart and Mickunas 1990:91). Consciousness is always consciousness of something: in this case, music. How am I conscious of music? How am I "in the world" when my consciousness is consciousness of music? First, of course, my consciousness of music constitutes an experience of music, and this is culturally mediated; obviously, my experience of music is bound to be different from someone else's in another culture, not to mention others in my own. And I experience various musics differently over the course of my life. But for the moment, let me attend to my ordinary and current consciousness of music, both generally and in one particular case.

I take people making music as my paradigm case of musical "being-in-the-world." For me, making music is incomplete when I do it by myself; it is completed in a social group when I make music with others. You may or may not feel the same way, but I want to take making music with others for my paradigm case. I could have chosen making music in a string quartet, a gamelan, a blues band, an Old Regular Baptist church, or a Ghanaian drum ensemble, but for this exercise I choose making music in an old-time string band, with fiddle, banjo, and guitar—a peak experience which I consciously seek and find.

Here is how I would describe this experience phenomenologically. Desire compels me to make music. I feel this desire as an affective presence, a residue of pleasure built up from my previous experiences with music and dance that makes me seek it out in order to know it better. It is a curiosity of all my bodily senses and I feel it embodied in them: an embodied curiosity. Knowing people making music begins with my experience of music. Playing the fiddle, banjo, or guitar with others, I hear music; I feel its presence; I am moved, internally; I move, externally. Music overcomes me with longing. I feel its affective power within me. Now I have moved from what phenomenologists call the "natural attitude," the normal everyday way of being in the world, not to an analytical way but to a self-aware way.

I feel the music enter me and move me. And now the music grows louder, larger until everything else is impossible, shut out. My self disappears. No analysis; no longer any self-awareness. The shutting out is a phenomenological reduction, what Husserl called *epoché*. It is a radical form of suspension. I no longer feel myself as a separate self; rather, I feel myself to be "music-in-the-world." Eventually music returns me through desiring to myself. That is, the be-ing of desiring brings me to myself, re-presents myself to consciousness. The "I" returns; I am self-aware, I see that I and others are making the music that I hear.

When I see that I and others are making the music that I hear, I want to know these others. For us to understand one another we must know one another. How may we know one another? Who are you? If you were an object I might come to know you as I know other objects. But you are a person making music and I come to know you as a person (see Code 1991:37). We seek to know one another through lived experience. Through common, intersubjective experience we enter the world of interpretation. Interpretation turns sound into music, be-ing into meaning.

When my consciousness is filled with music I am in the world musically. My experiencing mind tells me that I have a musical way of "being-in-the-world" when I make music and when I listen and move to music so that it fills my body. I call this musical being, and it is a mode of being that presents itself as different from my normal, everyday modes of experiencing, from my self-conscious modes of experiencing, and from my objectivizing modes of experiencing.

I would like to ground musical *knowing*—that is, knowledge of or about music—in musical *being*. I look, in other words, for an epistemology of music that is grounded not in a detached or objectivizing way of "being-in-the-world," nor in a reflexive, self-conscious way of being in the world, nor either in what phenomenologists call the "natural attitude" or everyday way of being-in-the-world. Rather, I think that musical being is a special ontology and that knowing music requires that we start from musical being.

Another way of saying this is that I ground musical knowledge in the practice of music, not in the practice of science, or linguistics, or introspective analysis. In my paradigm case of musical being-in-the-world I am bound up socially with others making music and when that music is presented fully to my consciousness it is the music of the whole group, not simply "my" music, although at peak moments I feel as if it is all coming through me.

This brings me to my experience of doing fieldwork, for it, too, is an experience of myself in relation to other people. For many ethnomusicologists, fieldwork is intersubjective and personally transformative. Like many of my colleagues, I experience fieldwork not primarily as a means to transcription, analysis, interpretation, and representation, although it surely is that, but as a reflexive opportunity and an ongoing dialogue with my friends which, among other things, continually reworks my "work" as "our" work (see also von Rosen and Francis 1992; Hutchinson 1994). Risking immodesty, I offer a recent example: a letter from

one of my Old Regular Baptist friends in which he said, "Thank you for the way you have helped us look at ourselves" (Elwood Cornett, letter to Jeff Todd Titon, August 18, 1993). And I thank them reciprocally. My experiences of fieldwork have usually been intensely lived; in them I have become acutely conscious of my roles, stances, and identities; I have felt love, camaraderie, and anxiety. Most representations of ethnomusicological knowledge, of course, exclude expression of the experience of fieldwork, but a phenomenological approach to these representations requires its inclusion and the inquiry into values that it generates.

A reflexive look at the types of relationships fieldwork engenders reveals that fieldworkers, and those who are the subjects of fieldwork, bring identities to the encounter and are cast in a variety of roles (Titon 1985). By role playing I do not mean to imply inauthenticity, but rather to use the concept as the sociologist Erving Goffman developed it, to show how people behave socially in daily life (1959). In the postcolonial world, when mere collecting is considered exploitive, and when some peoples simply will not cooperate with visiting ethnomusicologists, it is naive to think that the ideal field relationship will always result in friendship. Sometimes a kind of contractual relationship, implicit or explicit, in which each party helps the other, is more effective. Sometimes a combination of friendship and tacit contract is most effective. In another frequent role in the new fieldwork, the ethnomusicologist becomes student and the "informant" becomes teacher or wise elder. Infrequent and atypical roles include opposition, deception, lying, and spying—unethical under most circumstances, but rationalized on the grounds that the music-culture being understood and then exposed is illegitimate and corrupting (see Pillay 1994; Kingsbury 1988).

A phenomenological epistemology for ethnomusicology arises from our experiences of music and fieldwork, from knowing people making music. If we believe that knowledge is experiential and the intersubjective product of our social interactions, then what we can know arises out of our relations with others, both in the field and among our colleagues where we live and work, and these relations have an ineluctably personal aspect to them. The documents (texts) that we and our friends generate in the field have a certain immediacy to them—field notes, photographs, recordings—that remind us, when we are no longer in the field, of those relationships.

While we are with our friends, these documents appear—at best, and when they do not get in the way—not so much as objectifications but as extensions of our relationships. But when we are back from the field, in the university, in the library, or study, alone, particularly if our friends are far away, these field artifacts take on a very different cast. They substitute for experience by evoking our memories of it. Like a photograph taken or a brochure brought back from a holiday abroad, they are documentary and evocative at the same time. They traffic in nostalgia. In their presence, and the absence of the people I knew, I experience loneliness and longing. My task now is to represent the music-culture where I have

worked, not only to students and colleagues, but also to the people in that music-culture. I search for forms of representing that will keep my experiences before me, in memory, and evoke the people making music whom I have known. Thus, I represent them to myself as well. The conventional representation that presents itself to me is narrative musical ethnography; two other forms that I will discuss are ethnographic film and hypertext/multimedia.

Narrative, of course, is the way we habitually tell ourselves and others about our experiences, and so it emerges as a conventional form in phenomenologically weighted representations of people making music. At its best, a narrative weighting in the descriptive ethnography of a music-culture invites the reader to share, imaginatively, in the experiences that are represented. Anthony Seeger's *Why Suyá Sing* derives much of its interpretive power and authority from narrative (1987b). Not that the book is entirely narrative, of course. For Seeger and others writing narrative ethnomusicology, ethnography becomes an experience-weighted genre in which narrative includes background information, interpretation and analysis, and above all one in which insights emerge from experience: one shows how one comes to understand (see also Feld 1990; Rice 1994). Narrative is not new to ethnomusicology. Mantle Hood's narrative passages in *The Ethnomusicologist* (1982[1971]) and Bruno Nettl's stories in *The Study of Ethnomusicology* (1983) are among their most telling. And experience-based narrative interpretation is increasing in cultural anthropology as well. Instances abound. Renato Rosaldo's *Culture and Truth*, for example, begins with his celebrated article "Grief and a Headhunter's Rage" (1993[1989]:1–21). Rosaldo could not understand how grief and rage "go together in a self-evident manner" for the Ilongot of the Philippines until his wife died as "she was walking along a trail with two Ifugao companions when she lost her footing and fell to her death some 65 feet down a sheer precipice into a swollen river below. Immediately on finding her body I became enraged. How could she abandon me? How could she have been so stupid as to fall? I tried to cry. I sobbed, but rage blocked the tears" (1993[1989]:9). Rosaldo had to experience a combination of grief and rage himself before he felt he could fully understand this aspect of Ilongot culture.

As Clifford Geertz has pointed out, writing good ethnography takes a great deal of rhetorical skill, and it forces us to face the fact that we are primarily authors, not reporters (1988). But if we are authors, we risk displacing the reader's interest from the people making music whom we are writing about, to ourselves. Auto-biographical narrative ethnography has generated opposition from those who find it self-indulgent and unprofessional; indeed, the popular term for it, "confessional," indicates the problem of displacement. Yet narrative ethnography need not displace the attention from people making music to authors' consciousness. Instead, an author may skillfully work up a scene and cast herself or himself in the role of a bit player, someone whose participation isn't very important during the event, but whose reflections on it afterwards serve as a kind of interpretation. This,

after all, is what Geertz does in his celebrated essay about a Balinese cockfight, although one may pause at Geertz's literary method of divining meaning and wish that it were more congruent with the Balinese people's own views. The prologue and first chapter of *Powerhouse for God* are also written as narrative ethnography, carefully utilizing tape recordings, photographs, fieldnotes, and recollections of my experience to recreate and evoke the scenes of a luncheon conversation and a homecoming worship service (Titon 1988). Finally, narrative ethnography is well suited to showing an ethnomusicologist in dialogue with people making music.

Film's (and video's) images and synchronized sound are conventionally understood to portray people making music and to place the viewer in the position of observer. Film's evocative power is extraordinary: we feel as if we are watching something real. Of course, it is possible to defeat the experiential aspects of film by making films that imitate books or by making films that represent scientific experiments, as much ethnomusicological filmmaking attempts to do. But a phenomenological approach to filmmaking attempts to involve the viewer by evoking and reflecting on the experiences and relationships that obtain in a musical community. This relationship between the filmmaker and viewer can take one of three forms: the filmmaker can place himself or herself in a fully authoritative position, usually through an omniscient narrator; the filmmaker can depart, ghostlike, from the film, making it appear that the viewer is merely looking at the action and eavesdropping; or the filmmaker can in the film itself interact with the subjects and the viewer, and both can reflect on the meaning of the film. It should be plain that interactivity and reflexivity is best suited to the kind of experiential understanding that arises from fieldwork and music making (von Rosen and Francis 1992; von Rosen 1992).

Hypertext and multimedia are a third means of representation that seem to me to do justice to an experiential bias toward people making music. Whereas a narrative text is a linear read, hypertext can be a weblike structure that allows readers to choose their own paths through the assembled information (Landow 1992). A computer is not required for hypertext, but a computer enables hypertext very efficiently, Interactive hypertext empowers readers ("authors-who-are-to-be," in hypertext fiction writer Michael Joyce's words) to comment on the information and thereby alter it for the next reader. Multimedia is often allied with hypertext to represent sound recordings, images, and movies. A carefully assembled hypertext is capable of representing the insights as well as the ambiguities of the experience of acquiring knowledge through fieldwork. For example, in the Davenport Hyper-Card Stack, a reader hears fiddle tunes and is told that they seem similar (Titon 1991). One path leads to musical analysis, and transcriptions demonstrate the tunes' similarity, but another path leads toward the fiddler himself and his demonstration that the tunes are different. The representation leaves it to the reader to resolve the paradox. Or not resolve it. A further development is hypertext fiction (see Coover 1993).

Not all hypermedia projects allow meaningful interactivity. Many "educational" hypertexts are nothing more than huge text-and-context assemblages with very efficient links, organized hierarchically rather than in a weblike fashion. The experience of such hypermedia "learning" environments is not much different from the experience of being in a library, where one seeks explanations. But the experience of a weblike hypertext is more like the initial stage of playing a game: one seeks to understand it.

In this chapter I have maintained that we have usually sought to explain musical sounds, concepts, and behavior rather than to understand musical experience. And yet our own most satisfying knowledge is often acquired through the experience of music making and the relationships that arise during fieldwork. It seems to me that in our ways of being musical, and in our ways of doing fieldwork, we, like the subjects (people) of our study, are open to transformations through experience. Furthermore, when we ask our musical friends for their "native" points of view or overhear what they say, they most often speak in terms of personal experience and understanding rather than offer systematic explanation.

If all of that is so, then an epistemology erected upon the ethnomusicological practices of music making and fieldwork as the paradigm case of our being-in-the-world, rather than upon collecting, transcription, and analysis as that paradigm case, will privilege knowledge arising through experience, ours and others'. And in our external representations of that knowledge, we seek those forms that best produce understanding. If we must rule out such unconventional representations as fiction or musical performances, because these are not available to scholars, at least not now, then narrative but not necessarily self-centered nonfiction writing, interactive and reflexive rather than authoritative or merely observational film, and weblike, interactive hypermedia are promising forms of representation that will convey understanding both in us, in the process of their formation, and in those with whom we seek to communicate. Yet I do not wish to dispense altogether with explanations as a form of knowledge, only to privilege understanding. I cheerfully admit that I continue to practice transcription and analysis, and to be curious about issues involving musical structures, history, and geography. An epistemology of musical knowing that follows from our musical being-in-the-world privileges experience and understanding, but it cannot possibly do without explanations because, after all, we also experience knowing by means of explanations, and we put those to work in daily life.

What of the future of fieldwork? If, as I have claimed, contemporary ethnomusicology rests epistemologically on fieldwork, then the poststructuralist challenge to fieldwork must be answered if the discipline is to continue. Indeed, some have called for the abolition of ethnomusicology. This critique is mounted on several grounds, three of which are central. The first is the charge, familiar since the late 1960s, that fieldwork-based enterprises rest on asymmetries of power and therefore involve the illegitimate use of the fieldworker's authority. In other

words, fieldworkers have no legitimate right to represent their informants, for their purposes are not neutral—after all, ethnomusicologists' careers ride on these representations. The informants are the ones with the proper claim to authority, and they should be the ones to write—or not write—the ethnomusicological texts. A second charge is that fieldworkers enact a version of the heroic quest, although they do not realize this. The consequence is that musical ethnographies fall into a single pattern—the quest narrative, implicit or explicit. The problem is that the quest pattern, rather than the musical life of the culture under study, governs the representation and interpretation of the data. Thus, for example, music-cultures are viewed as utopian or dystopian, and ethnomusicologists become heroes, flawed heroes, or antiheroes (see e.g., Hood 1982 [1971], where he writes of the importance of the ethnomusicologist's role in helping to build a large gong). Moreover, as a questing hero the ethnographer can scarcely claim authority to represent another music-culture: the hero has a different agenda. A third charge is leveled on epistemological grounds. Poststructuralist thought denies the existence of autonomous selves. The notion of fieldwork as an encounter between self and other is thought to be a delusion, just as the notion of the autonomous self is a delusion, whereas the notion of the other is a fictionalized objectification.

Neither the poststructuralist challenge nor a variety of answers can be considered here in the detail they deserve. But the beginnings of an epistemological answer may be found in the preceding phenomenological account of music making. Making music, I experience the disappearance of my separate self; I feel as if music fills me and I have become music in the world. But I also experience the return of the knowing self. The experience of music making is, in some circumstances in various cultures throughout the world, an experience of becoming a knowing self in the presence of other becoming, knowing selves. This is a profoundly communal experience, and I am willing to trust it. A representation grounded in this kind of experience would, I believe, begin to answer the poststructuralist challenge by reconfiguring the ethnomusicologist's idea of his or her own self, now emergent rather than autonomous. Autonomous selves enact heroic myths. Emergent selves on the other hand are connected selves, enmeshed in reciprocity. Connectedness is a value that challenges the postmodern critique of contemporary society. I am willing to assert this ecological value and its intimate relation with music-making and fieldwork on the grounds that the survival of far more than ethnomusicology depends on it.

Postscript: 2006

I recall a conversation over lunch one day more than a dozen years ago with my friend Patrick Hutchinson, an uilleann piper. I remarked to Patrick that in order to play music well with old-time musicians (that is, among those of us who play southern Appalachian string band music on fiddle, banjo, mandolin, and guitar)

"you have to know how to visit." Visiting means treating others with respect, care, modesty, courtesy, exchange, and reciprocity. It means establishing a sound and hopeful relationship before "getting down to purpose," if there is any purpose to get down to. It means that "special kind of courtesy," as Michelle Kisliuk (1988) wrote about bluegrass jam sessions, that leads to good music—though it does not always do so; yet without it, good music (by which I mean group musical experiences felt to be good, in a way that is surprisingly delightful, even magical, while they are happening) is difficult, if not impossible.

Hutchinson replied that it was the same thing with Irish traditional music: you have to know how to visit. And his dissertation (1997), conceived well before he met me, is all about visiting (always in his teacher Chris Langan's kitchen, tea and talk first, piping after), friendship, and what Hutchinson, who eventually tied for first place in the All-Ireland uilleann piping competition, learned over the years about musical being-in-the-world from Chris. And not just from their making music together, but from Chris's talking of music; for Chris had been a blacksmith, and his ideas about setting a tune rose to art in crafting metaphors that enlarged understanding as he brought them effortlessly from blacksmithing into piping.

Visiting, friendship: these are the products of a music-making epistemology, and they ground fieldwork in a musical being-in-the-world. They implicate music, not language (talk, writing), as the basis for knowing people making music. Language, of course, is usually taken as the basis of communication. I continue to suggest that we remove it from the center and replace it with music. The inconsequentiality of signified meaning in musical sound, thought to be an impoverishment when compared with language, turns out to be an advantage, when we consider grounding ethnomusicological fieldwork in the relationships that arise from musical being-in-the-world. Language is inherently unstable, signifying multiple meanings, subject to differing interpretations. The language of literature gives us the heroic quest myth, the basis of scientific distancing, manipulation of objects, investigative reporting, our spying on the world. But this is an unreliable intelligence when human beings are our subjects. Music, conceived not as a signifying language but as a collaborative relationship among the people making it, gives us, at those magical moments of self-transcendence, a connection among living beings leading to friendship and thus the basis for an epistemology of fieldwork based in musical, rather than linguistic, being-in-the-world.

Visiting, friendship: in theorizing this friendship model of collaborative fieldwork it is useful to distinguish among two points on the spectrum of friendship. One might be called an instrumental friendship, in which there is a quid pro quo, an unwritten contract-like relationship in which each benefits from the other's continued presence and work. The usual relationship between the fieldworker and chief informant, translator, advocate, consultant, teacher, however the relationship is best described, is always to some extent instrumental in that regard. Each is useful to the other; they are perceived as partners and to a degree they are.

The other point on the spectrum is one in which the friendship does not depend on usefulness, quid pro quo, or partnership, but rather is founded on admiration and a desire for the other's well-being. Now, an interesting thing about the social musical experience I wrote about in the earlier edition of this book, is that the disappearance of the self and the co-becoming of music in the world suggests that same desire for the other's well-being. In other words, this kind of musical experience is always moving us along the spectrum from quid pro quo toward that selfless desire. To be sure, there is a quid pro quo in the musical experience; each of us contributes our part and if someone, something, isn't felt to be quite right, it puts the peak moment out of reach. Instead, one compartmentalizes and is very much aware of one's separate self, and everyone else's separate self. But to come back to the point: if we want to ground fieldwork with people making music in the experience of making music with people, then we will be led to theorize our relationships in fieldwork on the very same grounds as our relationships in music making. And this leads to friendship, and that leads to visiting.

Visiting, *ceilidh*: In *Passing the Time in Ballymenone,* Henry Glassie describes the old Irish tradition of the *ceilidh*, or visit among friends, where modesty rules and life is sometimes coaxed into art (1982). Singing and music and storytelling and *craic*, or good talk, conversation raised to art, are sought and found in these visits that connect and reconnect friends. That good talk, intersubjective, is always a part of good fieldwork, where again mutual discovery is a sought and found experience. Visiting, then, is the social basis for fieldwork.

Now, visiting does imply decorum, a degree of distance, a certain privacy; and decorum can get in the way of, or postpone, understanding. In the long term, though, friendship and visiting offer a more sustainable way of getting to know people making music than the usual conception of fieldwork as a quest for knowledge. For that quest is born in an archetypal heroic myth, and it is a quest both for knowledge and power.[4] If you balk at a comparison of fieldwork with heroic quest, consider these parallels: long preparation, instruction from presumably wise elders, and apprenticeship (i.e., graduate school); the journey to a strange land, accompanied by various devices (not magic swords or incantations, but pens, paper, and instruments to record sounds and images); the struggle with alien ideas and forces (maintaining one's identity, difficulties in making sense of it all); and the triumphant return home to the kingdom (to write the book and share the knowledge inside the academy), with rewards bestowed (degree, job, promotion).

While the knowledge side of the quest myth may appear innocent, the power side remains implicated as an adjunct to colonialism, despite ethnomusicologists' frequent proclamations against ethnocentrism and on behalf of musical relativism.[5] As for the knowledge side, literary critics have pointed out the irony that while the heroic quest may impart knowledge, the hero often cannot communicate it upon return to the kingdom; the monster may be slain, but as the knowledge was

in the hero's experiencing, it is unavailable to the others: they have not lived it. Contrast the quest myth with the fieldworker as visitor, as guest, rising to friend. That is the model I support, and of course it has implications for human relationships well beyond ethnomusicology.

Ruth Hellier-Tinoco has questioned my "friendship model" for ethnomusicological fieldwork, pointing out that friendship, with its complications and long-term obligations, can be very difficult, perhaps too difficult, to maintain, without some sense of guilt and failure (2003; see also Cooley 2003 for a discussion of fieldwork and friendship). No doubt that is true, as in friendships generally, though often under more difficult circumstances. Does fieldwork imply distance? Does friendship dissolve fieldwork, make it into something else? I have written elsewhere about my ambivalence in conceiving of what I was doing as fieldwork, with blues musicians who were my friends long before I learned the term "fieldwork" (Titon 1995:264; 2003:119–31). The small literature on the friendships that arise from fieldwork also includes Joseph Casagrande's pioneering collection of essays (1960) *In the Company of Man*, and the more recent *Bridges to Humanity*, edited by Bruce Grindal and Frank Salamone (1995), with discussions of postcolonial complexities and their implications for fieldwork and friendship. In the introduction to the latter volume, the authors caution that "at least a portion of some field friendships is based on mutual, even if unacknowledged, gain; and once that gain is no longer present, the friendship no longer has active value" (pp. 2–3). Indeed, Hellier-Tinoco questions my term "fieldwork model" on the basis that "model" sounds too calculating. I do not intend it to be calculating. I mean to describe a way of thinking about sustainable fieldwork relationships. In this essay I have tried to theorize field relationships for ethnomusicologists based on musical being-in-the-world. Friendships typically involve both mutual gain and caring for the other. The act of bringing into the world expressive culture rising to art, as in music, story, or just good talk, renews the connection of friendship and grounds it in mutual, shared experience—which *is* a mutual gain.

Notes

1. This chapter is a revision of a paper presented at the annual conference of the Society for Ethnomusicology, Oxford, MS, October 25–29, 1993. Earlier versions were read in hypertext form to the Ethnomusicology Graduate Students' Colloquium in Fieldwork, Brown University, March 4, 1993, and to the Annual Conference of Finland's Society for Ethnomusicology, at the Sibelius Academy, Helsinki, April 1, 1993. The latter was published as Titon 1994. For an exchange of ideas I am grateful to Gregory Barz, Alan Bern, Timothy Cooley, Stephen Green, Katherine Hagedorn, Susan Hurley-Glowa, Patrick Hutchinson, Kathy McKinley, Jill Linzee, Nancy Newman, and Franziska von Rosen, students in the graduate seminars in fieldwork and in the history of ethnomusicology that I have taught at Brown University since 1986, in which we have discussed paradigms, phenomenology, interpretation, and a more humanistic ethnomusicology.

2. "Making" in two senses: (1) producing the sounds that we call music, and (2) constructing the cultural domain that we demarcate as music.

3. These boundary concepts continue to be discussed in an electronic conference on multiculturalism, primarily among ethnomusicologists, hosted since 1992 by Marc Perlman at Wesleyan University. The internet address is MC-Ethno@Eagle.Wesleyan.edu.

4. Susan Sontag's "The Anthropologist as Hero" (1994[1963]), a reading of Lévi-Strauss's *Tristes Tropiques* (1961), brought this idea to my attention years ago, and in the 1970s I devised and for several years taught a course entitled *Inventing Anthropology*, in which we explored the quest myth in popular ethnography as well as literature, reading ethnography as literature.

5. Especially Bruno Nettl, who in explaining the discipline many times over for fifty years, seldom misses the opportunity to state that ethnocentrism has no place in ethnomusicology. Reacting against those who have claimed Western art music is the finest music that humankind has produced, we proclaim a degree of relativism in which every music culture is entitled to respect and serious study. Of course, we each have our personal preferences among the musics of the world, but we would not want to say that those we prefer are worthwhile and those we don't care for are not.

Toward a Mediation of Field Methods and Field Experience in Ethnomusicology

Fieldwork is so central to contemporary ethnomusicology that I would suppose nearly every graduate program devoted to training ethnomusicologists has a course on it, probably with a title containing the phrase "fieldwork methods." The three nouns in these courses' titles (method, field, and work) speak volumes about our collective understanding of theory and method in ethnomusicology, and thus provide the place where I would like to begin this reflection on fieldwork. I hope to show the limits of the usefulness of this phrase and the possibilities for an alternative view of where the field is and what happens in the field, a view that balances method and working with experience and playing. To do so, I move narratively between theoretical or philosophical reflection and some of my own fieldwork experiences, just as I do in practice.

Fieldwork Methods: Epistemological Solutions to an Ontological Problem?

Method

The word "method" implies both a pre-existing theory and a concern with the epistemological problem of finding, verifying, and knowing the truth within the frames of reference defined by theory. A course with the title "fieldwork methods" implies that ethnomusicology has a theory or theories for which fieldwork methods have been developed to test, and the existence of such courses implies that these methods can be taught. But does ethnomusicology have such theories, and, if so, what are they, and what are their associated methods?

One view of ethnomusicology is that it has been rather untheoretical in its orientation, especially since the mid-1950s when, in the United States, it broke away from comparative musicology and its theories of evolution and diffusion. This

view is expressed by those who question whether ethnomusicology is a discipline (presumably disciplines have theories) and who, on the contrary, assert that it represents merely a domain of interest shared by a community of scholars. Those who support this theoryless view of ethnomusicology need only cite the myriad idiographic studies of individual musical cultures that dot the ethnomusicological landscape and the relative paucity of recent attempts to posit and test explicit theories. From this perspective, what passes for theorizing in ethnomusicology amounts for the most part to retrospective catalogues of what has been done and prescriptions for what might be done rather than statements of relationships to be explored in the field. This sort of theoryless ethnomusicology would have no use for methods; indeed, methods, whether applied in the field or in the laboratory, are impossible to define in the absence of theory. Perhaps professors with such views don't offer courses called fieldwork methods; rather, a title such as "things to do in the field" would suffice.

A second view of ethnomusicology acknowledges that, although no single theory predominates, ethnomusicologists currently work with many theories, and have woven a "polyphonic" theoretical fabric. If a discipline requires a single, unifying paradigm (Kuhn 1962), then ethnomusicology still might not qualify, but at least each of its contrapuntal theoretical lines would require a disciplined method. A multiplicity of theory would account for the plural form of the course title, "fieldwork methods." The polytheory view is advanced by those who claim that no descriptive work of the sort that minimally constitutes ethnomusicological writing can take place in the absence of some sort of theory. Alan Merriam made this point repeatedly in the definitional debates of the 1950s and 1960s. Bruno Nettl's *The Study of Ethnomusicology: Twenty-Nine Issues and Processes* (1983) provides a "book of lists" of ethnomusicological theories, although some of them predate the 1960s, when the idiographic, intensive studies based on extended fieldwork in one place, called for by Mantle Hood and others, became the norm.

To illustrate the point that multiple theories exist along with the correlative methods they require, I give three examples. One informal theory, for example, maintains that musical practices disappear, which leads to methods aimed at their accurate preservation as sound, film, or video recordings. A corollary of this theory-and-method combination is that a practice has been preserved when converted into a recording, that is, into a fixed text or monument—perhaps analogous to the way jam preserves fresh fruit. A second theory that undergirds some of our activity would state, if made explicit, that music exists as a "sound fact" to be interpreted and compared, at least by ethnomusicologists, using ordinary language description and Western musical notation. The main methodological questions raised by this theory concern accuracy (are the rhythmic durations and intervallic pitch relations correct?), systematicity (are the descriptive tools logically consistent and unambiguously understood?), and replicability (would others using the same tools produce comparable results?). A third theory states that music is a form of

human behavior created within a coherent cultural system, and therefore possesses structures analogous or homologous to other culturally constructed forms encoded as art, architecture, everyday speech, ideas about natural sounds, and cosmological or religious beliefs about the nature of the world. The methods characteristic of this theory involve describing and then finding ways to compare radically different formal structures and behaviors, typically through a reduction of those differences to a common structural model borrowed from linguistics and semiotics or through the elicitation of native metaphors and key symbols that link two or more cultural domains into a coherent ethnoaesthetic (Ortner 1973). This theory minimally requires fieldwork methods that go beyond the accurate preservation and description of music as an isolated cultural domain to the observation, recording, and analysis of other cultural domains as well.

The third view of the field holds that a large number (but certainly not all) of its practitioners share a core set of theories or beliefs that constitute the field as a discipline. Even Bruno Nettl, whose list of twenty-nine issues fits his view that ethnomusicology may be less a unified discipline than a field of interest, boldly asserted ethnomusicology's "central question" (1983:234) and offered a four-part ethnomusicologist's credo (1983:9), both of which, although he probably didn't intend them as such, could provide a place from which to search for our discipline. The credo seems particularly suggestive as a source for theories and methods that unify the discipline. In analyzing it, I will not question whether these beliefs are in fact widely shared, but rather show what they reveal about the link between theory and method in ethnomusicology.

According to the first part of Nettl's credo, we believe that music systems can be compared, so we need methods to determine "what is typical of a culture" and distinguish those items from "the personal, the idiosyncratic, the exceptional." Whether the methods would be applied as analytic tools in the lab or as social measures in the field he doesn't say. Second, "we believe that music must be understood as a part of culture," but he provides no shared belief about the methods that would elucidate this theory, aware as he is of the methodological counterpoint on this problem. Furthermore, he acknowledges that "many pieces of research do not directly address this problem," which amounts to the troubling admission that, in ethnomusicology, method (as actualized in "pieces of research") bears no coherent relation to a shared theory. Third, we believe that fieldwork, particularly "intensive work with small numbers of individual informants," is an indispensable method, even in the absence of any theory that it might test. If theory and method seem unlinked or incoherent in the second aspect of the credo, in the third aspect theory disappears altogether, although, to be fair, this belief in extended fieldwork should probably be linked to the theory that music is a part of culture. Fourth, "we believe that we must study all the world's music."

Two theories support Nettl's credo. The first states that music derives its value as an object of study not from the complexity of its formal properties or its

association with privileged social and historical groups, but by virtue of being a human activity—and all humans and all their behaviors are properly the object of scholarly inquiry. The second theory states that any claims about music that pretend to be universally true, or even true for a particular culture, have to account for all music, whether considered globally in broadly comparative studies or locally in characterizations of musical practice in a single culture. These two theories require methods that capture and consider all music within a culture and in the world, rather than methods designed to assess the relative value of particular genres, works, or musicians.

So what does ethnomusicology as a unified discipline, with its associated fieldwork methods, look like from the perspective of this credo? First, we need methods to collect and study all the world's (and a culture's) music so that we can make general, even universal, claims about the nature of music. We need methods to distinguish the typical from the idiosyncratic in the music we collect, presumably so that our comparative statements and universal claims have some statistical relevance. The methods we use during our extended, intensive fieldwork in one place, presumably to explain our theory that music is a part of culture, are apparently so disparate that they must be excluded from a shared credo. In fact, it may be possible to understand this relationship between music and culture in the absence of method, just by being there. If none of these theories appeal, then ethnomusicologists just churn out "pieces of research" in the absence of theory, and probably method as well. Read this way, Nettl's credo has the advantage of catholicity, but it returns us, ironically through shared beliefs, to a polyphonic understanding—and possibly an accurate one at that—of ethnomusicology without much shared method to go along with three shared theories. The one method we do share, extended fieldwork in one place, exists, according to this credo, unconnected to any particular theory.

In the 1990s, a case for a view of ethnomusicology as a unified discipline would probably be built around Nettl's credo that music is a part of culture (or is culture) but with the explicit continuation that fieldwork methods, including extended, intensive work in one place, exist or must be developed to demonstrate that relationship. This theory, that music is a part of culture, necessarily rests on a theory or theories of culture imported primarily from anthropology.

Anthropologists have "vexed" each other, to use Geertz's telling phrase, over the proper definition of culture, how it is manifested, and how it can best be observed and studied. At least three concepts seem in some sense primordial to the culture concept, though even these features have not escaped critique. First, culture, however defined, has to do with what is shared among a people. Second, cultures are bounded in space and often in time by the "ethnographic present"; we speak routinely and metaphorically of cultural boundaries that apparently block easy intercultural understanding. Third, bounded cultures contain insiders in relation to which the researcher, whether anthropologist or ethnomusicologist, is an

outsider. Attempts to understand music as a part of culture involve specifying methods for border crossings in order to live and work among insiders long enough to apply other methods designed to elicit the shared musical, speech, and other behaviors that would demonstrate this music-as-culture theory. It is probably not too bold to claim that the most frequently cited research in ethnomusicology since the late 1970s has attempted to define and apply methods designed to work out the implications of this theory.

The Field

In this review of the role of methods in relation to theory in ethnomusicology, the field emerges as the place where data are collected to test theories. It is a bounded place filled with insiders who share views about music, musical practices, and a host of other things. It is the place where we outsiders must go to encounter these insiders and their culture, and explain to other outsiders the relationship between music and culture posited by our theories. It is, above all, the primary place of knowing in ethnomusicology, a place privileged epistemologically by the theory that constructs it as the locus where methods will be applied to demonstrate the truth of our theory that music is a part of culture.

However, in this review of theory and method there was a suggestive alternative to this epistemological vision of fieldwork, an alternative that seemed unconnected to theory, that by implication left method behind. It was the third aspect of Nettl's credo: We believe in fieldwork. Fieldwork for what? Not apparently as a place to test and work out theory, an experimental place in other words, but a place to become an ethnomusicologist, an experiential place. This third aspect implies the belief that the experience of fieldwork, whatever its methods or even in the absence of methods, constitutes the sine qua non of the state of being an ethnomusicologist. In this credo we have the privileging of ontology (being there) over epistemology (knowing that), and the beginning of a potentially fruitful turn away from fieldwork methods toward fieldwork experience. According to this credo, sometime during or after fieldwork, one becomes an ethnomusicologist. In effect, the self is transformed and reconfigured in the act of understanding one's own or another culture.

The view outlined here that the field is a place of experiment and that fieldwork is an epistemological process exists in parallel and unconnected to the view that the field is a place of experience and that fieldwork is an ontological condition. It would be easy and tempting to demand that we choose one or another of these views; indeed, both sides could and have been the subjects of merciless critiques. But instead of choosing between the two positions, it may be more profitable to attempt a mediation between them. Could, for example, the transformative moment in one's "being-in-the-world"—in one's self, as it were—from non-ethnomusicologist to ethnomusicologist be understood as a particular example of more general transformative experiences during fieldwork that lead to new un-

derstandings? If the self rather than method were the locus of explanation and understanding (not, by the way, the solipsistic object of understanding), might this realignment contribute to the reformulation of theory and method? On the other hand, could theory and method, which take for granted a fixed and timeless ontological distinction between insider and outsider, be reordered within an ontology that understands both researching and researched selves as potentially interchangeable and as capable of change through time, during the dialogues that typify the fieldwork experience? Although such a mediation may be too ambitious for this chapter, it is on the horizon both of this chapter and of this volume.

Work

The emphasis on theory and its accompanying epistemological problems helps to account for the term "work" in our hypothetical course title, "fieldwork methods." Aside from the way "work" valorizes the enterprise as possessing at least the potential for generating both symbolic and economic capital within our own social and economic system (Bourdieu 1991), work must surely be necessary if an outsider is to cross cultural boundaries and enter a conceptually distant field—this last metaphor itself configures a place of unremitting physical drudgery—filled with another category of beings, insiders, whose workings with music as culture must be explained. But if fieldwork is reconceptualized as an ontological project, would the term "work" still be appropriate? When one *is* in the field, isn't existence also fun and playful, at least from time to time? And don't we, as human beings, enter into caring, as well as working, relationships with other human beings while in the field, even as we do our research, apply our methods, and test our theories? Could we not search for another mediation, along the lines of the one suggested earlier, between the epistemological, methodological work of explanation in the field and the ontological understandings of human and musical experience in the field? Would we whimsically retitle our course, "life-experience understandings in ethnomusicology"?

Playing, Caring, Experience, and the Understanding of Bulgarian Music

At Home in the Field

My own sense that concepts such as the insider–outsider dichotomy, the impermeability of cultural boundaries, and even the field as the privileged place of ethnomusicological understanding might need rethinking began not with reflections on theories and methods like those above but with various attempts to understand Bulgarian music, both in and outside what ethnomusicology traditionally defines as the field. The most important event that led me to rethink these ethnomusicological givens occurred when, after a long and unsuccessful period of trying to learn to play the Bulgarian bagpipe (*gaida*) from one of Bulgaria's finest

pipers, Kostadin Varimezov, I suddenly understood the basic kinesthetic principles that would allow me to play somewhat adequately in what I think is fair to call a virtuosic instrumental tradition (Rice 1985, 1994, 1995).

As part of my research on Bulgarian music, I decided that I should learn to play a traditional wind instrument, either the *kaval* (an end-blown, rim-blown flute) or the *gaida* (a mouth-blown bagpipe with one melody and one drone pipe). When I began this project, I was working on a dissertation topic; one of my goals was to discover native terminology used by women to describe their polyphonic musical practice (Rice 1977, 1980, 1988). I was distanced by gender and methodological stance from participating in what was essentially a women's singing tradition. In learning to play an instrument, I had no particular research questions I was asking, but as a musician I wanted to learn to play and participate in this tradition. In retrospect, I realize that I separated fieldwork from fieldplay at this early stage of my career in a rather unprofitable way. Epistemological methods and questions were associated with the former, and the ontological process of becoming a musician was the goal of the latter. At the time I gave no thought to whether and how these two positions might be linked or mediated.

Some years later I was able to invite Kostadin to come to the University of Toronto, where I was teaching at the time, as an artist-in-residence for the academic year. During this period we, along with his wife Todora, co-created a "field" in which collecting and interviewing work and learning to play were conjoined. I recorded their repertoire of instrumental music, song, and dance in the manner of epistemologically oriented fieldwork, and continued to learn to play, dance, and to a lesser extent sing in Todora's monophonic tradition, which was open to male performance. Even though I acted methodologically in the collection process, I didn't know where it would lead me theoretically, since I was more interested in music-as-culture questions than in music as sound fact, and I couldn't really observe music in Bulgarian culture in Toronto. In fact, I collected for two existential reasons. First, I imagined that the items I recorded would provide the repertoire for my existence as a musician, which it did. Second, I worried that, far from their close-knit, extended family, time would weigh heavily on them. I reasoned that spending time with me recording their repertoire and their life stories would make their stay more pleasant, which it did. My collecting in this case resulted from caring for them, not from theory or method. In retrospect, I would say that I had created a fieldwork situation that was structured ontologically rather than epistemologically, and with no particular expectation of a connection or a productive mediation between the two positions. It is probably unnecessary to add that even in the more typical situation of fieldwork far from home, there is no field there; the field is the metaphorical creation of the researcher.

The oddness, as fieldwork, of my research in Toronto only increased when the Varimezovs returned to Bulgaria and left me alone to continue on the path of becoming a musician in this tradition. (In Bulgaria, at least before World War II,

little boys who would be musicians were sent to a real field to learn on their own, out of hearing range of adults; I was, at home, metaphorically in just such a field, one rather different from the one constructed by ethnomusicologists.) Under Kostadin's supervision I had mastered certain aspects of the playing technique; I could play melodies in a number of different pentachordal modes and in the famous Bulgarian asymmetric meters (5/8, 7/8, 11/8, etc.), and I could separate melody notes one from another by creating low-pitched "crossing noises," to use the pejorative Scottish bagpiping expression, with the appropriate closed-fingering technique. (These noises are necessary because the bagpipe's sound cannot be stopped with the tongue, as on most wind instruments.) However, I had failed to understand how to create the characteristic high-pitched ornaments that seemed so crucial to the bagpipe's style. Kostadin could not explain them to me in words, gestures, or musical notation the way he could melody, rhythm, and articulation, and, whenever I tried to insert them by lifting my thumb before the melody note, he would complain that I had "lost the style" and that I didn't yet have "bagpiper's fingers" [*gaidarski prŭsti*].

If I wanted to *become* a musician, I now had to do so in the presence of the tradition in what I would now call its textualized form—as both recordings of Kostadin and my memories of lessons with Kostadin—rather than in the presence of informants and insiders. Again in retrospect, I would argue that this apparent liability, as understood from the perspective of traditional fieldwork, imitates, in fact, one of the experiences of acquiring culture generally. All of us who grow up in culture and acquire its traditions do so only partly as a result of direct, pedagogical intervention of the sort commonly associated with scolding by parents, teaching by teachers, or informing by informants; culture and its traditions are also acquired by observing, mimicking, and embodying shared practices (Bourdieu 1977) and by appropriating, understanding, and interpreting shared, symbolic actions (Ricoeur 1981c) without the direct intervention of parents, teachers, informants, and insiders.

Without Kostadin, but still determined to learn to play the *gaida* adequately, I analyzed the recordings by slowing the tape down, only to discover that the high-pitched ornaments were richer and more varied than I had imagined. Moreover, I realized that my mental image of how to move the thumb and forefinger of my top hand could not under any circumstances produce this dense ornamentation. Still, I struggled gamely on, trying to play with some, if not all, the ornamentation and to approach the speed with which Kostadin and other Bulgarian musicians played. Then, one day I began to think about one of what I now call the "textualized traces" of Kostadin's attempt to teach me to play. He had told me that the key to the ornamentation was in the *razsvirvane* (the "playing around"), a series of melodic phrases as the bagpiper fills the bag with air and starts the reeds of the melody and drone pipes. Each phrase begins with a long note on the highest pitch of the *gaida*, followed by an ornamented descent. To play the long note, the player

lifts simultaneously the thumb and two or three fingers of the top hand off the instrument. It suddenly dawned on me that if I did the same when I played the ornamentation, that is, if I lifted all my fingers simultaneously rather than, as I had been doing, the thumb first followed by the fingers necessary to produce the melody note, then I could produce the complexity and variety of ornaments that had proved so perplexing. This new kinesthetic understanding allowed me to play faster and more relaxed, and include more ornaments, than I ever had before. It sounded to me as if I had found "the style" I previously had "lost," acquired the elusive "bagpiper's fingers," and solved *le mystère des doigts bulgares*.

Between Insider and Outsider

It was this learning experience, at home in the Bulgarian version of the field to which children are sent to become instrumentalists, that caused me to reflect on some of the basic tenets of ethnomusicological theory and method. One of the most troubling questions was simple: Where was I? And I didn't mean the question just in spatial terms, that is, where is the field? Where was I in relation to ethnomusicological theory? And where was "I" in the temporal trajectory of myself becoming an ethnomusicologist and musician?

Until I found "*gaida* player's fingers" in the early 1980s, I had been strongly influenced by the methods provided by cognitive anthropology to develop a theory of culture as mental activity. Cognitive anthropology uses the elicitation of language terms to make inferences about internal rules, categories, and distinctions that "natives" employ when acting culturally and socially. Its positing of a contrast between "etic" (from phonetic) and "emic" (from phonemic) analyses seemed particularly attractive to me and other ethnomusicologists, who feared that Western-style (etic) analyses might ignore, misunderstand, or even violate important (emic) principles operating within a culture. Given that ethnomusicologists think and talk a lot about music, it seemed an attractive way to discover how natives think and talk about music, and thus gain insight into a supposed insider's perspective on musical and other forms of cultural practice. When I distanced myself from music making and tried to understand the Bulgarian insider perspective through words about music, I was happy with the results and felt that they represented significant advances over an outsider's etic analysis (Rice 1980, 1988), but when I fully engaged with the music, overcame my scholarly distantiation, and attempted to appropriate the style to the point where I could not just talk about it but play it as well, I ran into the limits of this language-based method and its associated theory of culture. I encountered precisely the "linguocentric predicament" that Charles Seeger (1977:47) would have predicted for me.

Starting with etic musical analysis and working with a native musician whose vocabulary for talking about music was limited, I had approached an understanding of the tradition, but there still was a significant gap between where I was

as an outsider to the tradition and where insider instrumentalists were. They knew it, and worst of all I knew it, too. Bulgarians have a theory to explain this gap: How could I ever really understand their tradition when it wasn't "in my blood"? And some ethnomusicologists have a comparable theory; outsiders are forever doomed to partial understandings compared to insiders, never mind that most Bulgarians can't play the *gaida* either.

When I finally solved the mystery of bagpiper's fingers, I did so in dialogue with Kostadin's tradition of playing, preserved in recordings, after my conversations with him had ended. In the process, I believe I moved to a place untheorized by the insider–outsider distinction so crucial to much ethnomusicological thinking. After talking to a cultural insider, which took me in the direction of an emic understanding of the tradition but not all the way there, I confronted the tradition directly as a sound form and kinesthetic activity, and made it my own in an act of appropriation that transformed me, my self, into something I hadn't been before, a person capable of playing in this tradition with at least minimal competence. This transformation did not, however, make me into a cultural insider; I was not, at least it seemed to me, a Bulgarian. While Kostadin couldn't explain his ornamentation to me in enough detail to make me understand it, I came to be able to explain it to myself and to others; I now understood the finger movements and other mental processes necessary to produce the *gaida*'s characteristic ornamentation. My understanding was neither precisely that of an outsider nor that of an insider. Although the linguistic methods of cognitive anthropology had helped me narrow the gap between emic and etic perspectives, I could not in the end close that gap completely. When, on the other hand, I abandoned those methods and acted musically, it seemed as if I fell right into the gap between insider and outsider, into a theoretical "no place" that felt very exciting, if not exactly like a utopia. I was neither an insider nor an outsider.

The perspective I had acquired in the process of learning to play competently (not necessarily well) was neither emic nor etic. It was my own. I could now supply from my own self-understanding verbal explanations of the complex mental processes necessary to generate this music, explanations that at least one insider, Kostadin, had been unable to supply. If emic understandings are located in other people's heads and given to us in their language reports, then my understanding wasn't emic. On the other hand, if etic understanding involves applying objective analytic methods to sounds without regard for their cultural salience, then my understanding wasn't etic either. I felt as if I had achieved a mediation between these two theoretical categories, these two ontological conditions, and that this mediation challenged fundamentally one of the most important theoretical foundations of our discipline. If I was right, I would eventually need to search for new foundations. But before doing that, I needed to return to Bulgaria and put my new understanding to the test by playing for Bulgarians.

Playing in a Field of Expanding Horizons

When I returned in the mid-1980s to Bulgaria, the ethnomusicological version of the field to which outsiders are sent to become experts, I was delighted when Kostadin and others confirmed my self-assessment of my understanding in a number of direct and indirect ways. He stopped asking me to leave out the high-pitched ornaments, and we worked on inserting them into all the necessary places. He could now show me where in the melody to use them by gesturing in midair with his "bagpiper's fingers" as I played—and I understood what he meant. One of his sons, an amateur player himself, noticed that the first finger of my top hand, crucial to the ornamentation, moved just like his father's, that is, he saw as much as he heard one of my "bagpiper's fingers," which I had acquired not from observation of his father's finger but in a metaphorical dialogue with his father's recordings. A younger bagpiper, who had also learned, as I had, by listening to Kostadin's recordings rather than by being taught and informed directly by him, recognized in my playing Kostadin's ornamental style: "It is as if you are listening to Varimezov."

I was, of course, pleased and excited by their comments, but one of the most touching moments for me occurred at a celebratory gathering of their extended family in a village near Burgas in the foothills of the Strandzha Mountains, the area where Kostadin and Todora came from and in which their tradition flourished. We sat outside under a grape arbor on either side of the traditional *dŭlga trapeza* [long table] that provides the locus of all Bulgarian celebratory meals. As we ate and drank, Kostadin played the *gaida* and Todora and a younger woman sang songs. During the evening an elderly neighbor, with an impressive mustache of the type worn mainly by older villagers, approached and sat down across from me. Kostadin introduced me as the professor who had invited him to Canada for a year, and told the man that I played the *gaida*. "Hah, an American plays the *gaida*," he almost spat out in surprise and disbelief. He then turned to me and ordered, "Play, and I will tell you whether you are a *gaidar* [bagpiper]." I thought, "Oh brother, there is no way I can satisfy this guy," particularly since my playing usually went to pot when I was nervous, as he had made me. Kostadin handed me his *gaida*, and I reluctantly began playing. When I stopped, to my surprise he smiled, seemed pleased, and said, "You are a *gaidar*."

In their comments and actions, these Bulgarians confirmed that my self-understanding was now leading to recognizably Bulgarian musical behavior. Although I wasn't a Bulgarian, I could act like a Bulgarian in the production of a complicated musical form, and when I acted like a Bulgarian in this particular way, they did too; that is, if the occasion were right, they danced. I could now enter into a dialogue with Bulgarians not just in their language but in their music and dance forms as well. Although I was no doubt an outsider ethnically, weren't they accepting me as something like an insider musically and therefore culturally? (After

all, music is culture, according to one ethnomusicological metaphor.) Actually, one Bulgarian took this connection between cultural performance and ethnicity further than I was willing to. During a village fair in 1973, a man, whom I had noticed scrutinizing me intently as I talked and danced but whom I didn't know, called me over and demanded to know where I was from and why I was there. (I worried that he was a member of the state security apparatus, with whom I had had a number of unpleasant run-ins.) I explained that I was an American living in Bulgaria for a year or so "on a specialization" to study its folk music. "Hah," he said, "you lie! You speak Bulgarian, and you dance Bulgarian dances. Therefore, you are a Bulgarian."

These sorts of interactions "in the field" suggest that categories of insider and outsider may not be particularly helpful terms to describe the kind of dialogic relationships in language, music, and dance that develop between people who perform and appreciate traditions they have each made their own in varying degrees. Just as I had tried to enter into the horizons of their tradition, they now seemed to accept and include at least some of my actions within the horizons of their understanding of that tradition. Perhaps, I thought, now was the time to begin in earnest the search for new theoretical and even philosophical foundations for ethnomusicology.

Field-Play Understandings: Ontological Solutions to an Epistemological Problem?

Remnants of Romantic Hermeneutics in Ethnomusicology

The recognition of a distinction between the knowledge of insiders to a culture and the knowledge of outsiders to that culture has been, since the early 1970s, an important, perhaps even central, aspect of method in, and a fundamental episte-mological problem for, ethnomusicology. The distinction is usually traced to a book published in 1954 by the linguist Kenneth Pike, who distinguished between what he called etic accounts of language and culture, which were based on the categories of scientifically trained observers, and emic accounts, which sought to understand the categories and meaningful distinctions of native speakers and cultural insiders. Cultural anthropologists in the late 1950s and 1960s found the distinction useful, and it spawned a number of new research paradigms variously labeled ethnoscience, cognitive anthropology, and the ethnography of speaking. It was these trends that influenced a new stream of ethnomusicological research in the 1970s and 1980s, including my own.

Nearly forty years after Pike's work, ethnomusicologists continue to discuss it, reinterpret it, define research projects and methods in terms of it, and criticize the limitations of the work based on it. A panel at the 1992 SEM annual meeting took it up, and in 1993 the journal *The World of Music* devoted an issue to it. At some level

the distinction seems axiomatic; after all, it is rooted in the very concept of culture and the concomitant notion of cultural boundaries. It is cultures with boundaries that define the positioning of insiders and outsiders. On the other hand, when we start analyzing this supposedly axiomatic distinction, we bore each other with questions and doubts. Isn't etic really a particular kind of emic? Is it a dichotomy or a continuum? Have we misinterpreted Pike's original idea? And on and on.

Pike's distinction has its roots in a philosophical tradition begun by René Descartes inquiring into the epistemological foundations of knowledge. Descartes felt that, only by doubting being, both in its supernatural form as defined in the religious tradition of the day and in its natural form as the perceptible world, could he understand the conditions limiting human knowledge. His radical doubt of supernatural and natural being led him to conclude that only ego could be known to exist without doubt; his argument, *cogito ergo sum*, might be glossed, "I doubt therefore I am." Descartes' doubt of the possibility of knowing anything about the world set in motion the long history of Western Enlightenment philosophy devoted to the epistemological questions "what do we know?" and "how do we know it?" This stream of Western philosophy spawned a seemingly necessary set of distinctions between the ego and the Other, subject and object, objective knowledge of observed behavior and subjective knowledge of inner experience, mind and body, the natural and social sciences, and insiders and outsiders. In this Enlightenment view, knowledge of the world is dependent on methodologically precise, objective observation. In turn, the limitations of these methods prevent us from examining inner experience and the intentions and meanings of others—at least without experiencing what might be called methodological embarrassment.

I undertook my search for alternatives to the Enlightenment position that privileged epistemological problems while doubting being and the existence of a world as the result of my fieldwork experiences. But I came to realize that such a search had already been started by nineteenth-century Romantics, who were desperately interested in understanding the intentions, abilities, inner experience, and motivations of Others, especially those Others they believed to be geniuses. A theologian, Friedrich Schleiermacher (1977), and a philosopher, Wilhelm Dilthey (1989), are usually credited with founding Romantic hermeneutics to interpret and understand the works of genius produced by Others. But in keeping with the Enlightenment and the scientific revolution for which it provided the philosophical foundations, Dilthey in particular suggested that one would have to understand the Other by analyzing directly observable behavior. Today much work in the social sciences, including ethnomusicology, relies in large part on this Romantic hermeneutic tradition, where the Other is now not the genius of Romanticism but the exoticized Other, the insider, of fieldwork methods. This work is simultaneously reluctant to give up on the possibility of objectivity, and possesses a new confidence that formal methods, like those of ethnoscience—a telling label, by the way, ethno*science*—can be applied to knowing something about the Other.

Although the Romantic hermeneutic tradition, which continues to influence much of the social sciences and ethnomusicology, fosters objective methods, much of twentieth-century philosophy, social sciences, and natural sciences, the last in the wake of the theory of relativity, has made us skeptical of even this much recourse to objectivity. It is now common to point out that the outsider stance is not objective but a particular kind of emic perspective with the backing of powerful institutions in powerful countries. But the opposite is also true, that is, we often continue to insist that claims to an emic perspective and to understanding the meanings assigned to behavior by insiders must be subjected to the same standards of validity and verifiability as objective inquiry. So, although many of us believe in something like multiple subjectivities and have abandoned the search for objective knowledge, we still tend to demand and trust in objective methods to demonstrate to colleagues our understandings of the other's intentions, feelings, perceptions, distinctions, and rules. It is at this contradiction that we really have to seek a new philosophical foundation for our ethnomusicology and our social sciences and to try to mediate the dichotomies we have inherited from the Western Enlightenment and pre-modern scientific traditions.

Phenomenological Hermeneutics as a Foundation for Ethnomusicology

The philosophical tradition that I have found most helpful in reinventing myself as an ethnomusicologist, because it seems to possess the potential for just such a productive mediation between experimental, objectivist strategies of observation and experiential, subjective knowledge of the force of meanings and intentions, goes by the name of phenomenological hermeneutics. It represents both a continuation and a break with the tradition of Romantic hermeneutics, which, in the work of Dilthey, has been so influential in the social sciences. The main thinkers and their works that have influenced me are Martin Heidegger's *Being and Time* (1978), Hans-Georg Gadamer's *Truth and Method* (1992[1975]), and Paul Ricoeur's *Hermeneutics and the Human Sciences* (1981c). Clifford Geertz's (1973, 1983) interpretive anthropology also participates in this philosophical project. I am going to review some of the main claims of this philosophical tradition, particularly those that radically challenge the Enlightenment tradition, which provides the foundation for so much of contemporary ethnomusicology. As I do so, the sources for some of the language in the previous two sections should become clear.

In phenomenological hermeneutics, the world, far from being doubted by the subjective ego, is restored to its ontological and temporal priority over the ego or subject. The world—or in our terms, the culture or the tradition—exists and the subject/ego is "thrown" into it. According to Heidegger, "being-in-the-world" is the ego's ontological condition before knowing, understanding, interpreting, and explaining. What the ego/subject comes to understand and manipulate are culturally and historically constructed symbolic forms such as language, dress, social

behavior, and music. In hermeneutic jargon, the unbridgeable gulf between subject and object is mediated as the subject becomes a self through temporal arcs of understanding and experience in the world. The self, whether as a member of a culture or a student of culture, understands the world by placing itself "in front of" cultural works. This sense of understanding a world is rather different from the notion that the outsider as subject must, through the application of ethnoscientific methods, get behind the work to understand another subject's (the insider's) intentions in producing the work. In the hermeneutic view, the subject, supposedly freed from prejudice by method, is replaced by the self, who inevitably interprets and understands the world before any attempt to explain it can proceed. Understanding, in this tradition, precedes explanation rather being the product of it, as it is in the Enlightenment tradition. This idea should be immediately attractive to ethnomusicologists, who have frequent opportunities to observe that highly sophisticated nonverbal musical understanding often exists in the absence of verbal explanations of it—precisely the case with Kostadin's knowledge of high-pitched ornamentation.

Since, according to this philosophical tradition, we understand our world in terms of pre-existing symbols, like language, before we explain it, our explanations are always conditioned by pre-conceptions and pre-understandings given to us by those symbols. The self-conscious task of bringing that understanding to language involves what Ricoeur (1981c:164) calls a "hermeneutic arc." If we take music to be one such symbol system, we can say that the arc begins with pre-understandings of music, either as a performer or as a listener who finds it coherent, and passes through a structural explanation of music as sound, behavior, and cognition, to arrive at an interpretation and new understanding of the world or culture referenced by music acting as a symbol. Phenomenological hermeneutics thus helps to recast the problem of understanding the experience of musical symbols from a fruitless and methodologically unsound search for an unknowable, subjective, psychological inner quality in the subject or the Other to an interpretation of the world that music references by a self operating within finite but expandable horizons.

The metaphor of horizons, which we use routinely in our pedagogical work ("Let's study this music to expand our horizons") but often replace with boundaries in our scholarly analyses of cultures, has been theorized anew by Gadamer. Rather than cultures with boundaries, Gadamer explores the metaphor of a world with horizons. Like the physical world, the horizons of an individual's social and cultural world change as he or she moves through space and time. Whereas Enlightenment philosophy leaves us with a certain confidence in a rational and fixed subject moving through the world, analyzing and in some sense controlling it while keeping it at a distance, hermeneutics suggests that the subject becomes a self in the encounter with the world of symbols. In other words, I became a *gaidar* (and an ethnomusicologist) in the encounter with Bulgarian music and musicians. The

notion of the subject as constant and above the world, as "reigning over objectivity," is an illusion (Ricoeur 1981c:190). It follows that, if such an independent subject existed, it would impose its interpretation on the world. In Ricoeur's view, on the other hand, the ego constructs itself as a self by being thrown into a world. In his view, appropriation, or "the act of making one's own that which was previously alien, . . . ceases to appear as a kind of possession, as a way of taking hold of." Rather, appropriation "implies instead a moment of dispossession of the narcissistic ego." Ricoeur continues, "By the expression self-understanding, I should like to contrast the self which emerges from the understanding of [symbols and symbolic action] to the ego which claims to precede this understanding. It is cultural works, with their universal power of unveiling, which give a self to the ego" (Ricoeur 1981c:192–93.)

My appropriation of Bulgarian bagpipe performance, although it began as a selfish desire to learn the tradition for myself and what it could do for me in the American world of scholarship and amateur performance of Balkan music, went as far as it did because I cared for Kostadin. He in turn began to pressure me to appropriate the tradition completely, that is, to transform myself into a *gaidar,* for himself. My self-transformation had become meaningful and important to him and his self-definition and self-regard. He did not remain the inveterate insider, but transformed himself and expanded his horizons in his encounter with me and my world.

Marcia Herndon, in her 1993 article in *The World of Music* issue devoted to the emic/etic dichotomy, wrote "I speak as myself; neither fully insider nor outsider, neither fully emic nor fully etic" (1993:77). I believe that I got to this place vis-à-vis Bulgarian culture, but by a different route. Herndon attributed her ontological condition to "my mixed-blood status with Cherokee coming from both sides of my family" (1993:77). In hermeneutic terms, however, all those who place themselves "in front of" recorded or performed musical works, whether or not they can claim any genetic relation to those who produced them, may be able to make this claim: I am neither insider nor outsider; I speak as myself, a self formed, reconfigured, and changed by my encounters with and understandings of Bulgarian, and indeed all kinds of other, musical works and performances.

For Ricoeur, appropriation is the process by which a scholar, or anyone thrown into a world, "struggle[s] against cultural distance and historical alienation." Since, in this starkly un-Romantic view, access to the inner experience of the Other is neither attainable nor sought after, one is left to interpret symbols and symbolic behavior in terms of the world or worlds they potentially reference, an understanding that is finite, changeable, multidimensional, forced to compete with other interpretations, and limited by the expandable horizons of the individual. As Ricoeur puts it, "It is because absolute knowledge is impossible that the conflict of interpretations is insurmountable and inescapable. Between absolute knowledge and hermeneutics, it is necessary to choose" (1981c:193).

When, as in ethnomusicological research, a new world of music is encountered, new understanding results when the horizons of the researcher's world are expanded to include at least part of the world that the new music symbolically references. From this perspective, the researcher seeks to understand not so much the inner experience of people from another culture, but rather the world suggested by music sounds, performances, and contexts. Because ethnomusicologists often find themselves at some cultural or historical distance from the traditions they study, appropriation is the dialectical counterpart of that initial distanciation. Even so-called insider ethnomusicologists, those born into the traditions they study, undergo a productive distantiation necessary for the explanation and critical understanding of their own cultures. Rather than there being insider and outsider ways of knowing, all who place themselves "in front of" a tradition use the hermeneutic are to move from pre-understandings to explanation to new understandings. Even an insider faced with a particular cultural work or performance may not interpret it in the same way as the insider who produced it and was "behind" it. In other words, not just scholars follow this hermeneutical arc. All individuals operating within tradition continually reappropriate their cultural practices, give them new meanings, and in that process create a continually evolving sense of self, of identity, of community, and of "being in the world."

More Field Experience and Dialectical Strategies

Ricoeur is a master of dialectical thinking. First of all, he identifies seemingly irreconcilable oppositions, like the one between objectivity and subjectivity (or, in another essay, between history and fiction), demonstrates how each side partakes of qualities of the other, and then finds a way to mediate the opposition by resetting the terms in which the opposition was proposed and seemed so primordial. If ethnomusicologists adopt his philosophical stance, then we will be forced into such dialectical strategies. We will no longer be satisfied with identifying and then choosing between the oppositions we generate. Just such a mediation between insider and outsider was what I attempted in the previous sections.

Other oppositions await mediation as well. For example, the experience that I described earlier of becoming a musician through an encounter with the Bulgarian tradition suspiciously echoes Mantle Hood's 1960 call for acquiring "bi-musicality." In the 1960s Mantle Hood and his students often seemed to acquire this "bi-musicality" in order to study and report on music "in its own terms" and for its own sake, with culture and history providing little more than a "context" in which music was made. Alan Merriam and his disciples, on the other hand, were calling for a study of music as culture that often seemed to ignore "the music sound itself" and to challenge the respectability, and certainly the relevance to music-as-culture studies, of the knowledge gained through "bi-musicality." As I mentioned, I was mainly interested in the music-as-culture metaphor when I began my study

of the *gaida.* In touting the scholarly benefits of becoming a musician in a tradition I wish to understand, have I entered a vicious circle? Am I merely reproducing the oppositions of thirty years ago? I don't think so.

One payoff of my self-transformation into a *gaidar* superficially resembles Hood's emphasis on music in its own terms, and that is my ability to explain aspects of the sound structure of the music from the perspective of a performer using the language of Western music theory and notation. Some of the description in the second part of this essay was devoted to such explanations, but my emphasis on the state of "being-in-the-world" with Bulgarian musicians also refocuses questions of music as culture beyond notions of the analogies and homologies that musical performances and other cultural actions possess to questions of the social and cultural relationships generated between the selves who make music in culture. When the field researcher begins to participate in meaningful cultural action, then the pragmatics of music and culture, that is, the study of the conditions underlying specific musical and cultural utterances, becomes the focus of investigation. When the field researcher engages in acts of musical interlocution, as I did, then the ontological condition of the self and other agents seems to compete with the ontological priority normally given to observable music and language behaviors and events in epistemologically driven ethnomusicological theory and method.

It was when I began to participate, even at my limited level, in musical conversations with Bulgarian musicians during field trips in the late 1980s, that I came to understand how Bulgarians, operating from a variety of social positions, interpreted the structures of musical utterances as referencing a world. Bulgaria in the 1980s experienced the death throes of communism. As the society divided itself along political and ethnic lines (the latter in the form of severe government-imposed sanctions against the Turkish and Gypsy Muslim minorities), musical practice participated in the contestation. Where the Communist Party had once dictated the public forms of musical production, during the 1980s those in opposition to the Party sought new musical forms to express their distaste for the Party's policies, practices, ideology, and aesthetics. Musicians played and listeners heard enormous variations in musical style, and often seemed to make aesthetic choices based on political preferences. It was in this political context that Todora said to me, as we listened to an outstanding *kaval* player on the radio, "May it fill your soul." There are a number of questions that her expression of enthusiasm raised for me. Why is the soul the locus of aesthetic pleasure? What in music filled her soul? Why did that performance fill her soul?

I can't yet answer the question of why the soul is the locus of aesthetic pleasure, except with the rather banal suggestion that it may have something to do with self-identity as Orthodox Christian Slavs (even when people like Kostadin and Todora are not particularly "religious"), but in musical conversation I began to get an inkling of the what and why of soul-filling performances. The precise pitch and presence or absence of the barely audible ornaments and "noises" between melody

notes in instrumental performances, which had presented me with so many technical and musical problems, turned out to be crucial to the identity of the player and the playing style. And questions of identity—whether one was Bulgarian, communist, anticommunist, Muslim, Gypsy, Turkish, Western-looking, or backward-looking—were what was at stake for most Bulgarians in the late 1980s. Kostadin's way of playing the ornaments was "Bulgarian" and even regionally distinctive, a marker of "authenticity." Muslim Gypsy musicians, on the other hand, used a slightly different style of ornamentation, and some younger *gaida* players tried to imitate it, primarily because it was flashy and fashionable, even when they didn't care to link it to its possible political references. I learned to play one such tune "in conversation" with Kostadin's nephew, who for the most part followed Kostadin's example, but when I played this tune for Kostadin he insisted that I replace an ornament his nephew had played below the melody with one above it. The way his nephew and I played the phrase was "empty," according to Kostadin. Though the quantity of ornaments didn't change, one version created for Kostadin an aesthetically empty response whereas the other one was capable of filling his soul. Fieldwork in Bulgaria had taught me that his preferred way of playing was filling because it referenced a familiar, comfortable world of previous experience, a world dominated by Bulgarians and the progress and security provided by the Communist Party. His nephew's way was empty aesthetically because it referenced a world of change, threat, and potential instability. Without that participatory experience of music making and living in Bulgaria, the explanation of what at first glance seemed a rather mystical link between aesthetics and metaphysics—"May it fill your soul."—would be impossible. That nearly imperceptible ornamental tone was not just a feature of musical style but a source of soul-filling (or empty) aesthetic experience and, through its capacity to reference a world, of social and political experience.

Throughout this chapter I have exposed a number of oppositions: studies of music "in its own terms" versus music as culture; explanations based on methods versus understandings based on experience; and insiders and outsiders. I have held out the hope for some sort of mediation, rather than a choice, between them. This study follows the temporality of my experiences with Bulgarian music and with ethnomusicology, and in so doing reveals that it is almost surely in the temporal dimension that the mediation between these oppositions will occur. Heidegger's insight that the fundamental ontological condition of "being-in-the-world" was its temporality is probably also the place to seek the kinds of mediations demanded by these oppositions. An initial understanding of musical style and production became the ground on which I in time built an understanding of music as culture. Instead of bracketing experience while focusing on experiment as our methods require—or glorifying experience while abandoning method as our dissatisfaction with positivism grows—Ricoeur's phenomenological hermeneutics suggests bringing experiment within the framework created by experience. Instead of ex-

planation for the natural sciences and experience for the human ones as Dilthey suggests, Ricoeur brings explanation within the framework of experience in his "hermeneutical arc." Instead of immutable outsiders, the "hermeneutical" arc may provide a pathway from the outside, with its cultural alienation, toward the inside by means of appropriation and understanding. Instead of generalized insiders, the "hermeneutical" arc may provide another path from the inside, with its cultural engrossment, toward the outside by means of distanciation and explanation.

In this chapter I illustrate two "hermeneutical" arcs that mediate between method and experience and between explanation and understanding by moving through time. The first began with my understanding of many elements of Bulgarian musical style as well as an understanding of the limits of that understanding as far as ornamentation was concerned. Further attempts to appropriate the tradition led to an explicit, verbalized explanation of how those ornaments must be produced and to corresponding problems of the appropriate methods to use in their description. The arc ended, "for the time being," when explanations led back to an understanding of how to play the instrument to produce sounds that were understood by me and interpreted by Bulgarians as adequate representations of Bulgarian musical style. The second arc began with this new understanding of musical style, moved through an explanation of the locus where musical style accounted for aesthetic satisfaction (the exact position of particular ornamental notes), and ended, again for the time being, with an understanding and interpretation of how and why this musical style references the politically charged Bulgarian world of the 1980s. These temporal arcs from understanding through explanation to new understandings contain the possibility for the mediation between field methods and field experience posited at the beginning of this chapter.

Phenomenology and the Ethnography of Popular Music

Ethnomusicology at the Juncture of Cultural Studies and Folklore

The categorization of expressive culture is an inherently vexed task, and doubly vexed are the lives of scholars whose methods and techniques of research are based on such categories. Despite the obvious problems that they entail and the criticisms that they have inspired, the art music/folk music/popular music trichotomy and the Western music/non-Western music dichotomy are still alive in the popular imagination and serve as the centers of gravity for music disciplines in the academy. Even as musicians, journalists, and scholars critique these categories, they continue to shape the hierarchies of prestige that attach to music styles and the people associated with them, the form and content of those musics, and the networks through which they travel. My musical life has been shaped by these categories as much as anyone else's. In this chapter, I will explore the complex ways in which these categories have affected ethnomusicology. In so doing, I will suggest how ideas from phenomenology and practice theory can mediate the tensions between ethnomusicology and the non-ethnographic strains of cultural studies and offer approaches to ethnomusicological ethnography that are applicable, not only to popular music but to music in general. In both the first and second editions of *Shadows in the Field,* contributors use their experiences as ethnographers and music makers to shed light on the issues of methodology that they raise, and in this chapter I continue this tradition.

When I enrolled as an undergraduate in Wesleyan University in 1984, I knew that I wanted to major in music. I had played guitar since my early teens, and though I had spent time in high school studying classical and jazz guitar, my heart was really in rock music. At the time, rock guitar performance was not an option offered at any liberal arts college that I knew of, and so I followed the jazz track in Wesleyan's undergraduate music program, with guitar as my instrument. The classes I took from the great tenor saxophonist and composer Bill Baron expanded my skills enormously and revealed to me the richness and complexity of be-bop

and post-bop jazz. This study fully absorbed my attentions, and from the perspective of the jazz theory I was learning, rock appeared to be simplistic and dull. Indeed, that is what my jazz guitar teacher told me—that rock was simple music for people without musical training or sophistication. I tried to convince myself of the correctness of this position. Searching for the formal structures of jazz in commercial radio rock did not put that music in its best light, and my guitar teacher quickly dismissed as childish the blending of popular traditions with Western Art music found in the work of progressive rock bands like Yes or King Crimson. What I couldn't set aside, though, was the affective power that rock music continued to hold for me.

The point was powerfully driven home for me one night when I overheard a conversation at a rehearsal in the Center for the Arts. A very advanced jazz guitar student was playing standards with my friend Pete, a tenor saxophonist with extraordinary jazz chops. Pete and I were younger than the guitarist, and as usual I was blown away by Pete's fluid command of the jazz vocabulary. As I was packing up my gear, I was surprised to hear the guitarist read Pete the riot act. "You can use all the Advanced Jazz Theory techniques you want," the guitar player said, "but I have heard all of that before. You should spend some time listening to The Ventures." The guitarist's point was not that formal complexity was bad or to dismiss the expressive power of jazz; he was an extremely skilled player, jazz was his first and only love, and I doubt that he had any deep affection for surf rock. Further, he was not espousing a musical primitivism—the idea that simple, emotionally direct music was more valuable than complex, "academic" music. Rather, his point was that the young tenor player should expand his listening and find new ways to make music compelling. This interaction gave credence to the affective relationship I had with rock, but questions still remained. How could one account for the emotional power of a music that seemed to be explained, and explained thoroughly, by traditional theory? What did that theory explain if it could neatly describe some elements of musical form, but didn't address its lived significance?

Wesleyan allowed its music majors a wide flexibility in designing their programs, but one requirement was an upper division course in either ethnomusicology or musicology. Beyond a course in South Indian vocal music that I had enjoyed greatly, I didn't have much experience with musics from outside the United States and Western Europe, but ethnomusicology still seemed closer to my interests than musicology. On the first day, the professor said that ethnomusicology was the study of any music whatsoever and that ethnomusicologists didn't make judgments about the quality of music. The value of rock was very much a sore point with me, and I snappishly asked, "Can you study rock music as an ethnomusicologist?" Calmly, the professor replied, "Not many people do, but there is no reason that you can't." I was instantly disarmed. The syllabus was like that of a graduate course, and across the span of the semester I devoured everything that the professor doled out—ethnomusicological classics at the juncture of musicology

and anthropology by Charles Seeger, Alan Merriam, John Blacking, Bruno Nettl, Marcia Herndon, and Norma McLeod, but also scholarship in linguistics, American studies, and folklore. In ethnomusicology I had found a way to be true to my emotional commitment to rock music and to explore the world of ideas. I applied to graduate schools the following year, and in 1988 I began to study ethnomusicology and folklore at Indiana University.

My Wesleyan training had crystallized my inchoate feelings about music and culture, and I entered graduate school with a small number of strongly held convictions, which were, and to a large degree still are, at the center of ethnomusicology: that all musics are equally worth studying; that music is inextricable from the rest of culture; that music doesn't have an inherent value, but is only valuable to particular people in particular societies; that it is not the job of the ethnomusicologist to engage in music criticism, but to understand how music works from the perspective of the people who make it and listen to it. That these guiding assumptions were largely applied to musics outside the Western world, and mostly to ones that were not mass mediated, at first simply seemed to be a historical accident—one that I hoped my career would help to correct. My ethnomusicology courses—primarily with Ruth Stone, but also with Portia Maultsby, Ronald Smith, and Dorothy Lee—helped expand a set of convictions into an understanding of theory and method, and nowhere was the connection of theory and method so tight as in the study of musical form. Not only were practices of composition and performance culturally specific, I was taught, but the very sound itself could be grasped in differing ways. While one could transcribe a recording and analyze the notes on the page, there was no guarantee that one was describing the "native perspective" of the music, and fieldwork was the methodology that could transform ethnocentric scholarly analysis of musical structure and meaning into richer, culturally informed interpretations. The late 1980s was a period of transition across the scholarly world, and the sea change in attitudes toward the politics of culture that was occurring had begun to make ethnomusicologists put scare quotes around "native perspective," a point to which I will return below. Nevertheless, as a student of a music that was openly sneered at in the academy, I found ethnomusicology's anti-elitist concern for the perspectives of music makers and music listeners to be enough to earn my emotional commitment. Ruth M. and Verlon L. Stone's classic 1981 article on feedback methods was a major text for me. Operationalizing theory in technique as well as in method, it suggested how the concern for local ideas about music making could be applied, not just to musical meanings but also to the questions of form and analysis that were more often the domain of musicology and music theory.

As my knowledge of ethnomusicology deepened, I explored the world of folklore studies as well. If ethnomusicologists examined the musical traditions that musicology neglected, then folklore embraced an even wider set of overlooked study objects—the material culture given short shrift by art historians, the liter-

ature that English scholars tended to pass over, the drama ignored by theater departments, and so forth. Indiana University was one of the few places where folklorists and ethnomusicologists had frequent contact, and ideas from performance theory in folklore became a fundamental part of my intellectual toolkit. Building on classic works by Del Hymes, Richard Bauman, Roger Abrahams, and Barbara Kirshenblatt-Gimblett, folklorists were conceptualizing performance as their study object and seeing texts as partial records of the on-the-ground reality of expressive culture. With scholars in ethnopoetics attending to subtle features of verbal art that were parallel to the musical nuances which enliven rock guitar playing, and with folklorists exploring the interactions among participants in folklore performances which were akin to those in music events, I felt like a disciplinary convergence was in the making around the work of performance theory. As a result, new items were added to my list of fundamental methodological commitments: don't reify texts; practices of production and reception are one's proper study object.

My professors and colleagues at Indiana were supportive of my interest in American popular music, particularly my primary mentor Ruth Stone and also Portia Maultsby, who was one of the pillars of the International Association for the Study of Popular Music. In my early days at Indiana, a folklore student had written a dissertation on rock music, and it seemed that the anti-elitist thrust of ethnomusicology and folklore studies would easily embrace the musics I cared about. In the larger disciplines, though, the populist mission of these fields collided with other basic assumptions there, producing serious problems and contradictions. Though the Indiana ethnomusicologists were more than happy to see the discipline embrace popular culture, many in the field were wedded to the notion that mass mediated music was in some way "inauthentic"—a strictly profit-oriented affair unworthy of attention, and, outside Western Europe and the United States, a corruption of the genuinely "other" cultures which were ethnomusicology's proper object of attention. And despite a century of broadening definitions of folklore, many in that field saw their study object as the rural, the oral, the traditional, or the pre-modern and were hostile to the study of anything smacking of popular culture.[1] That high art, popular culture, and folklore were historically emergent discursive constructions inextricably tied to power relations, rather than trans-historical categories of expressive culture, was not widely accepted in these fields, and the failure to appreciate this clashed with the broader populist themes of these disciplines. Lingering ideas from romanticism still plagued both folklore and ethnomusicology: folklore often looked to the survivals of the pre-modern world for a life-affirming communitas, while ethnomusicology frequently saw the music of authentic non-Western others as serving a similar function.

If the romanticism and exoticism in folklore and ethnomusicology complicated their anti-elitist missions, three intellectual movements that I encountered at this time deepened my thinking about the issues that these movements raised. First

was the new politics of culture scholarship in anthropology, to which I alluded above. Where I had come to see fieldwork and the search for native perspective as a vivifying corrective to elitist forms of humanities research, the contemporary work on field methods emphasized how ethnographers had exoticized their "informants" by exaggerating cultural differences, ignored the power relations in both the field and the field encounter, and actively participated in the administration of Europe's and America's past empires and their contemporary post-colonial hegemony. Indeed, this scholarship argued, inasmuch as ethnography was understood as a method for objectively revealing the perspective of the other, ethnographers were participating in the colonial project by representing themselves as having something like a meta-perspective (an ability to contain the perspectives of a range of cultural others) and understanding the lives of their research participants as nothing but the enactment of cultural scripts. Writing was now seen, not as a transparent media for communicating ideas, but as a historically situated practice whose tropes were implicated in larger power relations. Spurred on by these ideas, a number of scholars explored techniques such as dialogic ethnography, reflexive ethnography, autoethnography, and experimental writing in order to give research participants a greater voice in their representations, flush out older colonial rhetorics, and acknowledge the partiality and subjectivity of the fieldworker. James Clifford and George E. Marcus's important 1986 edited volume *Writing Culture* was the best-known statement of the new ethnography, but the ideas in it had a long history and a thriving contemporary practice, which was richly discussed in Ruth Behar and Deborah A. Gordon's 1996 *Women Writing Culture*.

I had looked to fieldwork to acknowledge the suppressed and disparaged cultural difference of American popular musicians, but the new ethnography warned about the tendency to reify difference and exoticize others. The new ethnographers didn't urge us to abandon fieldwork, but to be more reflexive and dialogic in our ethnographic practices. This came as a relief as I began to read the 1980s cultural studies scholarship on popular music. Work by the early generations of scholars at the Centre for Contemporary Cultural Studies at the University of Birmingham was richly ethnographic or employed historical methods that were analogous to ethnography (for example, Hoggart 1957, Thompson 1963, Willis 1977). However, the best-known 1980s cultural studies work on popular music (such as Hebdige 1979 and Frith 1981) didn't seem to connect to that ethnographic legacy, and as cultural studies took root in non-ethnographic humanities disciplines in the United States, this tendency increased. Political meanings were ascribed to music in ways that seemed to have nothing to do with local perspectives and everything to do with the scholar's political assumptions. A still developing literature, cultural studies was important in that it drew attention to the role of power relations in music and acknowledged the importance of popular culture as an object of study. However, much of this work seemed to have no concern for the experiences of the people who made and listened to the music and

treated them as ideological dupes enacting cultural scripts. The old ethnography may have had its problems, but this new anti-ethnographic cultural studies seemed elitist and dismissive. And while the populist thrust of ethnographic ethnomusicology and folklore was partially negated by its romanticism, its exclusion of popular culture, and the flaws of the old ethnography, it seemed at least plausible that the new, dialogic ethnography could partially redeem it. (Happily, much of today's cultural studies is richly ethnographic, but this kind of work was not dominant at the time.)[2]

This situation was further complicated for me when I became exposed to neo-Marxist practice theory (Giddens 1993[1976], 1979, 1984; Bourdieu 1977; de Certeau 1984[1974]. Roger Janelli's lectures on social theory in general and Marx in particular brought home to me in a direct and visceral manner the ways in which power can act as force to shape and regiment social life. Where a naïve adherent of the old ethnography might try to dismiss a concern with power relations as something that is an artifact of the scholar's perspective or might complain that functionalist sociologists ignored the agency of those who were dominated, the new theory could not be so easily brushed aside. Neo-Marxists saw society as constituted through practices; this jived well with the new folkloristic emphasis on performance, enactment, and doing. That all action was shaped by social context was a tenet of neo-Marxism as well, and this fitted nicely with the emphasis on music's ties to cultural context that was the bedrock of ethnomusicology. Already used to seeing "native perspective" as informed by culture, it was easier to accept that power relations might shape people's beliefs and practices and to take this as a basic research question. The heart of practice theory for me, though, was the Giddens's theory of structuration (1984). Here, society is seen as constituted by people's actions; however, those actions are not the effect of unconstrained free will, but take place in the context of past acts and a larger social world, which shapes and informs them. Structure is constituted by agency, agency is informed by structure, and the dialectic of the two is the mechanism by which social life is reproduced and transformed. No fast comments about native perspective or agency could distract from the fact that we live in a world of crushing inequities, and practice theory allowed one to think about the phenomena of power without reifying society or denying agency.

With the excesses of cultural studies as a negative example, it seemed that a combination of practice theory and dialogic ethnography might offer a way forward for the ethnomusicological ethnography of popular music. But something was still lacking. The theory of structuration didn't address the role of expressive culture in social life, and while dialogic ethnography offered a way of doing noncolonialist ethnography, it wasn't largely the work of scholars concerned with expressive culture. How did all of this apply to the rock music that inspired me so? What was needed was a deeper grounding that could tie the dynamics of structure and agency found in practice theory together with the questions of perception,

meaning, and experience that ethnomusicologists cared about. It was in phenomenology, an intellectual tradition in continental European philosophy, that I found this grounding. Ruth Stone's crucial 1982 book *Let the Inside Be Sweet* was the first in ethnomusicology to be based in phenomenology, and the major achievements in theory and method that it developed broke the path for the work in phenomenological ethnomusicology that came later (see, for example, Feld 1994[1984]; Titon 1988, 1997; Rice 1994, 1997; Friedson 1996; Berger 1997, 1999; Berger and Del Negro 2004; Porcello 1998; Wolf 2006). At Indiana, Stone introduced me to primary philosophical texts by Edmund Husserl (1962[1913], 1960[1931], Maurice Merleau-Ponty (1981[1945]), Alfred Schutz (1967[1932]), and Don Ihde (1976), as well as her own later work (Stone 1988). As Jeff Todd Titon has observed in his chapter in this book, phenomenology is a varied intellectual tradition, and my own approach to it draws heavily on Husserl and Schutz, as read through the lens of Merleau-Ponty.

The fit of phenomenology and practice theory was tight. Giddens quoted Merleau-Ponty in several works, and more recently C. Jason Throop and Keith M. Murphy (2002) have shown the dependency of Bourdieu's work on Husserl. The fit of phenomenology and ethnography—or, more precisely, the fit phenomenology with the most humanistic intentions of the ethnographic impulse—was even tighter. Despite its many difficulties, there is, I believe, a fundamental insight in the older ethnographic project as practiced in ethnomusicology. Though fieldwork can be conceptualized in a range of ways, many ethnographers in our discipline take as their task the goal of understanding the experiences of other people. However inchoate or undertheorized, the ethnographic concern with experience is, I believe, the key to ethnomusicology's bracing engagement with the on-the-ground world of people and their music and the saving grace of its anti-elitism. The emphasis on experience is also what makes phenomenology relevant to ethnomusicology and ethnography. Phenomenology offers a rigorous method for studying experience.

Husserl's famous notion of the epoché is the starting point here. Husserl argued that to address the basic issues of Western philosophy, one must set aside speculative theorizing and ground one's arguments on the only thing we have for certain—our experiences. This is extremely difficult, though, because our view of our experiences is clouded by a host of philosophical and lay presumptions, and the arena par excellence for considering the nature of experience is perception. In everyday talk, for example, we often use the word "experience" to mean something that is purely subjective and standing against the real world ("well, I experience that song as annoying, so it is annoying to me and no one can say otherwise") or the product of sense data mechanically registering on our minds. Husserl's great insight was that both of these ideas were abstractions from the lived reality of our experiences, and in his tradition of phenomenology work begins by bracketing out such assumptions and returning anew to those experiences themselves. When we do so, we discover that, strictly as experience, the world is not some subjective and

endlessly flexible fantasy, and neither does experience merely mean result from sense data colliding with our nerves. Strictly as experience, objects announce themselves to us as autonomous things there for exploration. For example, I experience this computer keyboard, not as a mental fantasy that can be anything I want it to be, but as a real physical other. At the same time, strictly examining my experience of the thing, I observe that I don't passively record the keyboard's color and shape by registering sense data; I actively explore it by focusing my eyes, pointing them here or there, touching the keyboard with my hands, turning it over, and so forth. What we have in sense perception is the subject confronting an object, and experience cannot be reduced to either side of that equation. Returning anew to experience, Husserl develops his most important notion—the intentionality of consciousness.[3] By describing consciousness as intentional, Husserl does not mean that we plot and plan all of our actions, but that consciousness never exists by itself. Consciousness, Husserl suggested, is always "consciousness *of* something" (1962[1913], 223), such as memories of the past, judgments, ideas, emotions, physical objects, the whole sweep of objects of human experience.

The significance of all of this for method—in philosophy, but also for ethnography in general and ethnomusicological ethnography in particular—is the notion of *structures* of experience. Experience isn't merely a mass of particularities. The relationships between its parts produce patterns and regularities that allow us to make concretely grounded but broadly applicable generalizations. In any given perceptual situation, for example, experience has a focal center, a fuzzy fringe of phenomena grasped with only partial clarity, and a loose horizon that points to a larger world to explore in the future. Further, experience is fundamentally temporal in character, although the fact that we approach phenomena, explore them, leave them, remember them, and forget them doesn't dissolve their permanence or mind-independent autonomy. As I have argued in a number of writings (1997, 1999; Berger and Del Negro 2004), the intentional structure of experience can be used to provide a direct link between phenomenology and practice theory. Perception is a kind of social practice in the practice theory sense of the word. All perception (and, to make the link to ethnomusicology clearer, let us speak specifically of music perception in the acts of playing and listening) is something that people actively achieve and is at the same time deeply informed by culture. Indeed, it is one of the places, the key place, where meaning in music is made.

Introducing ideas from practice theory into phenomenology approaches increases their relevance to issues of power in ethnography and basic problems in the politics of culture. Taking musical experience per se as our study object and treating it as actively and culturally constituted, we no longer find ourselves studying music as a reified cultural force whose connection to other phenomena (politics, culture, society) is abstract or undertheorized; rather, it becomes a domain of practice in the flow of peoples lives, a concretely grasped realm of activity that is shaped by other practices and has the potential to be influential upon them. These ideas

connect directly to the issues of native perspective raised earlier. What is good about the scholarly commitment to native perspective is that it draws our attention to experience (rather than decontextualized musical pieces or genres) and understands experience as basically social. The problem, however, is the implication that there is only a one local view of things, a view that is identical across "others," produced by culture rather than agents, different from "our" own, and ultimately fully discoverable by the all-knowing ethnographer. Phenomenological approaches to culture address these problems. In his work, Alfred Schutz argued that experiences can be partially shared across individuals and offered an elaborate scheme for analyzing the dynamics of partial sharing. Attending to both the possibility and the partiality of sharing in the manner of Schutz, we can see experience as cultural without homogenizing groups of others, denying cultural difference between groups, or so exaggerating difference as to make others incomprehensible. Further, phenomenology's emphasis on the agentive dimensions of practice allows a fieldworker to see research participants, not as merely enacting cultural scripts, but as actively constituting their experiences. And most important, understanding fieldwork itself as an attempt to partially share experience, the phenomenological ethnographer places her/himself on the same plane as the research participant, thus forwarding the dialogic agenda of the new ethnography.

Three Tenets for Phenomenological Ethnography

From these basic Husserlian and Schutzian insights, phenomenology could, I felt, provide a set of research questions and methodological presuppositions for an ethnomusicological ethnography of popular music that would satisfy the needs of the new ethnography and avoid the pitfalls of 1980s cultural studies. My research since the early 1990s has sought to pursue this program, and the following is a brief summary of its theoretical and methodological tenets.

First, when doing music analysis, the object of study is not a piece or performance; it is not music sound or music structure, but rather pieces, performances, sounds, or structures in the lived experience of social persons. Music perception is not, of course, completely up for grabs; for example, one can't hear the melody of "The Star Spangled Banner" in Iron Maiden's song "Hallowed Be Thy Name" without passing from perception to imagination. However, any given collection of sounds can be grasped in more than one way, and one job of ethnomusicological ethnography is to understand how music sound emerges in the experience of those who make it and listen to it. The issue is particularly important for the ethnography of popular music. For example, extreme metal (the genres of heavy metal music that I have spent much of my career studying) is not unrelated to Western conservatory music, but neither can it be analyzed strictly in terms of the theoretical ideas found in the conservatory. Indeed, the traditions of music analysis in Western musicology and theory are so varied and complex that it isn't

immediately clear which strain of analysis one should apply. Any social world in which people engage intensely with a group of compositions or performance will by necessity develop its own practices of constituting music sound in experience; such practices are always varied, but they are often complementary or partially shared across the individuals and social roles within that world. Using interviews, feedback interviews, observation at rehearsal or performances, and participant observation, ethnographers can get a sense of the differing musical forms that are experienced by people taking part in these social worlds. Here, the phenomeno-logical ethnography of popular music is not unlike that of older forms of ethno-musicology, but with several key differences. Treating partially shared experience as its study object, phenomenological ethnography attends to both the commo-nalties and the differences in the participants' perception of the music. It does not see any given popular music culture as a corruption of more authentic conser-vatory or folk traditions, but merely as a social world that has complex relation-ships to other social worlds—that is, as a group of people and their practices whose historically emergent boundaries are there to be discovered.

A second object of research for phenomenological ethnography is the par-ticipant's organization of attention to the music and the situation as a whole. Here, the ethnographer asks a wide range of questions. Which elements of the music sound are the foci of the participant's attention? Which ones are located on the fringes of attention, which ones trail off into the horizon of experience, and which ones are actively ignored? How does the organization of attention differ among varying participants in the interaction? What place does music sound have among the varying elements of the event, such as the visual appearance and behavior of the other participants, memories, fantasies, or the participant's experience of his/her own body? How is the experience of the event organized across its temporal length? How are elements of music sound or other parts of the event retained in experience once they have moved into the recent past, and what structures of expectation are at play? Against conservatory traditions, which sometimes place a high premium on exclusive attention to music sound, popular music traditions may emphasize stage behavior, audience participation, or the complex embedding of mediated sound in everyday contexts. In examining these issues, phenomenological ethno-graphy attends to elements of experience that are often ignored by other forms of scholarship and can thus get a richer sense of the ways in which music plays out in people's lives. Again, the techniques of interview, feedback interview, observation, and participant observation help ethnographers partially share the diverse social experiences of the people with whom they work.

A third area of inquiry builds directly on the first two. People engage with sound, make it take shape and form in their experience, and juggle it relative to other elements of the performance event, but they do not do so in a value-neutral fashion. Experiences of music and music events are embedded with affect, style, value—meaning in the broadest sense of the word. Like other fieldworkers,

phenomenological ethnographers seek to partially share the meanings that their research participants find in social life. Like the other elements of experience discussed above, a phenomenological approach to meaning sees it as actively and socially constituted and differential, though partially shared, across the various participants in a social world. Further, as Timothy Rice has emphasized in his chapter in this volume, phenomenological approaches to music inquiry see the constitution of meaning in music as an open-ended process, something that people discover as they perform, listen to, and reflect on music over time, rather than a pre-existing essence that is created once and for all. The exploratory nature of meaning in music makes dialogic and reflexive techniques particularly valuable for fieldworkers. In interview settings, ethnographers can explore music along with their research participants, discovering the meanings that they find in the music and also watching the style of their ongoing interpretive processes.

Music, Politics, and Dialectics

The dialogic nature of interpretation returns us to the issue of the politics in music and the dialectics of intellectual traditions with which this chapter began. The classic texts of ethnomusicology emphasized that music is shaped by and tied to the rest of culture. Translating this idea into the language of practice theory, we could say that the meanings that people find in music do not depend solely on their musical practices, but also on their experiences from other domains of practice as well—their work lives and home lives, their sexual lives and social lives, their experiences in families, schools, places of worship, sites of public debate, education, healing and dying, the whole sweep of their existence. To do phenomenological ethnography is to understand how musical meanings are shaped by and have the potential to influence practices and experience from other domains. More important, people are aware—sometimes with great clarity and a keen intensity, and sometimes inaccurately or dimly—of the differing kinds of practices and experiences that are available to the people around them, as well as the kinds of opportunities that they might have in the future. However accurate or inaccurate, this broad sense of a social world is the context in which people forge their musical meanings, and those meanings are the ones that might affect their practices in other domains of their lives. Indeed, the interpretation of the social world is an ongoing activity, and the new ethnography sees the reading of that world as something that the fieldworker and the research participant do together. The implications of this, however, are far more complex than they might initially seem.

It may appear that because experience is the contents of consciousness, a person's descriptions of his/her own experiences must be the final word on the topic. There is, however, a wide range of ways in which this may not be the case. On a basic level, any item in our experience will be distinct from our description of it, and the richness and subtlety of phenomena make it possible for people to misdescribe their

own experiences. If this is true of individual entities in experience like a chord progression or a melody, then the broad sets of experiences that constitute everyday life would be even more difficult to describe. One's interpretation of one's social world—one's place among others, the privileges, rights, and opportunities offered to oneself and others—is even more complex. Further, the interpretation of experience is itself a kind of social practice, and to see it this way is to acknowledge the agency that people have in understanding their experiences, and, at the same time, to accept that their interpretative practices are shaped by their social context. Received discourses about race, class, gender, sexual orientation—the whole panoply of identities—inform the ways in which we understand our own lives and the world around us, and these discourses are not uninfluenced by the facts of domination and power. The humanistic desire to "give voice" to our research participants has made folklorists and ethnomusicologists uncomfortable with the Marxist notions of ideology and false consciousness. Indeed, the worst excesses of 1980s, non-ethnographic cultural studies privileged the scholar's interpretation of music and often dehumanized music makers and listeners by ignoring their perspectives. However, assuming that people have a perfect understanding of their own experiences denies that interpretation is a kind of practice and decontextualizes it. To act as though people cannot misunderstand their own experiences is ultimately to dehumanize them by deifying them, to deny their capacity to grapple with the complex realities of social life. As I have argued elsewhere (1999), neither the scholar nor the research participant is an infallible observer of social life, and the richness of experience requires dialogic methods in ethnography.

On a basic level, ethnomusicological ethnographers must start off by trying to understand the experiences of their research participants on the participants' own terms, to share as richly as possible their experiences of music and social life, and to understand their interpretations of that life. In narrowly focused research that seeks only to understand particular technical details of musical form or musical performance, it may be possible to stop here. However, where we wish to explore in its fullness the meanings that music has for a group of people and its role in their lives, we need to go further. Without denying that people have agency, we must also acknowledge that people's lives are fundamentally shaped by social and cultural contexts, that an unavoidable element of those contexts is power, and that power adds deep complexity to the process of interpretation. Attention to these realities forces us to recognize the twin perils of dehumanization and deification in ethnographic representation. The techniques of research and writing necessary for avoiding these perils are as varied and context specific as the issues at play in the field situation. In some settings, the ethnographer may simply raise questions that research participants have never asked. Another technique is to juxtapose against one another the interpretations of a single music by people from a range of differing perspectives, as Jocelyne Guilbault did in her well-known study of zouk music (1993).

The dialogic approach I have developed in my own work is one of critical phenomenology (1999). Much of my Ohio research has been on what is now referred to as "extreme metal," a set of genres of heavy metal music characterized by distorted guitar timbres, raspy, non-pitched vocals, and complex song forms. Many of metal's adherents are working-class youth from deindustrialized urban areas, and in the 1980s critics on the political left had compared metal unfavorably with punk music, which often was self-consciously political. Drawing on themes of horror and the supernatural and with rage as its most obvious affective tenor, metal has been read by some cultural studies scholars as a safety valve for venting class tensions. In the early stages of my Ohio research, I focused on traditional ethnographic techniques, seeking only to understand the metalhead's experience through interview and participant observation. When I asked about the meanings that they found in metal, the musicians pointed to a range of emotions—rage, certainly, but also sadness, depression, unbridled energy, and a kind of grandeur. The metalheads would have stopped there had I not pressed further, but I asked a variety of questions about the tenor of their everyday lives; in response, the musicians described their experiences of limited opportunities, poorly paid service-sector jobs, and unemployment in a city whose economy had been crippled as long as they could remember. When asked about the role of music in all of this, they spoke about metal encouraging critical thinking, personal responsibility, and an energetic, proactive spirit in overcoming obstacles.

After hours of interview and participant observation, I felt as though I had a sufficient handle on their perspectives to engage in critical dialogue. In conversations with my main research participant, Dann Saladin, and others, I explained the cultural studies interpretation of metal as a ventilator of social tensions—a cultural movement that distracts working-class youth from the social inequities that are the true causes of their problems, discharges their rage in politically unproductive ways, and keeps them from progressive action. Respecting my research participants enough to engage them in a critical dialogue paid off, and the metalheads' responses offered a realm of ethnographic insights. They made it clear that they were aware of the difficulties that they faced in deindustrialized America, and they depicted their music as a form of emotional exploration and a way of confronting the stultifications of daily life in a hostile world. A good show didn't ventilate tensions and leave the person in a serene, apathetic state, they said; it removed the emotional stumbling blocks placed there by a hostile world. I would not have discovered this interpretation without engaging in critical dialogue. Pressing further, I explained the position of Marxist critics, who argued that themes of rugged individualism and personal responsibility misinterpreted personal apathy as the root of problems that were really social and structural. The metalheads considered these ideas, and our give and take on the Marxist critique of profit, American race relations, and the politics of music and emotion yielded a range of insights that would not have been possible without critical dialogue.

While such techniques are not always feasible, they can deepen ethnography, satisfying the need of contemporary ethnography for attention to the ethics of voice in fieldwork, and also the demands of a critical cultural studies for a sensitivity to the role of power in expressive culture.

While the prominence of cultural studies has led ethnomusicologists to pay more attention to political concerns, the issues raised here are not only applicable to popular music research. They apply to any situation in which music is informed by power. In a recent essay, Deborah Wong (2006) articulated the range of ways that ethnomusicologists engaged with the field of cultural studies in the 1990s, and this phenomenological approach is only one form of interaction between these fields. Likewise, the technique of critical dialogue is only one way of deepening ethnography. Methodology is theory made concrete, and the fundamental issues of fieldwork here stem from our basic assumptions as scholars about the nature of social life and research. The perspective outlined in this chapter sees practice and experience as the study object of ethnomusicology. In this view, experience is the concretely grasped world—the lived contents of consciousness—but it also has a richness and thickness that demands of researchers a deeper engagement, a sustained inquiry that respects both the potential for insight and the possibility of misunderstandings by fieldworkers and research participants alike. The techniques of ethnomusicological ethnography may vary, but it is only by navigating this complex terrain of practice, experience, and power that we can respect the things we care about most—people, their music, and the meanings they find there.

Notes

1. For an overview of the history of romanticism in folklore studies and its place in the contemporary discipline, see Del Negro (2004).

2. In the late 1980s and early 1990s, scholars from a range of disciplines began to use ethnographic methods in popular music research, and this culminated in a number of important works, including those by Ruth Finnegan (1989), Sara Cohen (1991), and Robert Walser (1993).

3. The idea of the intentionality of consciousness was first introduced by the German philosopher Franz Brentano, but Husserl is widely acknowledged as providing its most important development and recognizing it profound significance for philosophy.

Moving

From Performance to Performative Ethnography and Back Again

Watching/Playing

I'm watching San Jose Taiko play "Gendai ni Ikiru" ("Living in the Present") (San Jose Taiko 1998).[1] I've seen them perform it live more than once, and I have two video versions of it. This isn't my favorite version, but I still get a rush when I watch them make the move that's part of its central motive. I know what it would feel like to play that jazzy phrase, and actually I'm dying to do it. The best part of the phrase is when the right hand strikes the drumhead and the left arm is simultaneously flung out over the top of the taiko, like an arrow. That's how you play it. But actually, I've never played it, and probably never will: it's one of San Jose's signature pieces, and I'm not a member of San Jose Taiko so I simply wouldn't play it. Yet I know the piece, having watched them play it so many times, and I can feel my left arm zinging out while I watch, and I immediately feel exuberance and joy when I do it, because that's the vibe San Jose Taiko has—it's their hallmark. They have this fantastic energy, and it's different from any other taiko group. Their technique and their level of skill is very high—enviably so—but you can also see the community base in their playing. I think you can see their dedication to the Japanese American community in their playing. Or maybe I just think I see it because I *know* it's there: I know several of their members and I've seen how they interact with other taiko players. They have a very inclusive and non-egocentric approach to taiko, and this is manifested in their spirit and *ki,* energy. It shows, and the more you know about it, the more it shows. They're probably my favorite taiko group (aside from the one I'm in, of course) and I would actually give anything to be in San Jose Taiko.

In this essay, I address the relationship between ethnomusicology and ethnography and lay out the central argument behind my nearly finished book on taiko in North America. The problems with ethnography have centrally occupied

anthropologists for almost thirty years, so at a certain level one has to ask, why can't we just get on with it? Surely we have learned from the disciplinary upheaval of the 1970s–90s. Haven't we changed our research methodologies and writing techniques? Can't we move on to more pressing things at this point? The problems with ethnography aren't new and haven't changed: they include the false binary of the insider/outsider, colonial baggage, and the empiricism still lurking behind a solidly humanistic anthropology and ethnomusicology. But ethnomusicology still struggles with its own relevance to anthropology because it hasn't sufficiently theorized the relationship between participatory research and the specific kind of ethnography that we do, which is very similar to anthropology but in fact not quite the same. Ethnomusicologists still need (1) to make sure that we are consistently engaged in the practice of critical ethnography and (2) to focus explicitly on creating performative ethnographies while acknowledging the place of auto-ethnography in our methodologies.

I opened by writing as a taiko player because that's what I am. Everything I said is true, and the *way* I said it was really how I think about San Jose Taiko and that piece. I wanted you to read my pleasure in watching *this* performance by *this* group, and I wanted to convey how playing taiko and watching taiko are intensely inter-related for taiko players. Or rather, I'm asking you to trust me as a representative taiko player, though that's theoretically risky in all kinds of ways. As I just explained that, I was moving back into another way of writing about taiko—one less spoken, based less in experience and more in interpretation—and another way of thinking about performance. And now I've stopped writing like a taiko player, and I've more or less shifted into thinking and writing like an ethnomusicologist. I don't talk with taiko players the way I'm going to write here, and I have struggled with what this might mean for the book I'm writing. I feel taiko players reading this as I write. The question is whether there are effective ways to speak to all such readers at once and still satisfy myself as both an ethnomusicologist and as a taiko player.[2]

Since 1997, I have been doing immersive research on taiko, focused on the play of identification that runs through North American taiko practice: the ways that ideas about race, ethnicity, gender, sexuality, age, and class are discussed, explored, and sometimes hardened in the course of playing these drums.[3] I am not attempting to write a broad, all-encompassing history or survey of these activities. Heidi Varian's *The Way of Taiko* (2005) offers this, and I imagine it will be bought by many, many taiko players who will learn things from it in some ways and disagree with its generalized statements in others. I am writing from what I know— from my own ethnographic experiences as a taiko player and as an ethnomusicologist in Los Angeles, in Chicago, in Raleigh, North Carolina—from learning taiko and spending a lot of time with other taiko players. I try to do justice to an unruly group of loosely connected people who love what they're doing and agree on some things but not on a lot of other things. The resulting narrative will be neither tidy nor uniform but it will be vivid and richly textured, and it will engage

with urgent issues that shape American culture generally: difference, globalized movement, agency, and self-determination.

Ethnomusicologists offer a particular take on ethnography that redirects postmodern and poststructural critical methods. I call this performative ethnography. My purpose is twofold, as it is for any performative ethnography: I want to try to convey the vibrancy and the critical effects of taiko in all its particularities, and to reflect on my own process of telling, testimony, and cultural critique. These two things are inextricably linked in performative ethnography. I can't tell you about taiko in Southern California without telling you about how and why I'm telling you about it, and I can't reflect on ethnography without doing it.

I am far from alone in this effort. The rich body of literature on performativity theory is my bedrock (Wong 2004:3–17) and I am particularly indebted to the work of ethnomusicologist Michelle Kisliuk and sociologist Norman Denzin. Denzin's book *Performance Ethnography* (2003) presents a model for how performance and political engagement can converge in the ethnographic effort; he calls this "performance ethnography." Denzin conceived of his book as a manifesto for how ethnography must be wielded as progressive cultural work. He writes:

> It remains, then, to return to the beginning, to take up again the task of offering a critical framework for reading performance ethnography's place in a progressive discourse that advances a pedagogy of freedom and hope in this new century. It is not enough just to do ethnography or qualitative inquiry. Of course we seek to understand the world, but we demand a performative politics that leads the way to radical social change. (Denzin 2003:225)

Like Denzin, I work out from a theoretical base in critical pedagogy (drawn from Freire 1970, Giroux 1983 and 1992, and McLaren 1997 and 2006) that presupposes a world of politicized praxis. Like ethnomusicologists and performance studies scholars, I study performance because it has effects on the world. Writing about performance thus needs to do more than simply describe the thing in front of you. Drawing on the work of others, I call for a performative ethnography that could have any of the following characteristics:

- It enacts the ways that performance itself is a social change agent: as a genre of representation, it attempts the act of transformative becoming.
- It assumes that performance is imbricated with, and constitutive of, cultural ideologies and political economies.
- It focuses on the ways that performance (narrowly or broadly conceived) is practiced and the ways it has effects, and it parses how these two valences are interconstitutive. It moves easily but stringently between the micro and the macro. It presupposes that performance is culture-making.
- It attends to the subjectivities engaged and probably transformed through performance. It moves between the subjectivities of the audience, the performers, the ethnographer, and others.

- It is not "the same as" performance. (If it were, there would be no need for anything besides performance itself.) But it evokes the choreographies and modalities of performance in order to break down the subject/object binary and to deliberately draw on the generative power of performance.
- It is informed by performance studies and all its influences, including feminist theory, postcolonial theory, and more.
- It shows rather than tells. It is specific and particular.
- It is reflexive and aware of its own medium, that is, it is an offshoot of post-1970s anthropological approaches to ethnography. When written, it is deeply informed by the praxis and poetics of writing. When filmed, it is informed by the lessons of a critical visual culture. It always pushes at the genre conventions implicit to its medium: the parameters and limits of "ethnography," "documentary," and so on, become an attendant question in the endeavor itself.

As I proceed, I will demonstrate performative ethnography and, simultaneously, will listen to the issues that it raises. Performativity sets into motion a series of spiraling, discursive responses, and ethnography should, too.

I am an ethnographer of taiko, but I am a taiko player who was an ethnographer first. The relationship between my two identifications is sometimes uneasy because I didn't go to taiko in search of a research project: as I have written elsewhere, I was drawn to it as an Asian American audience member, and I simply wanted to learn how to be Asian American through the loudness and physicality of taiko (Wong 2004). My path took me from performance to ethnography to autoethnography, but my deepest goal is a more powerful ethnographic practice for ethnomusicologists. In this research, performance came first: in the early 1990s, during a period when I was deeply involved in research on Asian American performance, I saw taiko performed live by two American groups (Soh Daiko from New York City and San Francisco Taiko Dojo) and I had a powerful, unequivocally visceral response to it. I responded as an Asian American watching other Asian Americans, and I am hard put to convey how commanding an experience it was; its power lay not in the form itself but in my reception of it as an Asian American. Certainly taiko is loud and visually exciting, but feeling compelled to learn it was driven by my political subjectivity. Studying performance that is rooted in the politics of race and ethnicity means getting familiar with the lightning-flash moments when critical and emotional response merge, but this one caught me unprepared. I had been moved many times by Asian American music-making, but watching taiko was one long extended moment of wanting to *do* that, wanting to *be* that—a deep urgent desire that I can now only describe as a truly performative effect. Watching taiko made me want to do it, and all that that means. Watching taiko made me want to play it, or maybe it's most accurate to say that it made me want to be a taiko player, because that confluence of grace, strength, discipline, confidence, noise, and visibility planted a longing in me. If you are not Asian American and perhaps have had a similar response to seeing taiko performed, your

response doesn't contradict or negate mine. It's not impossible that your subjectivity and mine have points of overlap, but our responses are not, and can't be, equivalent.

Deciding to do research on taiko came later. I emphasize this not to suggest that my intentions were somehow purer than they might have been if I had approached taiko as a scholar. The mere act of participating in performance will not necessarily achieve, cause, or produce anything in particular. Musicians and audiences who are invested in elite western forms of performance are very susceptible to certain ideological problems, including the following:

- the assumption that performing evinces automatic and mysterious access to the subjectivity of a composer or to anyone who performs that music;
- the belief that the ability to perform is encoded in (your choice) a genetic blueprint or the soul, and that it is activated by (choose one) the divine or blood heritage;
- the understanding that performing is categorically different from everyday life.

The genesis of my model was specific to my own experience and research purpose, but my aim is to tamper with the ethnographic practices of ethnomusicologists. Our theoretical indebtedness to anthropology is undeniable and ongoing, but the only ethnomusicologists who are read and cited by anthropologists with any frequency are a select few who publish consistently in anthropological venues, particularly Steven Feld and Marina Roseman (examples include Feld 1987, 2000, 2002, 2004; Roseman 1990, 1996, 1998). Nor is it self-evident that anthropologists *should* read the work of ethnomusicologists (though the reverse is definitely true). Learning how to make music is an extension of participant-observation research, but it offers specific possibilities distinct from the deep hanging out that we share with anthropologists. Unfortunately, ethnomusicologists sometimes valorize participant music-making in ways that reenact the problems listed earlier.[4] Learning music through participation was one of the foundational concepts behind contemporary American ethnomusicology as advocated by Mantle Hood, who argued for the importance of bimusicality (Hood 1960). Hood was most focused on describing the difficulties and challenges of learning music in the myriad ways different from western art music (e.g., through memorization and imitation). He said less about what precisely is gained from immersion in praxis. He argued that praxis is "essential" (55) but left it to the reader to infer that some deeper measure of understanding is the difference from observational or passive learning. Hood's emphasis on research through performance practice has had a profound effect on ethnomusicology—not least because he built it into the curriculum of UCLA's graduate program and thus influenced several generations of scholars—and yet his approach to it was remarkably untheorized.[5] Marc Perlman has effectively (and diplomatically) explored how Hood's approach impacted research on Javanese music theory. Hood focused on theories of mode and structure

in central Javanese gamelan music, and although he studied performance practice extensively, his scholarship on gamelan music theory was profoundly shaped by his time and his own training. He spent years interacting with Javanese musicians and learning from them directly, but they are essentially not present in his analytical work; his scholarship on music theory is empirical, produced by a unitary interpretive subject (Hood) and is barely ethnographic. Perlman provides a virtuosic and deeply ethnographic examination of how Javanese music theory was conceived and produced across a broad nexus of interaction (direct and indirect) between Javanese and western thinkers; he shows how certain key musical concepts proposed by Hood and his teacher Jaap Kunst allowed particular Javanese musicians (e.g., Sumarsam) to think about their own music in a different way and then, ultimately, to reject Hood's and Kunst's paradigm in the course of proposing their own (Perlman 2004:124, 125–26, 132).

As post-1980s ethnographers, ethnomusicologists know that experience is important, but they also intuit that the epistemological problem of reflecting on music in another medium *besides* music involves multiple translative shifts. Quite a few ethnomusicologists have offered painstaking and vivid descriptions of the experience of learning another music because of the way its micro-practices open up macro-level matters musical and beyond (Bakan 1999, Brinner 1995, Chernoff 1979, Hagedorn 2001, Rice 1994, 1995). John Baily argues for the "direct" nature of music study as a methodology and its value in learning about music through musicking. He also offers, almost in passing, the observation that learning to play an instrument moves the researcher into the "ergonomics" of the tradition, though he doesn't dwell on the specific significance of bodily experience and bodily knowledge (2001:93–94).

Stephen Slawek has described Hood's emphasis on performance praxis as conceptually limited, noting that "Mantle Hood advocated studying performance practice as, among other reasons, a means of intuitively constructing a music theory for traditions in which an articulated theoretical tradition did not exist" (1994:15–16). Slawek uses Hood as a jumping-off point for a deeply situated consideration of how performance-based research on Indian art music, with its formal student-teacher relationships, shapes the ethnographic project in profound ways, often unacknowledged. Slawek writes that it was slow and difficult for him to reach a reflexive understanding of this: he explains that he became an ethnomusicologist after serious immersion in Indian music study, and that his movement between performing and "doing research" was marked by a pronounced critical tension.[6] Slawek also argues that ethnomusicologists' performances are an unrecognized mode of intellectual effort. My effort in the following pages will attempt to pick up where he leaves off, but I would like to quote him extensively because his proposal has, as far as I know, not received much attention but represents a moment in the mid-1990s when ethnomusicologists were beginning to respond to the paradigm shift that took place in anthropology in the 1980s. Slawek acknowledged the move

toward experimental writing, related it to the foregrounding of performance in the *guru-sisya* relationship, and took the next step (1994:22):

> I am of the opinion that in the field of ethnomusicology, musical performance by trained scholars should be valued as a medium for translating a research experience into a statement of that research. Especially in the improvisational music of North India, such a performance has vast potential for poetic affect and critical interpretation. I submit that competent performance of Hindustānī music by a Western researcher amounts to an experimental translation of a cultural experience that may potentially equal a written statement in depth of intellectual engagement and most probably will surpass a written statement in the intensity of its emotive affect. If anthropology's future contribution is to be a more artfully conceived ethnography, I would contend that ethnomusicologists engaging in cultural studies through performance practice have, in a sense, been ahead of the anthropologists without getting credit for it.

This radical expansion of possible media through which ethnographic reflection might take place offers much, but of course the devil is in the details and ethnomusicologists haven't gotten "credit" for musicking about ethnomusicology because many of us don't do it, haven't thought about it that way, and haven't theorized how, exactly, we could operationalize alternative media. Ethnomusicologists are often reluctant to move away from experience, but we have also failed to zero in on the nexus where experience and interpretation overlap—on the critical interface where at least two modalities are engaged at once and are no longer a contrasting binary, so that a dual awareness becomes habitual. We have addressed the hermeneutics of knowing and interpretation (Titon 1997, Rice 1997), and we remain enthusiastic about musical experience, but the discipline remains oddly schizophrenic, weighted down by empirical baggage and a little naïve about the workings of subjectivity. Michelle Kisliuk has argued that if the construct of ethnomusicology is "pursued [. . .] to its empirical conclusion"—that there is no Other, that music is and isn't the thing studied (culture is), and that our discipline is no science—then the necessity of redefining its name as well as its purpose is unavoidably revealed (1998a:314). She calls for an ethnomusicology that exemplifies "a fully transdisciplinary, transgeneric, interactive, embodied scholarship" (1998a:314), which is where I now turn.

Working from Within

The ethnographer is always an outsider. Creating an ethnography of even a close family member would presumably entail crafting a new relationship beyond that of daughter or sister. When I devoted the final chapter of my last book to my father's engagement with music (2004), I wrote as a daughter/ethnographer. That chapter attends to the richness of family memory and thus can be read as a memoir, but I also focused on the complex links between people, music, race, intent, and cul-

tural work. I wrote for an academic reader though I could feel my relatives reading over my shoulder. Anthropologist Donna Y. Young reflects on the implied audience as follows:

> Even as natives, we go where we don't belong, we transgress borders. And that is the funny thing about "native anthropology." Even when we work at home, we tend to respond and to write as if we were outsiders. That is, we continue to translate the ways of one group of people for another group of people. We don't assume a native audience, we assume an academic audience. (2005:208)

I think Young is right in arguing that the assumed readership is as much a problem as the roles occupied by ethnographers during research, and it has preoccupied me while writing my book. But I have trouble imagining a form of ethnography that isn't ultimately for anthropologists or ethnomusicologists. As a genre, it should have an intended audience, which would not preclude other readerships. The point is not to do away with ethnomusicology, anthropology, or ethnography but rather to reformulate their circuits of use, because I remain sure that the ethnographic effort is useful.

The literature on native anthropology and autoethnography opens up the key issue: how proximity is imagined and enacted, and how the epistemological problem of knowing is put forward. Entering fully into a postcolonial and transnational world has meant that insiders are both anyone and everyone, and the field is everywhere and nowhere. These are not facile blurrings and I do not carelessly evoke a global circuit too often assumed to offer full and open access; still, the inevitability of multiple subjectivities on the part of both ethnographer and interlocutor is now usually understood, and the task of representing the overlap is thus difficult and necessary.

The complexity and ambiguity of autoethnography as a postmodern genre generate the right kinds of questions and suggest possible methodologies for the kind of ethnography I advocate. As anthropologist Deborah E. Reed-Danahy suggests, autoethnography can mean a number of things, from "the ethnography of one's own group . . . to autobiographical writing [that] has ethnographic interest" (1997:2). *Who* is doing autoethnography is wide open, and it may or may not be the anthropologist. Further, it signals connections between several kinds of writing, including native anthropology, minoritarian narrative, and anthropologists' reflexive reflection (1997:2). A non-unitary notion of self connects these genres: moving from self to subjectivity creates discursive environments in which authenticity must give way to position and identification. Neither the ethnographer nor her interlocutors are fixed or immovable. Writing in an awareness of such mobility creates vital performative possibility. As anthropologists Jeannette Marie Mageo and Bruce Knauft write, "Feminist discourse on emotion shows how internalized self-constructions can be played back against themselves with surprising repercussions for social and epistemic power" (2002:194), that is, the very

construction of subjectivity is the stuff of political action. I focus on taiko players who are mostly Asian American, and an amplified sense of multiple selves is part of what I must try to write into their sensibilities and mine.

Massed and Massive Experience: The Taiko Jam

I have long been fascinated by sound events featuring loud, uncoordinated musics that pile up and create presence, power, or authority.[7] The corporeal and sonic importance of the taiko jam offers productive models for thinking about sound, ethnography, subjectivity, and performativity. The collapse of subjectivity (mine/ theirs/yours) through musical experience is one of the key nodes of ethnographic action, and is the site where ethnomusicologists come to understand epistemo- logical change. It is also where ethnomusicologists confront the problem of au- thenticity: though we may participate, our experience is never equivalent to the authentic thing studied or heard. The problem of inauthentic experience haunts our writing. I turn to the taiko jam—a particular performance practice—because it offers an alternative discursive practice with a very different set of driving as- sumptions. The Japanese word *kumi* means "mass," "group," or "ensemble," and *kumi-daiko* thus means an ensemble of taiko or a massed grouping of taiko. North America taiko creates a specific kind of Asian American body politic. Taiko players use collective bodily experience to explore how subjectivities are activated and accessed when bodies come together in massed formations. The body remains one of the key sites for racialized discipline: the ways that race is constructed in the United States as visibly identifiable have profound implications for Asian Amer- ican bodily experience. The mass is uneasily theorized. Mass culture and the masses are unlocatable. Experience in the midst of a minoritarian mass is something else again, and the possibility of massive experience suggests organization and social choreography. Minority groups in mass formation produce disquiet, because they are usually up to something, for example, a demonstration, a riot. They occupy space in ways that verge on threat because they are not contained in the ways that ghettos afford; the mass can be broken up and sent back into a state of scattering and dispersal.

I could offer a generalized description of taiko jams, because I have observed and participated in many between Southern California and Japan. If I were to do so, it would go like this. The word "jam" is taken from jazz and means an im- provisation session, which is true for taiko contexts as well. Most ensembles, whether Japanese or North American, rehearse and perform precomposed pieces that are tightly arranged and choreographed. The "taiko jam," however, is a "tradition" that is about thirty-five years old in North America, that is, dating to the emergence of *kumi-daiko* in the United States. Its performance practice has no set rules. A taiko jam may be completely open or it may be led, but it is usually open to anyone and more or less features improvisation. Sometimes one player, or

several, will step forward (formally or informally) to lead the jam. Sometimes it starts with one musician laying out the *ji,* the *ostinato* timeline that continues throughout; since there are only about four *ji* that are widely used, whichever one chosen is usually instantly recognizable, and participants start to come in almost immediately. Individual performers usually alternate between improvising and simply keeping the *ji,* that is, will improvise for awhile and will then play the simple *ostinato* pattern to rest and to take in what others are doing before going back to improvising. The more daring or playful or experienced performers may try to improvise interactively with whoever is nearby, sometimes even playing on the same drum or trying to add dance movements.

But no, I'm going to *show* rather than *tell* by staying with specific events. This is central to the methodology of performative ethnography: you circle around particularities and skirt the conceit of the typical, the normative, the generalized, the characteristic, the archetypal. This jam was held on Sunday morning, July 17, 2005, in Los Angeles, outside on the plaza in front of the Japanese American Community Cultural Center and the Japan America Theater. The plaza is in some ways the town square for Little Tokyo, the historically Japanese American community in downtown Los Angeles, and free outdoor events are often held there. About seventy-five conference attendees participated. Two musicians were on a stage keeping the *ji* on *shime,* so this jam was a little more structured than some. The range in age and ethnicity was broad, with lots of players in their twenties and thirties, some older; an approximately equal number of men and women; lots of Japanese Americans and Asian Americans, quite a few White Americans, and one African American man. It was Sunday morning, the third and final day of the biannual North American Taiko Conference, and I was buzzing from not enough sleep and a lot of taiko. I wanted to catch this jam on video. Some jams in past years have had practically mythological significance (*do you remember the time when Kenny Endo suddenly laid into the odaiko and*—), though this jam probably hasn't gone down in memory that way—it was okay but not amazing. Taiko players aren't any more on at nine o'clock in the morning than most other musicians, and these were more die-hard musicians than taiko heavies; I didn't see any ensemble leaders or senior musicians out here. But it was still fun, and there were at least a hundred musicians out on the plaza, gamely playing in the morning sunlight. I watched three young people horsing around together on two drums, trading places, playing with solos, laughing a lot. A middle-aged White man was playing full tilt with 200% effort on one of the few tall standing drums (*odaiko*). Four or five older Asian American women wearing matching T-shirts were shyly playing the *ostinato* pattern, looking around with a kind of self-conscious hopefulness, clearly not confident enough to improvise. What you can't see is that I joined in after a while; I turned the camera off, put it away, and got out my drumsticks. None of my friends were there, so I just cruised around and jumped in wherever a spot was available. A couple of guys were keeping the bass line going, but there wasn't any

sense of everyone locking in. It was more like a big loose party, fun but not deeply satisfying.

Another jam, two months earlier, at the annual Intercollegiate Taiko Gathering, held in May 2005 in an outdoor plaza between buildings at the University of California, Irvine. All the participants were undergraduates in university taiko clubs. For me, the intercollegiate taiko gathering has a distinctively Asian American feeling even though it is not framed as an Asian American event. It illustrates the changing shape of "the taiko community," in this case the rise of a younger, primarily Asian American generation of performers. This jam went on for about forty minutes and involved about three hundred students at its height. There was no clear beginning because students brought drums out continually and joined in whenever they were ready. I was right in the middle of it but the Gen Y feeling of the gathering was pronounced, and I felt a bit like a middle-aged fish out of water. So I didn't play, but the feeling of this jam was fantastic. It was incredibly loud and driven by a youthful energy that was infectious. Listening was a matter of giving myself over to the wall of sound and occasionally zeroing in on whatever was in front of me. Most of the drums were pointed inward in a loose circular formation; the students in the innermost areas were the most focused on one another. Further out, the sense of exchange and locking in was more diffuse but still present. The spirit was contagious despite the wide range of skill and experience, and the jamming moved in and out of following a leader (Kris Bergstrom, a former club leader now admired as a teacher) and just *going*, all on its own. Almost all the participants were Asian American (plus a few White Americans, and a sprinkling of Latina/os and African Americans). Many of the students stayed close to their own groups and chose to play beside friends, but some entered into the bacchanalian spirit of the mass and moved around, joining smaller concentrations for a few moments and then moving on. A line of *fue* (transverse flute) players formed and started to wander through the chaos, first on the periphery and then snaking through the scattered drums. Eventually Kris, the young teacher, jumped up and led everyone into a thunderous roll and then out of it into a cadence known by any taiko player. The sudden sense of conjoined, coordinated playing was powerful in an entirely different and satisfying way. After the last beat, everyone broke into applause.

A generalized description of jams would go like this: A taiko jam is always loud, features extended moments of chaos, and is hugely enjoyable for the participants. That's actually the point: a jam is a participatory event rather than one to be watched or listened to as an audience member. A good jam (and most jams are good) begins in a spirit of camaraderie and goes up from there: as the heart rate rises and you really begin to sweat, you settle into the groove and you begin to feel euphoric—there is no other word for it. The combination of endorphins firing from cardio activity and the musicianly pleasures of locking in with others takes over, for me and for other taiko players I know or have spoken with or have played

with.[8] Jams have no single or authoritative drive, affect, or result. They end when people get tired. Or if people get tired of the *ji* (the ostinato pattern underlying everything), someone starts a new *ji* and it spreads out from that musician like a shockwave until everyone has shifted over to it. The entire jam ends, rarely, when people get tired and, one by one, just stop playing or, more typically, when someone takes charge and moves into *oroshi,* a pattern of starting very slowly and speeding up until you are playing a roll, and then gradually slowing down again. In a jam environment, getting everyone into a roll isn't difficult, and after settling into the physical effort of keeping it up for several minutes (it's known as an exercise in stamina), everyone starts to look/listen/wait for the cue to start slowing down, and someone inevitably offers that cue.

I am drawn to the political potential of taiko precisely because it is characterized by many performative practices like the jam. The rules for jam participation are clear: you *should* participate. Listening and participating are co-constitutive. Individuality and its diffusion into the heterogeneity of the group are both essential. Instigating or following are options in equal measure. The shared aim is to create something bigger than you; you know you literally *couldn't* do it by yourself. It's an experience you can only have in the middle of this uproarious mass, and only if you accept its generous terms. Learning this means learning something about the North American taiko community . . . and about ethnography, community-building, activism, and cultural change.

A passage in Dorinne Kondo's ethnography *Crafted Selves* (1990:16–17) has become justifiably famous for its finely textured writerly enactment of shifting subjectivity and disorientation—a breakdown of racialized and anthropological location. Kondo recounts how, after many months of fieldwork in Tokyo, she was shopping for supper and caught a glimpse of a Japanese housewife reflected in a display case . . . and then realized with a jolt of reorientation that she was looking at herself. She relates that moment as a breakdown of self in almost psychological terms, as a moment when her Americanness and Japanese Americanness and scholarly persona were wiped away in a second of nonrecognition that was deeply disturbing and even shattering for her. My own such moments of dis- and reorientation are ongoing and systemic to the effort of researching a practice that has in fact become central to my sense of self even when it does nothing but offer too many possibilities for how I have not yet become the taiko player or the ethnographer I want to be.

Playing in Place

Performative ethnography isn't a disintegration of ethnography into question upon open-ended question. It may fracture a given moment into mirrored possibilities, but I am not using it to shut down the necessity of arrival points. Rather, I have tried to demonstrate the contingency of certain moments. Any moment is

a doorway into multiplied possibilities. But I have chosen moments that I consider significant, and these choices were made by a subject who is both ethnographer and taiko player.

In my writing about taiko to date, I have routinely danced around the fact that North American taiko is never as visionary, critical, or progressive as I wish it were. Its history in the Japanese American community carries the weight of historical discrimination, and an awareness of those issues informs many venues where taiko is showcased, for example, internment camp pilgrimages, Asian Pacific Islander heritage events, and a recent museum exhibit.[9] But taiko players are not particularly apt to spend a lot of time talking about politics or meaning, and there is no simple reason for this. I know that some of those reasons include multicultural ideologies that encourage ethnic celebration but discourage communities from asserting ownership of their own traditions; I would argue that the memory and experience of internment is so deep that it is unspoken but always present, not necessarily voiced but always a point of reference. The radical politics of taiko are more likely to be asserted in filmed documentaries than by taiko players themselves. The performative politics of a loud and disciplined Asian American presence in an American public sphere that still thinks of race in terms of a Black/White binary is understood by taiko players in some ways and avoided in others. Even as I dig deeper into the groups to which I am fiercely committed—groups doing tremendously important cultural work—I long for an imagined taiko group that I could find or start on my own, one that would be all *about* Asian American identity, or maybe even feminist Asian American identity. I have a stake in what taiko becomes, and how it is deployed, and for whom. No single moment marked my move into this investment, and it is completely entangled with my hopes for performative ethnography. The deeper purpose of my ethnographic practice is the matter of performative effects. How does taiko change the terms of racialized practices? What kinds of interethnic and intergenerational encounter does it teach and demand? How can we think of taiko as the public performance of critical pedagogy?

My research is therefore not about me, but it hinges on autoethnography. It is a study of what taiko does in some places, for some people. It is very much a study of what taiko might become in some of its possible futures, and how it might carry forward pressingly important work in which I hope to participate; it is anticipatory in Attali's sense. I don't posit a simple collapse of self and other, because that is a political impossibility for critical work based on race. I hope that writing about taiko from the viewpoint of immersion will allow me to learn how performative ethnography creates engaged encounters that offer strategies for social change.

Notes

1. Michelle Kisliuk has been an inspiration for years, and I thank her for helping me think through the parameters and implications of performative ethnography. Faculty and

graduate students in the Departments of Music at UNC–Chapel Hill and the University of Virginia offered productive feedback in winter and spring 2006. Tim Cooley asked the right questions and was a skilled and persuasive editor.

2. There are a number of scholar-performers in taiko to whom I also address myself here, notably Angela Ahlgren, Paul Yoon, Mark Tusler, and Shawn Bender.

3. My work on Asian American performance is part of the larger body of post-1970s scholarship known as critical race theory: I treat American ideas about race as a social construct, and I assume that racial formations change over time only when material and economic conditions force adjustments. Those formations serve the interests of a given power elite that has no reason to invite change. Racialized asymmetries create the conditions for multiple, intersecting identities. Within those basic conditions, I view the Black/White binary as a problem and the work of understanding other racial formations as an important corrective to the invisibilities created by it. I am invested not in liberal humanist hopes for a level playing field but in questioning the terms of racialized disenfranchisement in a post–civil rights nation. Listening for Asian American presence and subjectivity through performance is one of many ways to contribute to the broader project of critical race theory.

4. Ted Solís asks productive questions about these matters in the commanding introduction to his edited collection *Performing Ethnomusicology: Teaching and Representation in World Music Ensembles* (2004), though some of the contributors are less reflexive about the built-in risks.

5. In the last few years of his life, Hood was interviewed by Ricardo Trimillos (2004) about the contribution of world music ensembles, but Hood's references to learning music remained both vague and romanticized. The questions that preoccupy ethnographers and ethnomusicologists at this historical moment in the early twenty-first century are profoundly different from those emphasized in the 1950s–1960s, when Hood was most active professionally. See Feintuch et al. 1995 for a sophisticated collection of essays that explore these matters further.

6. For example, Slawek refers to his own efforts "in translating my experiences and knowledge gained through discipleship into ethnomusicologically acceptable research" (1994:16).

7. See Wong 1998 for a discussion of how Thai funerals sometimes feature multiple music ensembles to create a powerful rite of passage.

8. But my/our experience isn't the only one possible. Perhaps you aren't very experienced yet, and you're feeling anxious and self-conscious. Maybe you're so worried about playing and knowing what to play without the security of a script that the experience is not at all pleasurable. Instead, you feel awkward and uncertain—it is so completely different from the disciplined pleasures of memorizing a piece and playing it well with people you know. Here, you don't know any of the people you're "playing" with, and they all seem to be better than you.

9. "Big Drum: Taiko in the United States," at the Japanese American National Museum in Los Angeles, July 14, 2005–January 8, 2006, overseen by folklorist and associate curator Sojin Kim. The political value of taiko was foregrounded in this landmark exhibit.

Virtual Fieldwork

Three Case Studies

Fieldwork and Virtual Technologies

In this chapter we ask how changing technologies of communication and information dissemination reshape our understanding of fieldwork in theory and practice. As Timothy Rice reminds us in this volume, the "field" in which we work is a metaphor. There is no "there" to which we must go. Maybe some of our metaphorical fieldwork is better done at home in front of our computers, watching a television program, listening to a radio broadcast, and so forth—all technologies that can be included in research methods we include under the rubric "virtual fieldwork." With three independently authored case studies from three very different projects, we demonstrate the varying ways virtual fieldwork is redefining ethnographic method.

Virtuality, as we are using the term, is the technological mediation of human interaction (for example, a telephone conversation places another's voice and ear virtually in your hand, and an e-mail exchange can become a slow-motion texted conversation), and also technologically communicated and constituted realities (the online video itself, the e-mail message itself, the chat room, even the Internet itself, as well as not-so-new technological products such as a film as it is screened, a sound recording as it is auditioned, etc.). Though we appreciate the communicative power of computer-aided communication (currently the Internet, in particular), experiences of virtuality are facilitated by older technologies as well. As Miller and Slater noted, virtuality "is probably intrinsic to the process of mediation as such" (2000:6). Timothy Taylor, for example, presents an interesting account of related debates around an early music reproducing technology—player pianos in the nineteenth and early twentieth centuries. Some of these devices were advertised as being capable of bringing famous musicians and even dead composers into one's home (Taylor 2007:298–99). This type of commodification of musical sound, ac-

cording to Taylor, moved people toward accepting what we are calling the virtual presence of, for example, performing musicians when they are not present in person. The question becomes how the virtuality becomes a part of peoples' very real experience. Recent ethnographic work on people using the Internet tends to interpret the virtuality of cyberspace as "a *part of* everyday life, not apart from it" (Miller and Slater 2000:7, emphasis in original). The Internet, for example, is a socially embedded phenomenon; the virtuality of the Internet is not separated from reality (Miller et al. 2004:80). Virtuality is only as real as any other cultural production; it has only the meaning with which people imbue it. Focusing on how *people* experience—and invest power and meaning in—communicative technologies returns the "ethno" to virtual *ethno*graphy. Virtual fieldwork employs technologically communicated realities in the gathering of information for ethnographic research. For us, virtual fieldwork is a means of studying real people; the goal is not the study of the virtual "text," just as for ethnomusicologists (generally) the subject of study is people making music rather than the music object exclusively.

The nascent ethnomusicological literature on the Internet concerns the use of technology to distribute the results of fieldwork, not the fieldwork method itself (i.e., Lange 2001), and studies of virtual communities that exist only online (i.e., Lysloff 2003; K. Miller 2007). Lysloff provides a helpful contrasting of his fieldwork in Java (travel, physical hardship, loneliness, all the markers of traditional anthropological fieldwork) with his study of an Internet musical community ("solitary work in front of a computer monitor," not what leaps to mind when we think about fieldwork) (2003:234–236). There is little literature on how our musicological research is affected by the perceived "technological divide," not only between those segments of society and large areas of the world that have no or only limited access to, for example, the Internet, but also between scholars who use new technologies and those who do not (Lange 2001:144). Yet in some ways we feel that the impact of the Internet on fieldwork is part and parcel with other rethinkings of the method encouraged most effectively by postcolonial theory. In 1995, George E. Marcus wrote of an emergent, interdisciplinary applied mode of ethnographic study, one that eschewed the restriction of in-depth research to a single locale in favor of a multisite approach. Such an approach, Marcus observed, was suited to objects of study that must be investigated "in diffuse time-space," including identities and cultural meanings (Marcus 1995:96). Mediated culture exists in this kind of immaterial realm, and the multivalent ideologies and identities with which it is inscribed may be generated not only in geographically mappable places, but also in cyberspaces. Christine Hine notes that conventional ethnographic methodology has been adapted to accommodate diverse disciplinary approaches, and multisite research is particularly conducive to a closely focused object of study (2000:41). In other words, it is less appropriate for the holistic examination of an entire way of life than for the study of people in specific aspects of that life such as, for example, people's practices as television viewers or music consumers.

Here we offer three case studies that show how three different scholars are both studying the use of virtual technologies and using virtual fieldwork as a component of larger projects. We move from what we feel is an obvious and even necessary subject for employing virtual fieldwork, Meizel's ethnography of the cultural practices in and around a reality television show, to a subject where virtual fieldwork is perhaps less obvious, Cooley's work with vernacular musics associated with surfing in California, and finally to a case study involving one of ethnomusicology's classic subjects, North Indian Hindustani music. Though the study of non-European classical musics is a tradition in and of itself, Syed shows us that how this music is taught and learned today is highly mediated by the Internet. Collectively we find that virtual fieldwork lends itself to new ways of engaging well-worn ethnomusicological questions: What is the relationship between music and identity for individuals and groups? How are communities created and maintained through musical practices? And perhaps most significantly for this study, do the ways in which people communicate cultural practices change those cultural practices? Challenging the polemic binary between "virtual" and "real" in the way we conduct our fieldwork, we seek to understand technologies of communication as human constructions that are as real as any other human cultural production. In that context, we experience virtual fieldwork as an organic part of our communicative research process with real people that we may or may not actually meet face-to-face.

Idol Fields: Mapping the Multisite Ethnography of a Television Show (Katherine Meizel)

By the time I drove to Los Angeles to interview communications major Bao Viet Nguyen, I had watched him sing on television, scrutinized posts about him on an electronic message board, read his blog, conversed with him by e-mail, and spoken to him over the telephone. All of these sites were key to my research, because they were also key to the packaging and marketing of Mr. Nguyen's identity on the televised talent competition *American Idol* (Meizel 2006). They also figured in the way he negotiated his encounter with the show. After his initial audition for the 2004 broadcast season, he documented his experience in a personal blog. When that audition aired in January, he watched as Internet debates seethed about how the show presented him, and his family, as refugee immigrants and paragons of the American Dream. His *Idol* time took place at multiple sites—in a Hollywood studio, in his home, where he saw it all unfold on a TV screen and on a computer screen. I followed him across all of these mediated locations in order to continue that story in the pages of my dissertation.

My research investigates *American Idol* as an arena where American identity politics are performed. This process is a collaborative one, involving producers, contestants, and viewers, and it happens across numerous places—onstage,

onscreen, online, and even *in* line, as thousands attend *Idol* auditions and other events. *American Idol* is a global phenomenon, with a corporate base in the United Kingdom and broadcasts that reach more than half the countries in the world[1]; it is an American phenomenon; it is a local, grassroots phenomenon across the United States; it is a technological phenomenon manifest in ringing cell-phones and millions of text-messaged audience votes, and in the daily discussions at hundreds of virtual watercoolers throughout the World Wide Web. A research object like this, whose existence is widespread across physical and virtual space, provides a somewhat nebulous field for ethnographic study. While I have included as much physically present participation and face-to-face contact as possible, reading Internet discourse and watching television add particular complications to the construction of multisite research. Television viewing might seem to be a kind of ultimate armchair ethnomusicology, a perception compounded by adding the analysis of Internet texts. However, the exceptionally interactive nature of *American Idol* as a televisual experience provides a cultural setting that is inherently conducive to participant-observation.

Like many turn-of-the-millennium forms of entertainment, *American Idol* relies extensively upon strategies of media convergence (Jenkins 2006) to ensure maximum investment of consumers' attention and financial resources. In the process, it capitalizes on the increasing blurriness of the line separating producers and consumers, and the implications of agency offered with viewer participation. This involves many kinds of place, from the audition stadium to the television studio, from the living room to the chat room, from the small town to Hollywood. Viewers can become contestants (in a sense, the product), auditioning for producers at selected nationwide locations. They can be transported onto the television screen this way, or by attending tapings of the live show. They become participants in the narrative of the show when they vote by telephone or text-message to determine the contest's winner, and become a pre-made market for the albums released following each broadcast season. A regional community often makes a concerted effort to organize voting in support of a hometown contestant, and the FOX Network's official website for the show enlists another kind of community, providing a discussion forum that—until the webiste was replaced in 2006—was explicitly labeled "community." The complete experience of *American Idol*, then, makes use of physical place and virtual space, established technologies such as television and recent innovations such as text messaging or digital music/video downloading. This networked design highlights an important consideration in the study of popular music: the field includes *all* of the expanding, shifting sites where culture is produced, disseminated, and consumed.

The field I have constructed for my research accommodates *American Idol*'s polylocal and multisite organization. I have attended auditions, public dress rehearsals at the show's Hollywood studios, and concerts, and at all those events have spoken with numerous fans and hopeful contestants. I have watched four complete

seasons of the show, discussed each episode with others, and voted. I have even entered the critical blogosphere as a journalist at an online magazine (Slate.com). Several former contestants were kind enough to grant me interviews, as were the *Idol* vocal coach and associate music director. Obtaining interviews is not always easy in the context of a commercial enterprise like *American Idol*, whose vastness and corporate structure have sometimes thrown a wrench into my plans. Many of those I wish to talk with are celebrities—whose status I helped to construct as a viewer—who speak only at press conferences, in media appearances, or through publicists. I have used fan-maintained websites, corporate websites, and even university e-mail directories to acquire contact information. Once, I used a free trial membership at an industry website to locate a publicist and procure an interview. (I cancelled the membership immediately after.) Calling the *American Idol* offices usually gets me passed circuitously from extension to extension, even from e-mail to e-mail. While I am never outright denied by the show's staff or by celebrities, I am often—loudly—ignored.

The official *American Idol* website also proved important in the early stages of my study. Owned by FOX.com, FOX Interactive Media, Fremantle Media, and 19 TV, the site displays biographical and personal information about the contestants and judges for all six seasons, as well as updated show information, links to advertisers and show merchandise, and so on. Most significant on this website is the "community," as the hyperlink indicates—the message board that provides minute-by-minute information about reception and the audience. While its literally textual nature makes research on the message board seem largely archival, it contributes to an approach that is at once historical (recent, from the show's 2002 debut) and synchronic. Though this method might be more Amazon.com than Amazon rainforest, it has nevertheless been part of my ethnomusicological fieldwork. It has even influenced my approach at physical sites. Most notably, it has provided information on how I and my companions might navigate the audition experience, and has alerted me to some of the concerns shared by many viewers regarding the show. In the latter case, it has been a bit like familiarizing oneself with the literature on a community before engaging in face-to-face ethnography.

In the end, though, my online research ran into trouble. As Barbara Lange observes, its continuously developing and changing technologies make hypermedia "far more ephemeral than print" (Lange 2001:145). Information gathered from the Internet exists at an intersection of oral and print culture, simultaneously fixed and unfixed in virtual space, and even something available online for years can, as I found out the hard way, vanish overnight. I had been advised by my university's Human Subjects office not to post on the official *American Idol* website myself, to avoid potential problems due to FOX's ownership of all material therein, including electronic messages. So for two broadcast seasons I read, and when I had chosen the posts I wished to quote, I was ready to set about the process of asking some follow-up questions and obtaining permissions—only to discover that just weeks

earlier the site FOX had provided for nearly five years had been removed. It was replaced with a revamped site on a new platform that left no access to the previous seasons' pages, or their forum texts. With no way to retrieve their contact information, I was unable to communicate with any authors of the posts I wanted to cite. I returned to the Human Subjects office for guidance, and it was determined that I should make sure none of the posts were still available in any way online, so that they weren't traceable to any author, and that I should provide no identifying information—such as screen handles or posting dates—in the text of my study. On the advice of the Human Subjects advisors, I kept my initial hard-copy printouts of the posts I quoted, and made a list citing dates and screen names for my own reference. This situation is far from ideal in the context of ethnography, as it makes both my initial treatment and my analysis of the forum's content exclusively textual. René Lysloff cautions against just this kind of singular focus on "cultural texts," as disconnected from the "public culture" of which they are a part (Lysloff 2003:233). Though my use of it did not in itself constitute fieldwork, this virtual source has been important in my project nonetheless, particularly as a way to gauge what I learn in the physical field. Discourse I hear at auditions, rehearsals, and concerts sometimes contrasts with what I read online, demonstrating potential discrepancies between written and other forms of fandom. But in my proverbial 20-20 hindsight, I believe that if I had chosen a public Internet forum instead of a commercial one, I might have avoided the issues that made my virtual mistakes into real problems.

As I have been studying an intensely mediated cultural product (or network of cultural products), I am participating in some ways I had not considered before. And my participation has raised a question about the nature of fieldwork for this kind of study: Does watching a television program make me part of an audience group, even if I do not consider myself a "fan?" I started out with this consideration in mind, attempting to maintain some distance, but eventually I was sufficiently motivated on several occasions to vote for my favorite contestants, to participate in the show's process, and came to think of myself as a fan after all. Sometimes I have had no choice other than to be an observer only, as when I twice attended auditions for the show—my age exceeded the limit for performers, and I could only watch from the crowd in the stadium as the singers completed their tryouts. I did, however, participate enthusiastically in the larger activities, as when the entire assembly was asked to perform songs together for potential broadcast footage.

Though my age prohibited me from becoming a contestant, I have approached my research on as many levels as possible in order to maximize my participation in *Idol* culture. I have become a viewer at home, a member of the studio audience in Hollywood and of the concert audience during the post-season tour, a part of the crowd attending auditions, an enthusiastic voter, and, during my journalistic ventures, part of the online *Idol* discourse. I was forced to reflect on the extent of my participation seriously when, in line for a May 2005 dress

rehearsal, one interviewee asked with concern whether I was actually going to the rehearsal, or just collecting information. He was relieved when I assured him that I was attending the rehearsal and pointed out my friend holding our place in line as I talked with others. It was important to him that I was not entirely an outsider, but a genuine fan of the show, and his willingness to discuss his *American Idol* experiences hinged upon my own participation as a viewer.

There have been further ethical considerations throughout my fieldwork, as well. I managed to attend the 2004 auditions only because I wrote to a friend asking if she knew anyone auditioning, as attendees are required to either be singing themselves or there to support a singer. She suggested a mutual acquaintance, who then invited her close friend to join us. Both women are beautiful singers with careers in music now, but both were turned down in the first round of auditions. We had thoroughly discussed the poor odds of being cast in *American Idol* before they made the decision to try, but nevertheless one of the women seemed devastated as we left the auditorium, and I thought about my own accountability for her emotional distress—if I hadn't been looking for someone to tag along with, it is possible that neither of the women would have gone to the auditions, and neither would have experienced the strain of a cold, sleepless fall night in line in the Cow Palace's outdoor parking lot, or the pressure of an audition and its disappointing result, or any additional pressures contributed by my own presence. I mention this not primarily out of guilt, though I feel not a little, but because it was my first personal experience of the ways in which I have been "casting shadows in the field," as geographically and ethnographically familiar as my "field" might be to me. The three of us who attended those San Francisco auditions, just hours from where I live, were very close in age, educational background (two of us even had the same vocal instructor as undergraduates), and musical interests. But issues of power are pervasive in *American Idol*, with ostensibly authoritative musical judgments delivered, relentlessly and sometimes brutally, by producers at the auditions, by the three onscreen judges who appear in every episode, and by the viewers collectively addressed on the show as "America." And studying participants in the making of a television show carries particular risks. While FOX requires potential *Idol* contestants to sign an official release, acknowledging that participation might result in "public ridicule, humiliation, or condemnation" (IdolOnFox.com, August 2005), I, as an ethnographer and not a corporation, have no such convenient recourse for absolution. Even in a situation where issues of power and cultural Otherness are not as outstanding as they might be in studies involving decolonializing processes or class divisions, a fieldworker nevertheless needs to understand his or her responsibilities in the way others experience their musical worlds.

American Idol's omnipresence in the media and in pop-culture consciousness requires both conventional and unconventional approaches to research, and raises both familiar and unexpected problems. In the ethnographically driven study of such an interactive television show, participant-observation comes easily, but it is

necessarily a multisite undertaking. This is fieldwork at home, and also figuratively abroad, in the dauntingly expansive corporate realm of the transnational entertainment industry. As that industry continues to develop, adopting new technologies and negotiating ever-changing ideologies, ethnographers will need to adjust, to keep following the music from where it is made to where it is sold, to follow producers, musicians, and consumers as they shape the field of popular culture together.

Wi-Fi Hawai'i (Timothy J. Cooley)

I was in paradise—and in a tight spot. In November of 2006, the Society for Ethnomusicology held their annual meeting in Honolulu, Hawai'i, and I was to present a paper on music associated with surfing in California and how it was related to Hawai'i. Surfing was introduced to California by Hawaiians, as is well documented, but sound waves are even more difficult to catch and document than ocean waves. One can learn much about the art of wave riding in the 1920s and 1930s on the continent's west coast by studying photographs of California's pioneering surfers; yet photographs of these same individuals playing guitars and ukuleles, and dancing on the beach conceal as much as they reveal. Even the best photographic technology of the day could not capture sound waves, and no one thought to bring (or thought better of bringing) a delicate shellac disc recorder to the wind and sand-swept post-surfing music sessions. Surviving memory accounts of pre–World War II California surfing music were tantalizing in their terseness—a few names of surfing musicians, Hawaiians who played music in coastal California—but the sound image was washed out by the tidal wave of popularity of the Beach Boys and the Dick Dales of the postwar surf boom.

What had me stuck in an expensive hotel room overlooking the legendary surfing beaches of Waikiki was new information. Provided just days earlier by a ninety-year-old man, this information was moving me toward new and deeper understandings of the early twentieth-century cultural exchanges between Hawai'i and California. E. J. Oshier was a pioneering California surfer, photographs of whom I had literally been staring at for years. In the pictures, taken at at San Onofre (a beach just south of San Clemente, California, with waves similar to Waikiki), Oshier was riding wooden surfboards and playing ukulele. But the photos would not speak and the ukulele would not sound; until they did one day while I was doing old-fashioned research sitting in an archive poring over rare publications on surfing at the Surfing Heritage Foundation in San Clemente. E. J. Oshier, it seemed, was living just down the way somewhere, and for the past decade or two was known more for playing music on the beach at San Onofre than for surfing. Internet and other directory searches did not reveal his contact information.[2] It was again word-of-mouth data collection that, after some months, finally produced a phone number. I called, and Mr. Oshier answered the phone

and agreed to a meeting. Less than two weeks before I was to read my paper in Hawai'i, I was interviewing one of California's surfing and music legends.

What did music sound like on California's beaches in the 1930s? Even sitting with the sole surviving surfing musician from that era could not remove a seventy-year sound decay. Oshier still moved on the same tall, lean frame of the surfer seen in the 1930s photos, but his voice, as he explained, was no longer capable of the falsetto singing that he associated with Hawaiian singing, and his fingers were no longer able to navigate the fingerboard of a ukulele or guitar. His frustrations with this were palpable, but his memory was keen and fueled by photo albums and a three-ring binder that piqued my interest. The binder was filled with word-processed song texts with chord charts and occasional song histories, many of which noted songs taught to the compiler by Oshier himself. The ethnographic radio tuner was picking up a signal from the 1930s; maybe if I adjust the volume a bit . . .

The live person-to-person interview is an ancient ethnographic method that crosses technology platforms well. That I introduced a digital audio recorder to my interview with Oshier seemed, in my mind, to remove none of the humanity from the conversation: an old and experienced surfer and musician had valuable and fascinating stories to tell this younger surfer, musician, and scholar. I love doing live interviews, and the slightly formalized human sociability that recorded interviews tend to encourage. The value of this fieldwork method exceeds that of the information inscribed on audio or video recording storage formats, mechanical, electronic, or digital. Yet another way to experience this value excess is that it ultimately overwhelms technology platforms. My cutting-edge audio recorder could handle only verbal data about the historical photographs that I deliberately asked Oshier to describe "for the recorder." The notebook of songs played by Oshier and his friends similarly resisted platform crossing. I can be heard reading off the names of songs and their composers on the interview recording—an oral attempt to cross technology platforms.

Nevertheless, the notebook of songs, chord charts, and histories that Oshier showed me that November afternoon was the logocentric material print-off of digitally manipulated data. Oshier informed me that they were compiled by a young ukulele player and regular at San Onofre, David Weisenthal. Resisting the urge to snatch the manuscript up and to run to the nearest photocopy machine, I acquired Weisenthal's telephone number from Oshier, and was dialing that number when sitting in my Honolulu hotel room.

Laptop open, hotel Wi-Fi service paid for, I was pulling together the final pieces of a puzzle that connected Oshier's ukulele playing on California's coast with even earlier Hapa Haole Hawaiian music by Sonny Cunha and with the recent music of Jack Johnson, a currently popular Hawaiian surfer-musician of California extraction. Sonny Cunha and Jack Johnson were linked sonically through the Hawaiian string-band-style covers of Johnson's hit songs by Rick Cunha, Sonny's grandson and a resident of California. It was a beautiful circle, Hawai'i to California, back to

Hawai'i only to be covered by the grandson of the "father of Hapa Haole music" who lives in California. I was calling Weisenthal to ask if he and the San Onofre group still play Sonny Cunha songs there on the beach. They do (circle closed). We exchanged e-mail addresses and a stream of data began to flow between our computers including names, phone numbers and e-mail addresses to a veritable village of southern California surfing musicians, photographs of many of these individuals (typically playing music on the beach), extended discussions about who does and did what, and in a roundabout way, a substantial archive of MP3 files of a surfing ukulele player. The old photos of Oshier were beginning to sound.

The MP3 files connect several California surfing musician groups and offer me new ways of fieldwork interaction, virtual and live. The recordings were created by Mike Goodin of Manhattan Beach, a small coastal city, part of the Los Angeles metropolitan area. The story of how I came to receive them summarizes some of the promise and problems of virtual fieldwork. It was weeks after my phone call from Hawai'i before I met David Weisenthal in person at San Onofre. By that time he had arranged for me—with e-mail and probably a few phone calls—meetings with key surfing musicians, and a group of senior Hawaiian musicians, from San Onofre to the Los Angeles area. Some of these individuals he had never met face-to-face but knew only through the digital exchange of information. Mike Goodin was one of the individuals whom David arranged for me to meet but whom he had not actually met in person; they were virtual acquaintances. Because of David's organizational acumen, I met Mike and several other Manhattan Beach surfing musicians, and have had the privilege on a few occasions to surf early in the morning with the Manhattan Beach crew, followed immediately by ukulele playing, singing, and an occasional hula in, of all places, municipal parking lot 26 (fig. 6.1). I've recorded several sessions of the Manhattan Beach crew in the parking lot, and their repertoire, including some of their own surfing-inspired compositions, is taking my research in new and unexpected directions. This research is complemented in no small measure by the some 45 MP3 audio files self-recorded by Mike Goodin playing ukulele and singing. These files, along with Word.doc files of each song's lyrics and ukulele chord charts representing Goodin's arrangements, are for me a primary source in digital form representing one individual's conception of present-day Californian surfing music. Producing a similar sample of a research subject's repertoire the old-fashioned way—recording in the field and transcribing myself—would have taken months. Yet alone the MP3 and .doc files are without context; they reveal nothing of Mike's surfing skill, his rapport with his fellow surfing musicians, or the way playing music can round off a morning at the beach.

In the short twenty-plus years that I have engaged in ethnomusicological fieldwork, I have experienced and taken part in a shift in technology and communication standards. The musical forms that interest me most and that I take the most pleasure in studying center around individuals gathering at the same time

Fig. 6.1. Manhattan Beach surfing musicians. Left to right: Laurie Armer, Mike Goodin, Al Lee, and Gene Lyon. Photo by Tim Cooley, 2 December 2006.

and place to enjoy each other's company and to make musical sounds together. When studying and teaching intensively mediated popular music genres—genres with strong virtual representation—I emphasize the aspects of those musical practices that require direct human interaction. Yet for my entire life, human interactions have included mediated exchanges of meaningful cultural expression. Radio, television, even print media offer their own forms of virtuality. Changes in communicative technologies do change the ways we interact with other people, and these changes are important to consider as we re-imagine fieldwork.

Since 1984 I have engaged in ethnomusicological research in very different settings from Papua New Guinea, to Chicago and rural Illinois, to villages in Central Europe, and now coastal California. For most of this work, introductions to individuals with whom I hoped to work were negotiated by a third party (a teacher, a governmental or some other authority, a well-placed acquaintance). This introductory process typically included letters (hardcopy), telephone calls if phones were common in the research site, and often a live introduction by the third-party facilitator. I maintain that it is still a good idea to take Thomas Rhys Williams's advice to write letters to the appropriate officials before traveling to a field (1967:7), and Edward D. Ives's recommendation that one write a letter to an informant

introducing oneself and one's project (1980:36). Of mediated communication technologies, the telephone remains a handy device for virtual fieldwork. We can talk with another individual who may be miles, even continents away, but their voice and ear is virtually present in real time. The addition of live video links, today most commonly mediated through computers with high-speed Internet access, adds moving image to the immediacy enjoyed in phone conversations. In my early fieldwork in Poland, a telephone conversation between America and Poland was a major event usually arranged ahead of time. Though conversation in person is always to be preferred, a telephone conversation is effective, and I then and now occasionally record interviews, always with permission, conducted on telephones.[3]

E-mail communication between some field consultants in the Tatra Mountains of Poland was an option by the late 1990s. Yet I found then and still find now that the individuals with whom I am able to correspond via e-mail represent a very narrow slice of the communities I was researching. Then it was limited to university students when they were in session, away from their home villages. Eventually, a few additional individuals acquired e-mail access through their employment, and now there are several Internet cafés in Zakopane, a central location to my research area, but I can still count on one hand the consultants living in the Tatras who have e-mail access, and they are all in their 40s or younger. But these few virtual correspondents are valuable. By the time I finished my book on Polish mountain music (2005), I was regularly e-mailing these few key field consultants, exchanging photographs and even audio files with these individuals. By then I could also fact-check using the websites of some of the musicians about whom I was writing—something inconceivable when I began researching Polish musicians in 1989.

My fieldwork among California surfing musicians began in 2004, and though hardcopy mail and telephone calls were especially important when I began making contacts, now many of my field consultants are first contacted via e-mail. With some of these informants, the bulk of my data may be exchanged via e-mail, though my most effective field consultants are individuals I have met with live at least once. After all, there is no Internet access while on a surfboard, and even in developed countries like the Republic of California, many people of Oshier's generation, and even of several generations younger, live in a resolutely analog world. They may possess digital devices from hearing aids to DVD players, but actually creating and manipulating data on a computer or digital audio recorder is not a technology everyone chooses to engage. I could not e-mail Oshier, for example. Other than through telephone conversations, I experienced Oshier virtually only through the recordings I made of his voice myself, live, in his presence. Though as with my other research projects, older community members such as Oshier are especially helpful because of their depth of knowledge, I find that Internet communication skews the age of my consultants younger and in some cases toward the more educated.

Paul Rabinow found that his best informants in Morocco tended to be like him in important ways: perhaps socially marginal, and apt to view their society from a step removed (1977; see also Robben 2007)—an interpretive positioning favored by anthropologists and to a large degree ethnomusicologists as well. William Noll found that in the field he, too, was "selecting partners" that resembled him in striking ways: they were ethnographers from the past whom he joined via the technology of writing (1997). Does the Internet and the increasing use of this and other virtual ethnographic methods amplify the tendency to find field consultants that resemble us in new ways that we have not fully considered? Does it draw together people who share similar technological proclivities? In the past, the traditional object of ethnographic interest tended to be elderly community members. Are these potential consultants less relevant in a methodology that employs newer forms of virtual fieldwork? Virtual fieldwork may change our research in other ways, as well. Does the ease of the exchange of certain types of data draw us to shape our research around that data rather than around other questions? And what of the context lost when we experience cultural practices virtually but not in person?

I am not sure if we can even opt out of some forms of virtual fieldwork and escape the sorts of criticisms Cecil Sharp received for rejecting early field audio recording technology in his song collection projects (Harker 1985: 194–195). For my research I choose to use virtual fieldwork, but will precede and/or follow up virtual communications and exchanges of information with person-to-person meetings. I find live human interaction essential for my research. But in a pinch, that old virtual technology called a telephone, or the newer ability to send text, sound, and video files around the world on the Internet are tools I am happy to live with.

Internet Guru (Nasir Syed)

My involvement with the Internet as a source of knowledge about Hindustani music is indicative of who I am, and the place and time in which I live. A first-generation American-born man of Indian/Pakistani heritage, I began taking sitar lessons in my late teens in 1999 as an undergraduate at UCLA. I became intrigued when I found myself in the presence of one of the world's finest sitarists, Ustad Shujaat Khan (Khan Sahab), a visiting professor in the Department of Ethnomusicology. With the guidance of my new teacher, my intrigue blossomed into a healthy obsession for learning, listening to, and practicing anything sitar. The obsession led me to India on several occasions and most recently culminated in my formal initiation as a "tied" disciple of Khan Sahab during a *Guru Purnima* ceremony (an auspicious annual celebration of the guru) in New Delhi in July 2006. My journey with the sitar has, however, simultaneously embodied and challenged traditional modes of learning within Hindustani music. My research suggests that I am not such an uncommon student in the twenty-first century. Here I explain

some of the ways that I use the Internet to support and supplement my study with Khan Sahab. As a student of Hindustani music, I find the availability of online educational information to be an especially powerful new site for my pedagogical development. I will show some of the ways the Internet is changing Hindustani music, particularly by expanding the way it is taught, studied, and archived.

As with just about any conceivable music cultural practice, the Internet can be a resource for both casual and rigorous research. As an example of casual research, before attending a concert of Hindustani music, I often search online for information about the musicians I am about to see perform. The Internet becomes a virtual pre-concert program guide as I navigate through the web to find audio and video clips, personal homepages, reviews, photographs, interviews, biographical data, Amazon.com sales numbers, performance histories, and discographies of the artists. Then after the concert, if I found the performance particularly interesting, I often repeat the process and re-navigate the web to learn more about the musicians.

This sort of casual research often leads me to—or links me to—more substantial sites of research and learning. For example, several of the artists for whom I combed the Internet in search of data are presented online via streaming audio and video clips teaching, demonstrating, and performing. Some of these sites provide what amount to virtual lessons. Many of my *guru bhai*s (guru brothers, or students of the same guru) increasingly turn to the same websites that I use in order to view an online video clip, for example, that highlights the left-hand technique of a particular sitar player. Expanding on a tradition that prioritizes face-to-face learning from one specific guru at a time, we may learn virtually, and simultaneously, from any number of masters—online. The masters we encounter online demand none of the filial responsibilities and many levels of devotion required by the traditional pedagogical model (though we must pay homage, as it were, to the Internet itself with our access fees), and we often never meet these online teachers face-to-face. Instead, these virtual sites collectively become a digitized "Internet Guru." I hasten to add, however, that my devotion to Khan Sahab is not lessened by my time spent learning from the Internet Guru. In fact, I have furthered my learning from Khan Sahab immensely by watching online videos of his amazing *jhala* (fast right-hand rhythmic movement), for example, on www .shujaatkhan.com. Khan Sahab himself remarked on the unarguably brilliant form and tone of this particular online recording. I further hasten to add that although technologically mediated learning from the Internet Guru is obviously different from a traditional model of learning, its potential for information dissemination is indeed being realized in the twenty-first century. Perhaps the homepage to Khan Sahab's own personal website illustrates the topic of my case study most aptly: "Shujaat Khan—A Symbol of Music in Evolution." My study of the sitar has expanded exponentially through what I consider to be my virtual fieldwork.

Beyond individual musicians discovered or researched on the Internet, my virtual fieldwork has taken me to several highly articulate sites created by esteemed

institutions devoted to Hindustani music. One example that I consider here is an online project undertaken by a premier music academy in Calcutta, the Sangeet Research Academy (SRA). The SRA website project exemplifies a contemporary crossroads between traditional modes of learning and new technologically mediated networks of knowledge. The Internet is changing the pedagogical landscape for both students and teachers within the Hindustani music tradition. The master-apprentice relationship, a traditional hallmark of the pedagogical structure within Hindustani music, is being renegotiated as Internet users alter the way people teach and learn instrumental and vocal music. As a direct participant in the changes occurring through the Internet, I have come to realize that aspects of my experiences with these changes are shared by many teachers, students, and fans of Hindustani music in India, North America, and elsewhere.

The SRA website provides a wide variety of instructional material ranging from detailed textual descriptions of genres and performers, to audio and video clips of varying length, to an unprecedented audio recording project known as Samay Rāg. The Samay Rāg project is an online database of audio recordings that collectively outline the essential characteristics of approximately 90 selected rāgs. The database is updated, at times, by the addition of rāgs. The Samay Rāg, a particularly rich example of the potential and possibilities for online learning, involves several senior teachers and students at the SRA. These musicians are among the finest and most well respected in North India. For each rāg, a vocal composition is recorded, accompanied by a transliteration of the song text into roman script and an English translation. In addition, the rāg is presented in theoretical terms with the ascending (*arohi*) and descending (*avrohi*) scales along with the characteristic contours (*pakar*) of the rāg. For example, Rāg Bihag is recorded by a well-respected and versatile female vocalist, Subhra Guha, in an audio clip streamable through RealPlayer, lasting approximately three and a half minutes. Within this short time, she effectively teaches the aforementioned components of the rāg and gives a performance in miniature to demonstrate the central qualities of Rāg Bihag. On one hand, it may be argued that short audio clips do not do justice to the potential depth of a rāg's development. However, I have heard great musicians comment on the brilliance of early twentieth-century recorded musicians who were able to accurately and succinctly express a rāg even within the short time frame afforded through the recording technology of that era. In either case, the audio clips and other interactive educational materials available free of charge on the SRA website can serve as a powerful learning tool for all students.

At first glance, it may seem that the academy, as a theoretical construct, is itself not a traditional modus operandi within the field of North Indian classical music. The Sangeet Research Academy, interestingly, claims to successfully embody and foster the traditional *guru-shishya* (master-apprentice) relationship in a modern institutional setting. Since the academy's inception in 1971, it has indeed grown to be recognized as a training ground for literally dozens of leading performers and

teachers of various styles of North Indian classical music. Simultaneously, however, the academy and the associated website serve as dramatic examples of the interface between tradition and technology. Perhaps the website itself states this tension most clearly in its "aims and objectives" page by explaining that the objectives are "based on a training system that is essentially the *Guru-Shishya Parampara* (master-apprentice tradition) with suitable contemporary fine tuning" (http://www.itcsra.org/sra_story/sra_story_aims/sra_story_aims_index.html: June 2007). With the breadth of instructional material that the SRA has made available online for public access, the website itself is a "contemporary fine tuning" that may shape the future of Hindustani music.

A second site for virtual fieldwork is Mehfil Tube. This website is a sizable database with hundreds of concert video clips of varying length compiled from various sources. The title is an obvious reference to the currently popular "You-Tube.com" that enables Internet users to view and post short video clips for free. The term *mehfil* refers to an intimate musical performance or soirée held in a small setting such as a home. The Mehfil Tube website provides free downloadable videos of North Indian classical music performances and music lessons for use by serious students, aficionados, and casual viewers alike. I argue that video clips available for public viewing are integrally connected with the learning experience for literally thousands of students of North Indian classical music. Both the Mehfil Tube and SRA websites, alongside dozens of similar websites, illustrate the dynamic nature of traditions changing through the intersection of technology. For students of Hindustani performance traditions, they collectively form a network of knowledge such that the Internet itself might be seen as the Internet Guru. Just as traditionally the student went and sat at the feet of the guru, now we can go and sit "at the feet" of our computers, accessing a vast wealth of digitally presented knowledge: the Internet is a new guru.

Perhaps the Internet in general, and online audio and video in particular, affords musicians and audiences a platform to share the wide web of talent in Hindustani music. Just as web surfers may have come to know me as a sitar player through the Internet, I have discovered literally dozens of sitar players by going online and following links. Most recently, I meticulously watched and rewatched amazing online concert videos of a relatively young and extremely talented sitarist whom I had not previously heard, Saeed Zafar Khan. By following links, I then reached his personal website, which I forwarded to dozens of other students and fans of Hindustani music with whom I am digitally linked. In a similar fashion, when others find someone new online, or new footage of someone already known, I am kept in the digital loop. The Internet allows us to link up with other artists, students, teachers, and fans in an attempt to stay connected to this web of talent in the world of Hindustani music.

Central to my argument is the ubiquitous use of websites such as these by students, performers, and teachers. Searching for "sitar" on YouTube.com recalls

several videos of sitar concerts that have been viewed more than thirty thousand times in less than a six-month period! Discussion boards connected to these videos contain several in-depth debates concerning musicological issues. As a student of Hindustani music, I find it empowering to discuss a particular performance of a unique rāg, for example, by posting on such discussion boards. I find it invaluable to hear a master musician outline, online, specific attributes and defining characteristics of a rāg. Literally dozens of video and audio clips of my own esteemed sitar teacher can be found online. I do not suggest that this format of transmission has replaced or could replace the traditional master-apprentice relationship. Rather, this digitally encoded resource for knowledge exists alongside or within the larger cultural practices that today make up the life of Hindustani music. The question then becomes: how can the Internet not be influencing students such as me? If an ethnographer is interested in understanding the modern landscape for learning North Indian classical music, websites such as these have indeed cast their shadows on the field.

Conclusions

Fieldwork should happen where music happens. As the apparatuses of music-making change, we must continuously re-imagine the "field," and redefine how we work there. If the performance and transmission of cultural practices are taking place in an Internet community, a website will make a rewarding research site. If public participation in music hinges on the convergence of old and new media, we must adjust our methods and embrace that juncture as our field site. And if e-mail is a part of how our consultants interact with others, perhaps it should also play a part in our own ethnographic relationships. However, we cannot enter any field blindly. Twenty-first century technologies emerge and grow with a seemingly unstoppable momentum. It is not only the sheer ubiquity of mediation, and hypermediation, that demands our attention as ethnomusicologists—new technologies offer new modes of communication, at once reflecting and shaping how culture is produced, performed, transmitted, consumed, and understood. But they are also, for many of us, something we take for granted, a part of our own quotidian lives, tools to which we instinctively turn to access and interpret the world around us. Especially because of our increasing technological fluency, it is important that we ask questions about dependence and responsibility. Should we use e-mail and the Internet simply because we can, or judiciously choose to do so when these resources clearly facilitate participation in a community? Does relying on current technologies limit our pool of consultants to those who have the same practical and financial capabilities as our own? And as we investigate online pedagogies in global classrooms, we must consider to what degree the World Wide Web actually brings people closer together, and to what degree distance learning means that we are learning distance.

Mediation is a conventional presence in our fieldwork, as when a microphone separates us from our consultants, or we hear and rehear them through our recorder headphones, or find our viewpoint in the viewfinders of our digital cameras. The instruments of the Information Age can at once benefit our work and magnify our reflexive anxieties about the impact of our data collection methods on our ethnographic integrity. Ultimately, we know, the heart of ethnomusicological fieldwork is contact with the human beings who make and live the cultural practices that we study. Virtual fieldwork can certainly influence, facilitate, or manipulate our experience of ethnographic contact. As an organic part of our very real experiences—a part of our everyday lives—virtuality is also one of the many ways we experience people making music.

Notes

1. According to Fremantle Media's website (http://www.fremantlemedia.com, May 2007), *American Idol* is broadcast to 113 nations. According to the *World Almanac and Book of Facts*, (McGeveran 2006:747) as of 2005 there were 193 independent nations in the world (this number does not include dependent territories), making *American Idol* available in 58.5% of all countries.

2. E. J. Oshier died February 19, 2007. Web searches in June 2007 produce more information about Oshier and his influence on California surfing than was available in 2006.

3. In the early 1990s, many Polish village homes did not have telephones, and the wait for a land-line phone could take years once a household applied for one. Mobile telephones, on the other hand, could be engaged immediately upon purchase and quickly became the standard by the end of the century.

Fieldwork at Home

European and Asian Perspectives

Fieldwork Unseen?

Many ethnomusicologists, possibly the majority worldwide, carry out fieldwork in their home societies. This chapter focuses on the experiences of two such fieldworkers, one from England and one from Taiwan. As partners, we have carried out much of our fieldwork together in recent years, comparing findings and sharing interpretations, each of us casting a shadow across the other's fields of inquiry, a point we return to at the end of this chapter. Jonathan Stock undertook the writing-up, so his work is described in the first person and Chou Chiener's in the third person, but this is very much a joint account.[1]

Reflecting on our home fieldwork is made challenging by the fact that the image of such study remains rare in English-language theoretical writing on ethnomusicological fieldwork. Even today, many such sources exhort us to adopt a model akin to that proposed by Malinowski, which John Van Maanen (1988:36) conveniently encapsulates as follows: "The fieldworker must spend at least a year in the field, use the local vernacular, live apart from his own kind, and above all make the psychological transference whereby 'they' becomes 'we.'" One of the few articles to explicitly focus on the practice of fieldwork in the nation of one's own birth, Rulan Chao Pian's "Return of the Native Ethnomusicologist" (1992), concluded only that such research was emotionally difficult and that the native researcher should aspire to the viewpoint of an outsider and to objectivity. This situation has begun to change in the last few years, but there are still very few explicit models for the home fieldworker to draw on.

Our experiences as trainee ethnomusicologists also provided incomplete guidance for doing fieldwork at home. As a Ph.D. student in Belfast in the late 1980s, I attended John Blacking's seminar "The Anthropology of Music," a course which assumed that our aim was to move into a feeling of proximity to previously

unfamiliar peoples and musical situations. For instance, Blacking urged us all to plan for a full year in the field so that we could document every calendrical rite and its associated music making. Yet students like me who planned overseas fieldwork were actually in the minority; most of my classmates that year were from overseas and studied traditions from their home countries, and they were often highly familiar with those musics already. Using the local vernacular during fieldwork was hardly a problem, and the period of fieldwork was a return to living with people of their own kind, a homecoming that brought with it familial and social obligations quite distinct from those that confronted a visiting researcher. As Blacking himself regularly pointed out, ethnomusicology was for these students a way to move back from focusing on musical specifics that were already deeply familiar, a means of envisioning broader concerns, musical and human alike. A third body of Belfast students worked on projects in Northern Ireland; the field was all around them every day, and field study, attending classes, part-time work, family life, and writing-up all overlapped. Blacking knew all this, and enthusiastically supported each kind of student, but his normative teaching assumptions rested on the idea of a journey to an unfamiliar setting overseas.[2]

Meanwhile, when Chou Chiener undertook her first training in fieldwork (with me) in Britain in 1997, she found her course concentrated on practical matters of research design, recording and photographic techniques, performance as a fieldwork tool, and the ethics of representation in writing. Her training no longer assumed that the researcher was necessarily a cultural outsider, but it still failed to address an important set of concerns that she had brought from Taiwan. In Taiwan, music had become an important instrument in debates between those who saw themselves as "Chinese" and those who preferred to deem themselves "Taiwanese," being widely used in expressions of self-identity and political stance. Chiener had already taken up a "Taiwanese" subject position, and had participated vigorously in pro-democracy student demonstrations in the early 1990s. She wanted her research to contribute directly to public understanding and the in-forming of cultural policy at home, but the fieldwork training course I provided offered little on how research might be directed toward social action. Familiarity with how ethnomusicology is currently taught in Britain and in parts of East Asia, Europe, and North America suggests that our experiences, while personal in their particulars, are not atypical.

If home-based fieldwork is less completely encapsulated in publications or courses, there are certainly many signs of a rise in the visibility of such work at present. Factors underlying this shift include the establishment of an academic infrastructure for ethnomusicology in many nations over the last two generations. In decolonized countries particularly, many researchers have focused on home musical traditions, whether for reasons of political and musical self-awareness or due to the typically lower costs this involves. Whatever our location, today's ethnomusicologist also faces the logistical challenge of sustaining a career for

employers who demand regular publication, full-time engagement in teaching, and sustained administrative service. Teaching and administrative work both militate against long periods of overseas fieldwork, while field study nearer the workplace can be fitted around the normal working week. A further outcome of our work as teachers has been the widespread formalization of ethnomusicological training in the academy. Fieldwork courses now appear on many curricula, at both under-graduate and graduate levels. The educational precept of learning by doing means many such courses investigate local musical resources, a pathway that draws more than a few ethnomusicologists into extended research close to home.

Meanwhile, ongoing migrations within and across borders, increased travel generally, and patterns of international media distribution leave the early disci-plinary focus on "authentic" traditions bounded by place and nationality ever more outmoded. The other now lives next door, and she hears music from around the planet at the press of a button or click of a mouse. Of course, that earlier focus risked overemphasizing difference and downplaying shared humanity, and has been vulnerable both to postcolonial critique (which identified a new round of foreign exploitation of third-world resources wherein the overseas researcher es-tablished an orientalist order that rewarded the researched only insofar as they remained colorful, exotic, pre-modern, different, and other) and feminist de-construction (which saw predatory masculine urges in the collecting, objectifying, controlling, and valuing of knowledge gained outside the home). And those wishing to travel far for fieldwork now have the striking environmental conse-quences of jet plane travel to consider. All these issues leave us much more keenly aware of what is at stake when we set out to investigate someone else's musical life; it is not surprising that home is increasingly seen as a research location in which challenges like these are, if not entirely eliminated, at least more fully understood.

Research at home also goes hand in glove with an increasing sense among ethnomusicologists that we should strive for positive social impact beyond the specific teaching- and research-related domains of academia. Although we may choose to apply our insights in overseas interventions as well, we clearly have a responsibility to contribute to the improvement of our home societies where possible. And doing fieldwork at home allows us not only to intervene productively but also to monitor the impact of these interventions over considerable periods of time (see further, Hemetek 2006).

All in all, it is increasingly apparent that "we" and "they," the ethnomusi-cologists and those whom we study, are not *normatively* separated by distance, language, wealth, or lifestyle, even while such distinctions remain significant in many fieldwork situations. Daniel Reed (2003:8) notes that "fieldwork is, in reality, just living—albeit a specially framed and focused kind of living—that does not end when we return from some metaphoric 'field.'" If this is so for an American in West Africa, then it seems even more apt when the fieldwork occurs at home, yet more intimately interfaced with the rest of our everyday lived experience.

The Intricacy of Home

We have so far used the term *home* as if there were no ambiguity to it. Outlines of some of our research topics show the complexities that emerge in actual practice. The examples extend on a continuum that stretches from fieldwork carried out close to home to that done far away from it, but it will quickly be seen that each outline contains multifaceted senses in which a researcher can be close to or far from home:

- Chiener's study of *nanguan* music in Taiwan. This is an amateur ensemble tradition which spread from Fujian Province, China, to Taiwan several centuries ago. Chiener began to learn *nanguan* in her twenties, so it was not the venue for her initial musical experiences. (She studied piano and then the Chinese twenty-one-stringed zither *zheng* and mouth organ *sheng* before coming to *nanguan*.) Nevertheless, this was a tradition she entered as a musician, not a researcher, and one that she learned while living in her home society. It was a music she adopted through the medium of her native language, and also a music marked out locally as specifically Taiwanese, this at a time when many in Taiwan were engaged in a kind of roots movement with distinct political overtones. In this movement, people like Chiener rejected aspects of the pan-Chinese identity promoted by the Nationalist Party (KMT) government in favor of cultural symbols that pointed to Taiwan's pre-KMT culture. Chiener revisited this subject in doctoral study overseas, working with the ensembles she had joined as a musician, her fieldwork built on trips back to her parents' home (Chou 2001, 2002; fig. 7.1).
- My study of English folk music sessions in Yorkshire, UK. Sessions for mixed groups of instruments (most commonly, fiddles, guitar, banjo, flute, melodeon) are fairly widespread in this region. I developed an active interest in them in the late 1990s, accompanying a graduate student to a session and participating there on bassoon, eventually—perhaps inevitably—beginning to research this subject (Stock 2004). The repertory of English folk music was essentially unfamiliar, and my already-established ethnomusicological instincts provided further distance from many others present, but I felt a sense of proximity to the group's modes of socialization, including their characteristic exchange of (supposedly) humorous banter. I blended easily into the session's predominantly white, middle-class make-up and (soon enough) into its 40-plus age profile. Musically, I was able to re-apply harmonic and rhythmic gestures acquired long before on my first instrument. There was some unlearning of classical music to be done, but much common ground, and even some sense of a return to closer musical quarters after some years spent learning Chinese instruments. Fieldwork at this kind of music event fitted readily around my usual domestic routines, and so home and field easily overlapped.
- Chiener and I are now working on a study of the daily musical lives of a community of Bunun Aboriginals in southeastern Taiwan. Bunun traditional music, especially the so-called eight-part polyphonic *pasibutbut* ritual singing,

Fig. 7.1. Chiener conducting fieldwork in Tainan, Taiwan. Photo by Jonathan Stock, 28 October 2000.

has been much studied, but little has been written about their pop bands, karaoke bars, or school singing lessons. Both Chiener and I can communicate readily with the people we have met, nearly all of whom speak Mandarin regularly in their daily lives. Chiener also shares much social context with our Bunun hosts. In discussions of matters as varied as food preparation or the popular music of previous decades, Chiener's background provides crucial common ground. As primary field researcher on the project, she is also becoming closer to them by simple dint of duration spent side by side. I myself share with many of the Bunun a background in Protestantism, which I acquired under parental insistence up to the age of 14 in England. For me now, the Bunun church praise songs can evoke nostalgia, a sense of a home and a childhood lost. Chiener, meanwhile, received a less faith-based upbringing. What existed was Buddhist in tone. To her, evangelical songs are new and foreign indeed. Overall, though, and despite our slices of shared experience and the efforts of our Bunun contacts to make us feel at home among them, neither of us would describe this as a study at home.

- My part-biographical, part music-analytical, part fieldwork-based study of the supposedly archetypal Chinese folk musician Abing (1890s-1950; Stock 1996a, 1996b). Since Abing himself died long before I first visited China (in 1989), he was himself long gone from the scenes of my fieldwork. I was a foreign researcher new to this topic in many respects, and none of those whom I met was an exact contemporary of Abing, although a small number had met him years earlier in their own relative youth. Exploring a shared enthusiasm for

Abing's music and a commonly held desire to better understand the transformation of traditional musics in the Chinese conservatory system allowed me and some of those I met during fieldwork to discover and develop affinities, but overall this project seems even further away from home than the preceding one.[3]

As this set of examples implies, "home" is as constructed as the "field." It may be multiple, as we add new "homes" to older ones as our lives progress. Doing fieldwork there activates a cluster of qualities. These include a sense of shared linguistic and interactive codes and emotional repertories, possibly allowing (or requiring) certain assumptions to remain unstated or certain modes of humor to unfold; a recognition of commonly held cultural values, expressive norms, and local knowledge; a (re)turn to deep-rooted musical intuitions and memories that predate any explicit research training or agenda; an awakened appreciation of mutual ownership, authority, political stance, and subjectivity; and the discovering of affinities based on variables such as age, gender, class, and taste. Not illustrated here, but surely significant in other cases of research near home is the possibility that those whom we study may know our relatives, or have ideas about the kind of people they must be based on profession, place of residence, or other factors. On the basis of these ideas, they may regard us not as strangers but as individuals already emplaced in a pre-existing web of social responsibilities distinct from the assumptions that greet a foreign researcher. Some of these qualities might normally be ascribed to us by those among whom we carry out our fieldwork or by ourselves in phases of reflexivity, but many will result primarily from acquired experience, such that a researcher of any origin moves toward (or away) from a feeling of being at home during an ongoing fieldwork study. Equally, some may be disputed or initially surprise us, as, for example, when field contacts first refer to the researcher as "our ethnomusicologist."

Quite a terminology has grown up around this subject. We read about "insider" versus "outsider" identity in fieldwork and see a related pair of adjectives often taken to label insider and outsider viewpoints, "emic" and "etic" (for significant distinctions in the usage of these terms outside ethnomusicology, see Herndon 1993:63–68). Then there are categories like "native researcher" (for an excellent account of its history and implications, see Narayan 1993) and the pejorative-sounding "backyard research." All these terms carry much baggage by now, and if use of each can temporarily produce clarity of emphasis, they risk reducing the often shifting and multiple identities a researcher carries during fieldwork to a single valency or position.

Some Differences in Fieldwork at Home

Our fieldwork at home stands apart from that we have done in foreign locations in several respects. These distinctions are grouped under four subheadings, starting

with reflections on our motivations in gaining access to the field. Aspects of the contrasting intensity of home fieldwork follow, with next a consideration of linguistic matters. Finally, we look at the social role of our research.

Gaining Access, Finding a Role

I never specifically set out to research English music, but as a university teacher in Sheffield (a post I had been attracted to in part due to the city's active live music scenes) I encountered a stream of postgraduates who were essentially studying home traditions, among them a part-time M.Mus. student, Paul Davenport. Paul was interested in research into the music and lives of a group of blind fiddlers who won local renown in late eighteenth- and early nineteenth-century Sheffield. Meanwhile, he ran an instrumental session at the Red House pub, which he invited me to attend.

Taking my bassoon along, I normally aimed to sit away from the center, anxious that my mistakes should not be too widely overheard. The other musicians seemed welcoming, commenting that the bassoon had a long heritage in English traditional music, even if it was little seen in contemporaneous sessions. Working primarily by aural trial and error (informed by a classical training in harmonic progressions), I soon realized that a position with good sightlines to one or other of the two guitarists normally present provided helpful visual clues as to shifts in the bass line. The other musicians never tried to instruct me as to how to play—indeed, they would strenuously deny any aptitude to do so—but they did now and then smile (or look shocked) at certain of the musical gestures I tried out, and they occasionally offered more generic comments that contained clues as to the limits of acceptable performance style, all of which I tried to learn from. I found this process of semi-improvisatory musical exploration highly appealing, and since the events were socially enjoyable as well, I soon became a regular member.

A teaching contact led to a rising performance interest, then, but my turn to these sessions as a site for fieldwork took longer. Before setting off to China or Taiwan for fieldwork I'd had first to write a research proposal that was coherent enough to persuade someone to give me a grant. A set of aims and a plan came before firsthand musical immersion, even if the plan sometimes changed mid-visit. Fieldwork at the Red House was the other way around: here I first participated and observed over an extended period, steadily developing a new musical enthusiasm, without the need to first generate research questions, plan a particular academic outcome by a set deadline, or even to keep fieldnotes after each session. I have done research overseas like this—my doctoral fieldwork in Shanghai also saw a specific research project emerge from immersion in a new musical location—but my other fieldwork in Asia since then has been much more goal-directed.

Chiener's experience in researching *nanguan* music offers a parallel but not identical instance. She studied Chinese music as an undergraduate in Taipei but found the *guoyue* genre (primarily pieces written or arranged in Chinese styles by

Western-trained composers) musically uninteresting. At the end of her studies she felt ashamed of her almost complete lack of awareness of Chinese and Taiwanese musical traditions. At this same time (the early 1990s) Chiener was active in student demonstrations against the KMT, sharing views held by her father and grandfather.[4] Commonly, there was a strong element of anti-Chinese ideology in such demonstrations, with the descendents of immigrants from Fujian over the previous several centuries seeing themselves as Taiwanese rather than Chinese. Their self-image as Taiwanese clashed directly with the claims to political authority over mainland China as well as Taiwan advanced by (non-Fujianese) KMT leaders Chiang Kai-shek and his son Chiang Ching-kuo.

Chiener was keen to find a genre that had a Taiwanese identity, not simply a more generic Han Chinese one. Night-market record stores led her to *nanguan*, and she began learning in 1992 with an amateur group. As a new entrant, Chiener was initially expected just to attend regularly and observe attentively, merely reading the notation during a practice that lasted for around three hours, a passive attendee at rehearsals seemingly not much noticed by the regular musicians. After a few months, she had been taught one simple piece. Sometimes, she was permitted to run through it at the end of the practice, if there was still time before people needed to go home. The *nanguan* musicians, it turned out, greatly appreciated the fact that she came each time and quietly observed, awaiting a possible opportunity to try out her tune. This attitude, as Chiener was subsequently informed, was one older members associated with a traditional teaching process in which the student wins acceptance only after demonstrating the behavior of a proper, serious learner, a moment that occurred in her case after six months when she was designated the role of tea server in the group. If this seemed a subservient task, she realized latterly that she was no longer seen as a generic new recruit who might or might not appear each week, but actually was now a group member with a role of her own. Gradually, she was able to take a regular performing place without requiring the other musicians' permission or special invitation. Only some five years later did Chiener decide to train as an ethnomusicologist and carry out fieldwork on this music. This existing experience and access shaped the ways in which she subsequently performed her fieldwork.

Intensity

Several aspects of Chiener's research in Taiwan point to further experiences that may also be characteristic of home fieldwork more widely. Having graduated to a more active role in performance (and in tea making) Chiener found that a dutiful learner in *nanguan* was expected to refrain from simultaneously seeking tuition or performance exposure elsewhere, unless taken along by a teacher from the group. Chiener observed this rule closely during her five-year period of group membership before leaving for studies overseas. At the same time, she observed how visiting researchers were expected to attend multiple groups (as a guest, not as a

member), irrespective of whether they knew any performance, and members of her group immediately suggested further contacts in other groups to all such visitors. Chiener's apprenticeship as a musical learner of *nanguan* was in this sense deeper but narrower than a parallel foreign fieldworker's would have been.

Returning to Taiwan for doctoral fieldwork during the summer of 1998, she encountered a further challenge that marked out her experiences from those of a foreign fieldworker. Hoping to assess how typical her previous learning had been, Chiener planned to participate regularly in three groups, fitting in occasional individual interviews as well. Loyalty led her to attend the weekly rehearsals of her own group, which were held on Mondays. The other two groups she selected met on Thursdays and Sundays, and a few senior musicians played in both ensembles. However, after a few weeks on this schedule, Chiener could tell that her parents were disappointed, especially her father, who worked away from home and only returned home for one day each week late on Saturday night. Numerous other relatives also expected to be visited during Chiener's trip home, and Sunday was then the only non-workday in Taiwan. As a result, she dropped out of the Sunday rehearsal, but some musicians at the Thursday group asked her why she did not attend Sunday's activities any more. Chiener found she could not answer; mentioning family responsibilities sounded like an excuse for not studying energetically. Foreign ethnomusicologists can face family problems, too, but the geographical dynamics are often different—the researcher may even be leaving family at home to go on the fieldwork alone.

For my part, on fieldwork in East Asia I have generally been able to devote much of each day to fieldwork. But at folk music venues in Northern England, meanwhile, I dip into the field perhaps for just one evening a week, and often take Chiener, now my closest family member, with me. I suspect that a large proportion of fieldwork at home, apart from that carried out on doctorates or postdoctoral fellowships, is carried out part-time like this in and around one's family, friends, and workplace. It sounds far from the Malinowskian ideal, but is not without advantages: studying English folk music sessions, I can shift between phases of active investigation on particular questions, analysis, writing up, the seeking of feedback, revision, and more general participation as a musician without having to worry about the impending expiry of research leave, a visa, or funding. If this makes it all sound easy and convenient, the intimacy of all this means that it may be particularly difficult to complete the study and leave a field which has become so integral to my life.

Language

My involvement in folk music sessions in and around Sheffield illustrates a third point. Although one of my initial attempts to join the banter that characterizes English sessions misfired spectacularly (interjecting "mad cow disease" midway

through a fellow musician's eulogy on rural English creativity apparently hit a nerve), I can generally follow the session's multidirectional discussions without too much difficulty. Here's an example:

> MIKE: [coming in from the bar area] Where's Paul then? Is he off on his holidays? [Chiener and one or two others play softly in the background]
>
> IAN: Yes, they're on holiday [referring to Paul Davenport and his wife] this week and next. [There's a lull in which the lack of fiddlers is joked about] I'm trying to think, [Ian strums the open strings of his guitar down and up]…have we done them, erm…
>
> MIKE: You're the deputy.
>
> IAN: I'm not the leader, not a leader.
>
> MIKE: [turning to Barry] You're the deputy leader.
>
> BARRY: [laughing] No, it's not my gig at all.
>
> MIKE: [to Ian] You're the second-oldest inhabitant. [Someone laughs]
>
> IAN: I'm not a lead player. [He takes up his mandolin and tries out patterns]
>
> MIKE: Or is Ron? I don't know. Are you the…?
>
> RON: [looking at Barry] As a member of the EFDSS [English Folk Dance and Song Society] National Council, you're the senior musician here.
>
> BARRY: No I'm not. [Inclines his head toward Ian] He's the senior musician here.
>
> TRISH: [in mock exasperation] Oh right! Well let's start something else then. We're not letting him have seniority.
>
> MIKE: [simultaneously] Don't start giving him status! It'll go to his head! [More laughter]
>
> BARRY: [gently indicating Mike] Learnt everything I know off that man there. [Ian just starts playing the "Worsbrough" and "Chatam" hornpipes] Gooseberry and Clapham! [Gradually all join in][5]

<div align="right">Fieldnotes, Sheffield, 11 April 2001</div>

Chiener has confirmed that taking in exchanges like this, with all its incomplete phrases, straight-delivered leg-pulling, unspoken cross-group references, and rhyming slang, is hard work in a second language. Indeed, she adds that this is why she does not feel ready to do fieldwork at these sessions herself, preferring to participate as a musician. I checked a transcription of the recording with those speaking to confirm exactly to whom they were referring, which a non-native speaker could do, too, though it would be a laborious process to go through a whole evening and require great patience from the other musicians. Now, it may be that many ethnomusicologists are better linguists than I am—there is little in the disciplinary literature about the challenges of fieldwork in a foreign language—but despite having spoken Chinese for almost twenty years, I know that I still catch less

in China or Taiwan than in English. For example, on my first visit to a *beiguan* (wind and percussion) group in Taiwan I found that the musicians habitually spoke Taiwanese (Holo), a dialect which is sufficiently distinct from Mandarin to be essentially unintelligible to me. I could derive next to nothing from listening to their normative interactions and so had to proceed much more intrusively by asking interview-type questions instead. This is an extreme comparison, but one can readily imagine that each of these kinds of fieldwork would lead to a contrasting ethnographic treatment, in turn.

Chiener's experiences extend this point. In her initial learning of *nanguan* Chiener kept some informal written notes, but she usually only wrote down details she might otherwise forget. These notes were made without any thought that they might later be revisited as academic fieldnotes; Chiener needed neither to record exactly what was said nor the precise date on which that information was imparted. If we might strictly call this phase Chiener's pre-fieldwork, it should also be clear that this earlier period of experience was more than a background to subsequent "proper" fieldwork following ethnomusicological precepts, and that it led to understandings that were not necessarily inferior to those gained in her later investigations. In a sense, achieving an established musician's identity seems to qualify her to become her own consultant, reflexively speaking, if not interactively. Ethnomusicologists certainly value the role of personal experience in developing a fieldwork project, but a writer's reliance on a base of experience accumulated prior to training in the full niceties of ethnomusicological fieldwork can raise questions as to how far the account represents wider realities in the society in question.

Not surprisingly, both Chiener and I have found that those we have researched have tended to take considerable interest in what was to be said about them when it was to be published locally in their own language. We have found many of our field contacts willing to provide detailed feedback on drafts, both in speech and writing. This is illustrated here by an extract of an e-mail from musician Malcolm Douglas in response to my writing on our spatial distribution in the sessions:

> On busier nights, it is sometimes impossible for me to avoid sitting more or less with my back to somebody or other. It's something I prefer not to do, but I would be less inclined now to compromise too far what I would consider my musical effectiveness (physical comfort is related, but secondary) by moving to a more problematic position in order to accommodate a late-comer than I might once have been. To begin with, I found your tendency to sit behind me even when there were less remote places available a little disconcerting, but I concluded that you had specific reasons for taking that position. (Malcolm Douglas, e-mail, 30 June 2003)

Application

Applied fieldwork is that with a primary intended output contributing social benefits, rather than the increase of original, scholarly knowledge or enhanced

performance skills. Of course, the term "applied ethnomusicology" reflects an older view of ethnomusicology as a science whose practitioners aimed not to interfere with the musical culture under study. Many ethnomusicologists now intervene overseas, negotiating the resulting ethical dilemmas as best they can, but non-intervention has always been morally irresponsible for those who work primarily at home.[6]

As noted earlier, Chiener began her studies hoping to change attitudes on traditional culture in Taiwan. Perhaps ironically, her decision in 2001 to settle in Britain means that I have so far been the one who has had more opportunities for social impact. Fieldwork in and around Sheffield has given me the contacts necessary to arrange education events, performances, workshops, lectures, residencies, conferences, and festival performances bringing people from various communities into the university (and those from the university out into the community; fig. 7.2). Several local musicians have commented on these various linkages, stating that they had assumed the university had no interest in them, that they wouldn't find a welcome there. This institutional validation is probably worth more to them than the fees that have also gone their way. The same might be said of an undergraduate

Fig. 7.2. Jonathan, bassoon, with English folk musicians Barbara Salmon, flute, and Paul Davenport, fiddle, raising funds for local railway services. Photo by Doug Salmon, 20 May 2006.

degree set up in Traditional Musics and Folklore Studies, which has recruited few students so far but seems to have been interpreted by some local musicians as a legitimation of their artistry.

Knowledge gained during fieldwork has helped me use my formal position to write a succession of references in support of grant applications great and small by activists in several music and dance traditions in this region and nationally, to campaign (unsuccessfully) against the UK government's revision of Public Entertainment Licensing, and to nominate (successfully) two representatives of English tradition for D.Mus. degrees here at the University of Sheffield. These latter, awarded to the exceptional singer and guitarist Martin Carthy and the indefatigable collector David (Doc) Rowe, are the first—and as far as I know—the only honorary doctorates ever awarded in England to folk musicians. Classical (and jazz and pop) musicians hold many, many more such degrees across the country generally, but due respect was paid to these two bearers of English tradition in front of two thousand graduates, parents, and university staff. There is, of course, a danger that I will cause harm or discord as well as (I hope) good through interventions like these, and I need to maintain fieldwork outside the academy to monitor the ripples that my actions cause over time. Of course, many university staff contribute in similar ways, but ethnomusicologists are particularly advantaged in having fieldwork as a means of developing effective interventions around their home institutions.

How Was It for You?

Since Chiener and I regularly accompany one another on fieldwork, we have been able to observe how people react to each other's presence in the field. We have sometimes also taken the opportunity to ask those whom we were researching how it was for them. Of course, doing so raises potential ethical difficulties: it could easily appear that we are seeking out reassurance for ourselves or negative commentary on the work of others. Still, in speaking to some of those whom we have researched, we were assisted by the fact that several have become close friends over the years, and so both understand our work well and know us well enough to be able to speak (relatively) freely. A number have either formerly or subsequently been studied by other researchers, and so speaking about those experiences allows a way into the topic more widely. A few have also had some academic training in folklore, ethnomusicology, or one of the social sciences, and so are accustomed to such questions. The following discussion with several of the English musicians referred to earlier illustrates this point:

> JONATHAN: You've been studied twice now, once by me and then by Shu for her M.A. What was the biggest difference for you those two times?"

KEVIN: Well, Shu took us in the other room one by one and interviewed us, whereas I didn't know you were doing it. You just wrote it up and showed us the draft. [Laughs]

JONATHAN: How about the fact that she's young, female, and Chinese whereas I'm older, male, and English?

KEVIN: [Pauses] A difference was...I asked her questions too. Like what were sessions like in China. I didn't ask you that....

BARRY: Shu is very engaging, so it's easy to chat to her. And it is interesting to discuss these things with someone of a different musical background....

PAUL: [Later] When Isabel came to research me she had a plan and a structure. She had some set questions, and even had some particular tunes she wanted to get me to play. She was doing a comparative study, you see, and wanted the same tunes from each of her fiddlers.[7]

Fieldnotes, Sheffield, 27 September 2006

These remarks raise several points related to doing fieldwork at home. Kevin and Barry, for instance, mentioned the pleasure they took in learning something from the overseas researcher—many of us will recall having received special attention as exotic others in our own overseas fieldwork. Similarly, when Chiener once brought her *sheng* to the English sessions, the musicians, who knew of it as ancestor of English free reed instruments, were delighted to actually touch one and try it for themselves; none has yet expressed an inclination to try out the bassoon. Meanwhile, both Kevin and Paul drew attention to the more intense research agendas of the two visiting students. Each needed to complete a dissertation within a relatively short period of time. In my case, I attended sessions as a musician for four years before finding an issue to write about.

My friends' comments lead me to wonder if I have been overly cautious in typically not leading research-related discussion or staging interviews at the sessions. I might justify that caution by citing a wish to observe the week-to-week flow of the event rather than transform it into something else through my intervention, perhaps even disrupting other individuals' musical enjoyment. As noted already, as both a native speaker and a regular attendee, it is easier for me to draw inferences over time than it would have been for either Shu or the student visiting from London. Interviews are a departure from the interactive norms that characterize English sessions, but the musicians may indeed interpret formal interviewing as a sign of serious regard for their opinions. Perhaps I have actually denied my fellow musicians a set of conversations they might have enjoyed, and left them unsure whether I actually cared about what they thought?

More specifically, Kevin's words here hint that I have left my fellow musicians wondering, perhaps uncomfortably, exactly what information I was storing up for later use? Following up on the e-mail quoted earlier, musician Malcolm Douglas

commented to me that he assumed I sat where I did the better to watch everyone else (p.c., Sheffield, 2 July 2003). Georgina Born's study of composers and music technologists at IRCAM in Paris provides a parallel instance where the invisibility of note-taking raised challenges for some of the researched: "even my intellectual informants had difficulty at times conceiving what I might be doing or bearing in mind the 'double' nature of my presence. As one informant and friend said, 'I never know when we're talking if we're simply talking, or whether you're going to go back home and write it up as notes'; to which I could only reply, 'both'" (Born 1995:8, 9).

Chiener has a particularly poignant example of such a clash of expectations. Her *nanguan* tutor, Lim Yinsu, had been very keen to train her as a great performer before she went abroad. When Chiener returned to the group on doctoral fieldwork in the summer of 1998 and spent much time interviewing people but little in performance, he was disappointed, stating: "Miss Chou, do you know that without learning more repertory you won't be good at research?"[8] In August 1999, Chiener returned to Taiwan for further fieldwork. Lim looked thin and weak—at that time he was seriously ill with cancer, and had just finished a phase of chemotherapy. One day, he very impolitely scolded her, saying, "It isn't worth teaching you! It is like sprinkling water on the ground." Chiener assumed that his worsening health was forcing him to take a stricter attitude than before and tried to explain: "I am sorry about my musical progress, but my writing will help other people know about *nanguan* music." Musician Chuang Guochang, who was standing nearby, perhaps had a better understanding of the situation. He said to Lim, "She will come back, don't worry."

Thinking over Lim's action later, Chiener realized that he was desperate to transmit his music to someone who could take it as a whole, and who could then be recognized as his great pupil. She was not the only one he had considered for this role, but the other candidate, also female, married an American and moved to the United States in 1998. The metaphor of sprinkled water comes from a saying that "a married daughter is like sprinkled-out water," one of several that indicate that daughters, who according to Chinese tradition are held to marry outside the family, are a poor investment for Chinese parents. This clearly indicates Lim's view that his pupils were his inheritors. He regretted that he could not find a man to teach, because a woman will belong to her husband's family and perhaps move away from the group or give up attending in order to care for children. In Lim's view, performing music was of a greater priority than academic study, and he was frustrated that Chiener seemed not to value the inheritance he was willing to bestow on her.

These examples show that the researched form expectations of us as researchers that are partly but not entirely shaped by our nationality. Gender, age, and marital status may be just as significant. Clearly, even those of us who carry out fieldwork in our home societies cannot take it for granted that our research aims

and methods will be well understood by those around us—in this, fieldwork at home is very much like that carried out elsewhere. However, the imperatives of overlapping identities—inheritor of tradition and academic researcher in Chiener's example—can be particularly pointed in home fieldwork situations, for fieldworker and "fieldworked" alike.

Conclusions

Doing fieldwork at home contributes to a more rounded ethnomusicology where all music making is treated as genuinely worthy of study, not just the far-distant. Fieldwork like this is also rich in opportunities for cooperation. Our respective studies at home have facilitated each other's entry into a different world of music, allowing us to accompany, bridge, explain, interpret, and sometimes to translate, not only for one another but also for others present. A married couple of differing nationalities, our home fieldwork might be seen as a means through which we each domesticate the other partner while simultaneously gaining a position from which to rethink our own approach to fieldwork overseas.

In fieldwork at home we take on roles and responsibilities that extend well beyond the production of learned papers and lectures. The exact distinctions explored in this chapter—in motivation and role, intensity, language and feedback, and in chances for application—may not occur in every home study, but where they do they seem likely to inspire ethnographic writing in which we compare our early impressions as musical learners with those added later as researchers, reflect on the intricacies of shuttling between one local role and another, respond to the needs and criticisms of native readers, and strive to contribute to and monitor projects directed toward positive social change. Managing all these is a fascinating and worthwhile challenge, indeed a way of living—a means of building new musical homes—which many an ethnomusicologist will find personally rewarding.

Notes

1. We are grateful to Lindsay Aitkenhead, Richard Jones, Simone Krüger, and Marin Marian-Bălaşa for their feedback on drafts of this chapter.

2. Witzleben (1997:236) offers observations that can be paralleled to this, discussing problems generated when a US model including a demand for bi-musicality from all students was fitted onto the educational reality in Hong Kong, where most students were of Chinese origin and wished to research Chinese musics.

3. Other researchers' work adds further examples to the list. For instance, ethnographers commonly include web and e-mail study today, researching from their homes and offices (see, for example, Wood 2001). Meanwhile Andrew Killick (2006) has recently noted the scope for the study of "holicipation," music making engaged in for solitary pleasure. And there is Judith Okely's (1996) "retrospective fieldwork," by which she refers to the revisiting of autobiographical experiences.

4. Taiwan was granted to Japan by the Manchus in a Sino-Japanese peace treaty of 1895. It was handed to the KMT-led Republic of China when Japan was defeated in the Second World War (1945). The KMT lost control of mainland China to the armed forces of the Chinese Communist Party in 1949, retaining only Taiwan.

5. Musicians named here are Mike Wild, Ian Spafford, Barry Callaghan, Ron Day, and Trish Bater.

6. The literature contains examples of the social impact we can achieve through carrying out fieldwork at home. Mellonee Burnim (1985:445), for instance, notes how we may become role models: "The cultural insider who conducts field research is commonly viewed as a contributor, rather than an exploiter. The insider has the opportunity to become a source of cultural reinforcement, and as a by-product, a source of cultural pride." Kay Shelemay (1997), meanwhile, shows that this role can also be ascribed to non-native researchers, through dint of their intense engagement with a tradition.

7. Jiang Shu and Isabel Watson have agreed that their names can be given. Those musicians commenting here are Kevin Marshall, Barry Callaghan, and Paul Davenport.

8. Lim knew Chiener's personal name but always called her Miss Chou. Use of her full name would have been too formal and use of her personal name over-familiar.

Working with the Masters

> The guru-shishya paramparā thus provides for the inheritance of a musical tradition and often is the cultural model of the natural relationship between father and son. The disciple learns not only the craft, but also the trade secrets. He learns how to behave as a member of the artist's community and how to become a professional performer on stage—and accordingly learns not only the art of making music but of being a musician.
>
> Daniel Neuman, *The Life of Music in North India*

The Problem of Discipleship

As I prepared to travel to India where I would undertake fieldwork for the first time, I remember being strongly influenced by Daniel Neuman's suggestion that I could get all I needed for my dissertation on the hereditary *tablā* players of Lucknow simply by becoming like a son to a great master. More than that, I was enthralled by the tablā and was especially keen to become a good player. I had prepared myself by taking lessons for two years from Manikrao Popatkar, an excellent Maharashtrian player who had settled in London. However, there, surrounded by all the cozy familiarities of my native land, I convinced myself that this wasn't the real thing and that only complete cultural immersion and the participant observation method promised to provide answers to the questions about Hindustani music and musicians I had at that time barely even begun to formulate. Finding my own *guru* or *ustād*, I thought, would be easy. I was wrong.

In the years before mobile phones and the internet came to India, or even functional landline telephones for that matter, it took me literally weeks to locate and meet the doyen of the Lucknow tablā tradition, Ustād Afaq Husain Khan. And when I eventually showed up at his house one pleasantly lazy January afternoon in

1981, I was greeted not by the warmth of interest I had expected but rather by remoteness and suspicion. Why did I want to learn? How long had I come for? What would I do once I left India? My ingenuous though honest answers, I came to realize, did me few favors and prompted yet more searching questions: what exactly would I be "researching"? If I really loved tablā so much and wished to learn it, why had I planned to stay for little more than a year? Would I ever return after that, or would I veer off to conduct my "research" elsewhere? My answers (or were they excuses?) quickly dried up. This rude awakening marked my entry into the colorful and complex world of the ustād.

An ustād is a master performer and teacher of a solo tradition, usually one that demands extraordinary technical virtuosity. As in all other walks of life, there are geniuses, charlatans, saints, and rogues among that special category of musicians called ustād. Our encounters with these masters may at times be musically and spiritually elevating and enriching; they may also be rather less than fulfilling when interpersonal, artistic, or research expectations are not met. Unsurprisingly, ustāds also have their own expectations; these may be satisfied in the form of a diligent and loyal seeker of musical knowledge just as easily as they may be dashed by one who fails to dedicate himself or herself fully to the task. Of course, not all guides or seekers experience the vagaries these extremes might suggest, but what is abundantly clear is that this form of teacher-student relationship involves a deep, personal commitment from both parties if it is to have any hope of bearing fruit. It is in this spirit that we submit ourselves to a peculiar type of asymmetrical relationship in which we cannot participate as equals, or share our songs and stories around campfires, or enter into a mutually revelatory dialogue. Instead, the cultural expectation of ustāds is that they are the omniscient masters and we their disciples who, like innocent children, must accept unquestioningly what we are told if we are to progress along the path to understanding.

It is important to distinguish between ustād (social category) and Ustād (honorific title). First, in their Persian, English, and Indian vernacular texts, writers in the nineteenth and early twentieth centuries commonly referred to "ustāds" when invoking the social category of hereditary, professional musicians in northern India. Although the term had nonsectarian connotations at first, over time it came to signify the legions of Muslim occupational specialists that were organized socially into exclusive and largely endogamous family units that served to protect their trade secrets and provide an environment that fostered extraordinary musical and technical ability. As such, the term gradually took on pejorative connotations, especially as widespread resentment grew toward the ustāds' perceived stranglehold over what was beginning to be thought of around 1900 as the cultural property of all Indians. Even more pointedly, as social mores began to change, the professional association of certain lineages of ustāds with a seedy underworld of dancing and the sex trade branded them with a stigma that still persists today. Indeed, *ustādjī*, which grafts onto the word a Hindi suffix of respect,

is not only a polite and endearing form of address for a musician but also a commonly used moniker for a pimp or criminal. When I first began looking for rented quarters in Lucknow, my close association with ustāds was a determining factor in being denied certain apartments. Landlords were concerned that the comings and goings of ustāds, not to mention the sounds of the tablā drums filtering down to the street below, would signal the presence of a brothel. The risk of some kind of impropriety and the potential threat to a landlord's honor were simply too great.

Second, Ustād is an honorific title for those in the Arabic, Persian, Central Asian, and Hindustani music cultures that have mastered a skill or craft and who, in the absence of an independent written canon, embody their traditions (Schippers 2007:124). One suspects, however, that its use has burgeoned rather indiscriminately over the past fifty years or so as displays of pure virtuosity at the expense of musical substance have become increasingly the norm. It is a common source of irritation for older musicians and connoisseurs alike when a young musician has the temerity to call himself Ustād in his publicity materials, or is introduced as such on stage (see Silver 1976:44). Mastery, I was often told, comes with maturity and in the form of recognition from other masters and seasoned connoisseurs, not by self-ascription or popular consent, no matter what degree of technical wizardry is on display. *Ustādī*—the quality of being an Ustād—comes legitimately to those who pass on their traditions (here invoking its Latin source *traditio*, meaning "to hand down"). Indeed, it used to be commonly understood that a musician was entitled to be called Ustād if his disciples themselves had disciples.

The maintenance of tradition as a shared body of skills and knowledge therefore requires ustāds and their disciples to be active links in the chain of musical transmission: at least three generations are held to be necessary for the chain to have structural integrity.[1] Such a chain is called a *silsilā*, a concept borrowed from the Sufi tradition in which a master passes on his spiritual teachings to successive generations of disciples. An ustād's disciple is a *śāgird* (pronounced shagird). There are functionally comparable Sanskrit terms for Hindu musicians and their students: a guru is a teacher, a *śiṣya* (pronounced shishya) is his disciple, and their chain of connection is the *paramparā*. Stephen Slawek (2000:457–459) invites us to contemplate the deeply rooted spiritual legacy that exists in the ancient bond between guru and śiṣya, and the almost godlike status of the guru as musical mentor and guide. As we shall see, the ustād-śāgird relationship in India has borrowed certain customs from its Hindu counterpart just as it has done from Sufi practices. Nevertheless, what both have in common, in an ideal sense, is a system where the master becomes the complete role model for the disciple not only in terms of the transmission of musical understanding and the technical means to perform it but also in terms of moral and ethical integrity, self-realization, vision, and personal depth. Most would agree that individuals capable of revealing the

musical-spiritual path are rare. Perhaps the most elegant example has been pro-
vided by Jean During, whose study of Ostād Elahi (*ostād* reflects the Persian
pronunciation) reveals an exemplary human being and master of the Kurdish
tanbour whose music became a vehicle for discovering mystical truths and at-
taining divine love (During 2003).

As my initial and somewhat hostile encounter with Afaq Husain had dem-
onstrated, discipleship and mystical truths were still some way off for me. Once I
had recovered from my inaugural interrogation, and had overcome the obstacles to
finding somewhere to live, it was time to begin taking lessons. I suspect that the
promise of additional income rather than any desire to facilitate my naive ambi-
tions to learn music and conduct research led to my official audition. "Why don't
you play for me," Afaq Husain began. I had been practicing hard to prepare for this
moment; I placed my drums carefully on the floor, made some minor adjustments
to the tuning of the skins, took a deep breath, and opened with my best shot: a
theme that could not possibly fail to merit praise. "That's enough!" I heard the
ustād say. I was stunned. I was barely five seconds into my performance. Surely he
needed to hear more than that! "No, I can tell from what you played that you need
to change everything. You need to start over from the beginning." I felt disem-
boweled. Perhaps I could have walked away at that instant with some of my pride
intact, and yet there was something so utterly authoritative and sincere about the
man who would become my mentor and guide that I was willing from that point
on to follow him and subject myself to the hardships that would inevitably follow.
That was the moment I realized I was merely a novice in the presence of a master—
the child in a father-son relationship.

Even someone better prepared than I was can be tested and turned away: the
accomplished *sitār* player Allyn Miner related to me her encounter with Ustād
Mushtaq Ali Khan, from whom she sought to learn some compositions. After
hearing her play, the ustād told her that she was not ready to understand what he
could tell her! To prove his point and establish his authority, he lifted a tiny corner
of the veil over his knowledge to reveal how one note ought to be enough to
identify an entire *rāg*. Miner recognized immediately that the single, delicate note
he played bespoke Rāg Multānī in all its essence: a fleeting flat second whose
intonation and articulation distinguished it from all instances in other rāgs. Thus,
if so much could be referenced in just one note, how could anyone possibly hope to
understand the complexities of larger melodic structures without becoming a
disciple and undergoing years and years of training?

Following my audition, progress toward discipleship required a period of
probation of indeterminate length. The tablā legend Ustād Ahmedjan Thirakwa is
said to have made one student sit quietly in the corner of the room and listen to
others' lessons for a full two years before allowing him to touch the drums. The
student's patience and devotion was duly rewarded, and in any case a great deal of
knowledge had already been absorbed through keen observation. Some never make

it. Others learn "behind the curtain"—a euphemism for any kind of indirect learning that in the past involved surreptitious observation but now often relies on recordings. Yet the importance given to musical inheritance in India means that, despite recordings, books, and music degrees at schools and colleges, entering into the master-disciple relationship is still regarded as the sine qua non for true musical understanding.

My own probation—an extended period during which my character and ability were observed and tested—lasted about nine months. During this time Afaq Husain did not allow me to do my "research," which, to him, meant asking senseless questions about musicians' social organization that would not make me a better player. He allowed no recordings to be made, neither of our informal conversations nor of his playing. He merely demanded *riyāz* (practice) and *mehnat* (application, hard work). At first I took about three lessons per week, mostly at my house, sometimes at his single-room dwelling. The latter was far from ideal because the women of his household practiced that form of female seclusion known as purdah, and I was discomfited by the knowledge that my presence restricted their movements and forced them out into the small rear courtyard where they hid from my view. As I now read back over my fieldnotes, I see many entries such as "AH didn't show up today, again!" and "AH suddenly turned up unannounced and gave me a superb lesson!" At first I wasn't sure if this was a cleverly designed strategy to keep me off-balance and test my resolve, or an indication of his lack of enthusiasm and commitment. Either way, test my resolve it certainly did, and it left me feeling I always had something to prove. Hints of his affection or concern for me in those early days were not unknown, however: "AH arrived unannounced this morning with a tin of powder which he started shaking all over my bed. Bedbugs, apparently. I told him I didn't have any, but he thought it best to be safe. Had an unexpected lesson."

The pattern I now see emerging in my fieldnotes suggests to me that Afaq Husain was exerting subtle control all along, and that he was testing my discipline and resolve. After all, maintaining discipline over one's disciple equates to authority, without which nothing can be learned. I noted that my ustād was annoyed with my lack of focus, exasperated by my inability to invest in the quantity and quality of practice he demanded (fig. 8.1). Yes, he acknowledged in a paternal sort of way, I was a nice boy always willing to do the odd jobs he asked of me, such as taking him on my motor scooter to and from the Lucknow television station where he worked as a staff musician; however, he also sternly lectured me about sticking to the task of practicing tablā instead of rushing off to Delhi or Calcutta to meet and conduct interviews with other musicians. I well remember a glorious Urdu verse of Mir Anis that he asked me to ponder: it suggested that a wise man is content to move within the limits laid down for him, just as a bead can only move along the string of the rosary. Clearly, I had not realized or accepted my limitations, either personal or practical, and yet the path was all too clear. My own
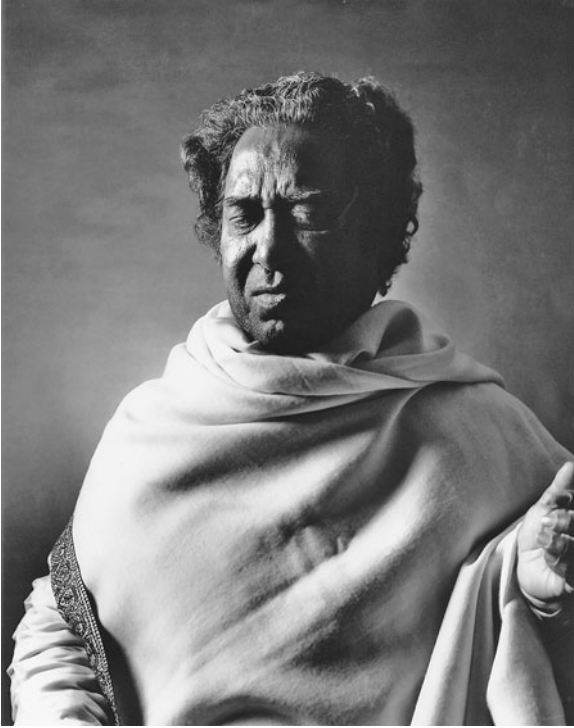
Fif. 8.1. Ustād Afaq Hussain (1930–90), who inspired awe in his students as they waited for him to emerge from these musically meditative moments. Photo by Gilles Bourquin.

cultural-linguistic heritage had taught me that "disciple" and "discipline" were derived from the same Latin root; if I hoped to learn (*discere*), then I needed to train my mind and character to attain the obedience and ordered behavior implied by these terms. From that moment I renewed my dedication to diligent practice. The results were apparent in my increased level of comfort with the radically different technique I had been struggling to come to grips with, and they were even more markedly discernible in the quality and character of sound that I began to produce. My new sound pleased Afaq Husain greatly, and my new tablā "voice" began to communicate with him in the way he wanted. From that point on, my relationship with him began to change. Soon I was ready for the all-important rite of passage to discipleship.

In that rite of passage, the *gaṇḍā bandhan* ceremony, with its mixture of Muslim and Hindu ritual, the master ties a thread around the wrist of the disciple. The thread symbolizes the new bond that transforms the student into a disciple. The act is witnessed by other disciples and members of the ustād's family, there are ritual offerings of incense and sweets, prayers are said, a gift of cloth for new clothing is made to the ustād, and an appropriate *nazrānā* (honorarium, but not a "payment" as such) is offered. The disciple takes a symbolic lesson, often involving just a few strokes or a simple composition. The disciple then plays a little, followed

by some of the more established disciples, ending with a short performance by the ustād himself. Though exhilarating, the ceremony itself was nothing compared to the interpersonal and emotional changes that were to follow: the ustād's love flowed, the women of his family were no longer in purdah from me, and my fellow disciples began to show a heightened level of friendship and allegiance toward me. This was my new family; I could now call my fellow disciples "*bhāī*" (guru-bhāīs are "brothers" under one guru); I had become like a son to a great master.

Brian Silver's ustād, Ghulam Husain Khan, described the essence of the relationship between master and disciple once the thread has been tied: "The ustād gives his pupil the maximum personal attention possible. He spends from five to ten hours a day with him, until the disciple understands the mind as well as the movements of the teacher. The teaching of music is the creation of a complete understanding between the two" (in Silver 1976:38). Daniel Neuman's ustād, Sabri Khan, insisted that knowledge is something that can only be attained through love and devotion: "These things cannot be imparted through money. . . . But you can buy it with your heart, love and care of your teacher" (in Neuman 1980:47). Devotion, respect, and caring for the master are demonstrated through <u>khidmat</u> or <u>sevā</u> (service): one greets him by touching his feet, sees to every aspect of his comfort and enjoyment, and shows complete obedience to his wishes. Slawek (2000:458) has provided some remarkable guidelines for honoring the guru drawn from Asvaghosa's "Fifty Verses of Guru-Devotion," written in the first century B.C.: in an ideal sense, the guru is next in importance to God, and any form of disrespect not only terminates the relationship but also courts disaster.

What I encountered from my ustād following my transformation into a śāgird was an enhanced level of training wherein the trade secrets began to be revealed: techniques, compositions, and wisdoms and poems offering moral and spiritual advice. Although some aspects of my formal lessons were retained, the best information and the choicest music came at unpredictable times and in informal situations—while ferrying my ustād by motor scooter, relaxing at a tea stall, or passing the day on a train ride—after all, I was now spending a lot more quality time with my ustād, and I was more likely to be on hand when his creative mood struck. Moreover, all prior objections to my "research" evaporated: I could at last ask those senseless questions about musicians' social organization . . . and get some answers! But in any case, like Thirakwa's student, I realized I'd already absorbed so much by just listening and observing all those months.

The Problem of Perspective

On the one hand, the relationship I have described is a sublime condition based on the master's love and the disciple's obedience; on the other, it is a power structure that infantilizes the disciple and creates near total dependency. Moreover, were I a woman I would certainly have had a different perspective on such a patriarchal

milieu. As a purely musical being, I realize that my hard-earned discipular status had given me privileged access to the cultural inside—to a share in a body of musical esoterica. Yet in many senses, the ethnomusicologist in me remained outside looking in, or perhaps it would be more accurate to say that I occupied a liminal space that allowed me to go back and forth between my experiences of a musical life and my need to quantify and analyze it as part of my ethnography. It is this extra dimension of liminality that differentiated me from my guru-bhāīs who, much to our master's relief, tended to have the kind of unswerving focus on music that I could never share for too long since I also had research to do and a dissertation to write. As Slawek's relationship with his gurus changed from disciple to disciple-researcher, he found himself, as I did, continually renegotiating the boundaries of what was deemed acceptable behavior so that he could pursue new areas of knowledge (1994:13).

The pressing question, then, is how does one maintain independence of thought and action in the face of an overwhelming authoritarianism that governs one's pursuit of socio-musical knowledge? It is the disciple's loyalty and obedience that is prized above all, and so even when it is deemed acceptable to ask general questions of an ustād, as I have suggested it was only after I had proved myself and earned his trust, it is still largely unacceptable to enter into a dialectic on the specifics of historical and social issues relating to music. This was a great source of frustration to me, much as it has been to colleagues who have had similar struggles with the rather awkward paradox that we are incomparably more ignorant and yet in many ways a great deal more worldly and broadly knowledgeable than those from whom we learn. Within their narrowly defined worlds they reign supreme, but they often have limited experience of the universe that lies beyond and very little understanding of the wider social, historical, and musical contexts in which we situate our studies. For instance, it was difficult or impossible for me to challenge Afaq Husain's view of Hindustani music history, with its anecdotal fictions that may have contained grains of truth, or his temporal concatenations of events and people that were hundreds of years apart. It was imprudent to delve, for example, into his family's social status and their connections to an insalubrious past whose revelation would have been socially and economically disadvantageous to him. News that I had visited or interviewed someone without his permission (which in many cases he would not have given), especially a musician from another tradition (who likely concealed things from me anyway owing to my allegiance to my ustād), aroused Afaq Husain's suspicion and anger. Moreover, it was hard for us to talk meaningfully about music outside of his tradition. All these things, and more, were relevant and important to me because, obviously, they would enable me to contextualize what I was learning and to tease out a more credible and nuanced cultural history. They were merely confusing and dangerous distractions to my ustād: a "simple man," as they say, whose unworldliness was palpable. Although I never intended to be disloyal or to deceive, my responsibility to my

research and to my academic discipline meant that my actions and ideas were at times bound to be perceived as disloyal and deceptive. On the other hand, my responsibility to my ustād as an individual—and the need to maintain trust in the relationship—meant that at times I had either to scale back my research expectations or struggle silently with the moral dilemma that I could not reveal to him everything I did during my fieldwork. I shall return later to this issue of conflicting loyalties.

I take issue with those who suggest that fieldworkers should purposely remain individuated in the research process because they are by design always destined to be outsiders (see, for example, Srivastava 2004:25). The ustād-śāgird relationship simply does not work that way, and in a cultural sense the śāgird must be accepted as an insider to a significant degree in order to be permitted access to the rarefied training whose purpose is to make him a fully functional representative of the tradition. Tim Rice (chapter 3) has provided ample evidence that the insider-outsider dichotomy lacks the subtlety to describe the complex dialogic relationships that develop between us and our teachers. In the master-disciple relationship, we encounter an asymmetrical power structure that turns the old colonial model completely on its head. It is a structure that does not invite the kind of reciprocity Bruce Benson (2003) insists is the key to a "truly equitable dialogue," at least not until one has spent many, many years becoming, metaphorically, an adult within the tradition. Until then, dialogue is limited, and the ustād's monologue rules. It is also a structure that demands a qualitative and highly personal approach, which by definition can never be objective. I agree with Robert Aunger (2004:15–16) that ethnographic objectivity is impossible anyway; however, his solution—a kind of methodological situationalism which requires that the analytical method take into account as many elements of the data elicitation process itself—is, in my view, untenable here because one cannot reduce an ustād's expertise or worldview to sets of multivariate statistics (he suggests changing the interviewer and the circumstances of the interview), set them in reflexive motion, and eliminate "spurious influences on primary data." Ustāds would never submit to that degree of control, nor would they permit other interviewers the same access to their knowledge. And in any case, who decides what is spurious?

On the other hand, if we are to embrace our subjectivity, and if our key method is to be our deep involvement in and experience of being musical in the presence of a great musical being, then how can we inhabit our subject without falling into the trap that so many authors do—Indian as well as non-Indian—of producing hagiographies that lack scholarly rigor? Actually, there is nothing intrinsically wrong with a hagiography. I have enjoyed reading accounts of the lives of many great musicians of India, although I am aware that these often reflect a decidedly uncritical and formulaic consciousness of genius that is better understood when one has worked closely with the masters. One simply has to know how to read a hagiography and how to decode and, moreover, rebalance it by means of

a rational, historiographical critique. The optimum approach (I hesitate to use "solution") to the problem of revealing a complex socio-musical being, I believe, has been provided in the hagiology, not hagiography, of Ostād Elahi by Jean During. Fundamentally phenomenological in its outlook, *The Spirit of Sounds* dismisses "objective" observation in favor of lived experience: the "intuitive perception of an essence" (2003:18), in this case, the sacred as it is manifested in the life and musical art of Ostād Elahi. It is an approach that attempts to preserve as much as possible the aura and mystery of a master's lifework through Elahi's own words, as well as in the testimonies of those that knew him. During explains (2003:203):

> To "preserve" does not mean to keep hidden or, on the contrary, to deliver in one bundle the whole of [Elahi's] thought and personality in all its richness and subtlety. It means to fine tune an image of this totality, according to the possibilities of perception or the cognitive frames of those for whom this image is intended, in such a way as to guarantee the accuracy of the portrait.

During is careful to allow only minimal interference from theories and models that have a tendency to exhaust the subject through "excessive illumination" (p. 20), or that demand its reduction to physical, psychological, or sociological facts that are intrinsically uninteresting to those involved in it. The drawback, perhaps, is that the implicit trust During places in the testimonies as a fundamental condition of his work may not easily be transported to studies of other masters. I will explore this issue in the next section.

The Problems of Exploitation and Manipulation

I think I was very lucky in finding an ustād who was basically a good person: not perhaps an enlightened being on another spiritual plane like Ostād Elahi, but sincere, honest, and humble to the point that he felt uncomfortable at overt displays of devotion toward him, such as the touching of his feet or excessive praise. I never felt financially exploited, notwithstanding the fact that my monthly honorarium to him seemed invariably to run out by mid-month and needed a healthy supplement: indeed <u>kh</u>idmat—service to one's master—demanded that I did all I could to help alleviate his financial pressures when need arose, especially as I was wealthy by his standards. Many have shared my luck, though others have been less fortunate: I know of many instances where charlatans and rogues promised much but gave little; where disproportionately large amounts of money were demanded as part of a professional agreement and yet <u>kh</u>idmat was also expected. Schippers quotes one irate musician as saying "Many gurus live in the twenty-first century, jet-setting around with their electronic toys, but they expect their students to live in the nineteenth century" (in Schippers 2007:124). Less scrupulous teachers have taught "with a closed fist" (Slawek 2000:463) and have engaged in wily games of *ustād-bāzī* in which they affected sincerity but exploited

students' loyalty to their own social and economic advantage. In the case of some female disciples, it has been sickeningly common to learn that sexual and emotional advantage had also been taken. This is the primary reason that middle-class Indian parents keen on a musical education for their daughters do not trust ustāds as a whole. Moreover, ustāds and gurus who settle or teach for periods in the West often carry their habits with them, good and bad. Clearly, whether or not one gets a good ustād all boils down to the quality of the individual behind the expertise. As the erudite and philosophical guru Bimala Prosad Chattopadhyaya wrote, "If the teacher is good, no obstacles will be thrown in the learner's way which are beyond his slowly unfolding powers to overcome" (1967:18). To my mind, this implies social and moral obstacles just as much as it does musical ones.

I know of many other more insidious forms of exploitation and manipulation that strike at the heart of the research enterprise itself by threatening to destabilize its integrity. In many instances, one can see how the asymmetrical power structure leans heavily on the ethnomusicologist's sense of loyalty as a disciple to elicit a comparable degree of loyalty as a scholar. I shall give two examples of this phenomenon.

In this first example I avoid naming names so as not to compromise the interests of the colleague who told me the story. This person shares with me a fascination for historical treatises on the music of India and for what they tell us about the changing socio-musical environment over the past 300 years. Our work in this field often piques the interest of our masters: I well remember the gentle curiosity with which my ustād greeted the news that the names of some of his ancestors had survived not just in family lore but also in Muhammad Karam Imam's Urdu book on music and musicians, *Ma'dan al-Mūsīqī*, written in the 1850s. Significant problems have arisen, however, when scholars are pressured by their ustāds and gurus to provide a justification for contemporary lore that is simply not evident in the historical record. My colleague's master sought to locate a name, or adopt one, or even invent one that would have served to situate his lineage in a place and time when his family's instrument was widely thought to have been invented. Had he succeeded in persuading his disciple-scholar to publish a fabricated account, it would have historicized the family's claim to a deeper and more prestigious lineage than any other, and it would also have lent authority to members' assertions of stylistic authenticity. It is sadly ironic that the responsibility to establish an ethics protocol for research with human subjects lies exclusively with us and does not extend to our informants.

The second is my own anecdote, for which I take full responsibility. One phase of my field research involved several interviews with the maestro Birju Maharaj, doyen of the Lucknow *kathak* dance tradition and, at that time, director of the Kathak Kendra in New Delhi. One particularly fascinating discussion took place in his family's home in Lucknow beneath the portraits of his illustrious ancestors, including his great-uncle, Bindadin, who, it is widely believed, danced as a child at

the Lucknow court of the hedonist Wajid Ali Shah, who was deposed by the British in 1856. My interests resided in the connections between Lucknow kathak and Lucknow tablā, and in the purportedly close relationship between the families of Birju Maharaj and my ustād, Afaq Husain. To this end, Birju Maharaj supplied me with many stories, one of which concerned my ustād's maternal grandfather, Abid Husain. I reported this story in my book *The Tabla of Lucknow*, to provide an amusing insight into the character of Abid Husain and his relationship with the brothers Bindadin and Kalka Prasad:

> The three used to practise together a great deal at Bindadin and Kalka Prasad's house, where Abid Husain was welcomed like a member of the family. They loved to play jokes on each other. For example, Bindadin, a Brahmin, often prepared an exquisite dish of lentils which he served to Abid Husain with a large wooden spoon. Abid Husain, a Muslim, would purposefully lift his bowl so that it touched the spoon and thus 'polluted' the lentils. Consequently, Bindadin and his brother could not themselves partake of the food and were forced to give it all to Abid Husain. (Kippen 1988:77)

Of course, I must also take full responsibility for choosing to admit this anecdote to my ethnography, much as I must acknowledge the uncritical manner in which I presented and discussed it. After all, it came directly from someone I respected greatly; someone I honored as a guru as well as a musical and artistic genius. However, I came to know subsequently from myriad sources that within the inner circles of the kathak world "everyone knows" Birju Maharaj's family is not Brahmin by caste. Only no one says so out loud. So what began as an innocent tale about a practical joke became, not only in the oral tradition but also in the scholarly record, a hard fact that validated the family's claim to elevated social status. Moreover, the reification of such assertions in print also allows for their spread through any literature that cites the original anecdote, and soon enough the originators of the claim can point to the overwhelming weight of supporting evidence. But why should caste matter in this case? It matters because Birju Maharaj's family identity is Kathak, and Kathaks claim to be the modern inheritors of an ancient tradition of story-telling Brahmin priests that roamed India more than 2,000 years ago. Furthermore, it is claimed that they danced their way down through the ages in the courts of kings, and that they taught Wajid Ali Shah to dance in the early nineteenth century. Kathaks believe that this pedigree gives them proprietorial control over the dance genre *kathak*, a term likely invented by them in the early twentieth century; they believe they are the ultimate authorities to whom all must pay homage and from whom all should learn. Aspiring dancers should never think of taking to the stage without the sanction that stems from at least a year's "finishing school" with Birju Maharaj, even if they studied with another teacher for most of their lives. The claim that begins seemingly innocently in caste status, then, is tantamount to hegemony over an art form.

The work of Margaret Walker into the history of kathak dance (2004a) has shown that Kathaks were actually low-caste entertainers in the early nineteenth century who were affecting to be Brahmins by the 1880s. It seems they, like many others, took advantage of British censuses to establish and perpetuate a new caste identity. As Walker scoured a wealth of documentary evidence, she found that my anecdote was not an isolated incident but rather part of a long and systematic attempt to reinvent tradition. As she herself put it:

> The account of the Kathaks as ancient priests [first] appears, not in any Persian, Urdu or Sanskrit document, not in any of the books by Wajid Ali Shah, but in the 1881 British census report. The story of a Kathak teaching dance to Wajid Ali appears as a suggestion in the 1910s, and only becomes "fact" in the work of dance scholars after the middle of the 20th century. The colourful tales about Bindadin amazing the court with his genius appear even more recently in the anecdotes of Birju Maharaj. There are many mysteries in the history of *kathak* dance, and the history of the Kathaks is one of the most intriguing puzzles. This is because the Kathaks fashioned this history themselves. The evidence for the Kathaks' innovated identity is in the historic record, but the evidence of their continued, purposeful reinforcement of this identity can be found in conversations to this very day. (Walker 2004b)

If we accept that all traditions are invented, then the invention of kathak as the "ancient" art of Kathaks should come to us as no surprise. What may be viewed with dismay as a deliberate manipulation of the historical record by some will be accepted by others as just another example of a clever adaptive strategy in a highly competitive world where patronage and opportunity are limited. Simply put, our duty is to subject all the data we collect to detailed scrutiny and to let nothing go unchallenged.

The Problem of Conflicting Loyalties

It would be naive to think that our obligation to report the pure and simple truth was paramount in our research and publications. As the cynic Oscar Wilde pointed out, the truth is rarely pure and never simple. First, our hard-won access to trade secrets places us in an awkward position and limits what we can do or say about what we have learned. For example, the repertoire that I published in *The Tabla of Lucknow* was vetted by Afaq Husain, and although I respected his decisions I was nonetheless disappointed that I was not permitted to discuss the structural and musical variety of some of the choicest *gat* compositions for which his tradition is known. I had been won over by their beauty; he was fearful they would be stolen. This conflict of interest demands constant negotiation because, as researchers, we feel we need to say something insightful and explicit in order to convince our readership of the value and importance of the things we study. In his collaborative work on the analysis of rāg performance with Wajahat Khan, Richard Widdess

(2005) pointed out that tension often exists between the analyst's desire to describe how a performance actually is and the performer's normative and prescriptive vision of how things should be. (Other researchers simply say that there are differences between what the masters do and say they do.) Analyses and notations, as explicit representations of musical ideas, risk exposing and reifying a performer's intentions in a way that might benefit would-be imitators.

Second, our ethnomusicological investigations often turn up what we think of as fascinating socio-historical information about our informants that they would rather hide or disguise, for I have learned the hard way that identity is never fixed but rather a constantly reinventable resource that can empower musicians as they adapt to new socio-economic circumstances. I chose to write about the background of my ustād's family, exposing information that he would not have approved of had he been able to read my book; thus, I took a risk that I felt was diminished by my reliance on historical documentation and other previously published studies, such as Daniel Neuman's *The Life of Music in North India* (1980) that laid bare so many unspoken elements of traditional musicians' social organization. Later, I explained my rationale to Afaq Husain and won his approval. However, different circumstances might well require different solutions: for example, to expose how some of the prominent, high-caste, senior Hindu performers of today were once lowly, hereditary Muslims in their youth, or how *sāraṅgī* and tablā accompanists rose to become great vocal soloists simply risks censure by our informants (especially as they become more educated, aware of and involved as analysts in the studies we make of them). Whereas I might now be willing to raise certain kinds of issues, given my reasonably well-established academic seniority, I doubt I would feel the same way if I were beginning a dissertation, or starting out in my academic career. Moreover, one must always remember that musicians and their families may be harmed by certain revelations, and that their livelihoods could be compromised. The pure and simple truth sometimes carries a high price.

To summarize, from both methodological and ethical perspectives perhaps the most interesting issue we face in regard to our studies with and of the great masters is one of divided or conflicting loyalties: our discipular loyalty to our ustāds and gurus and our academic loyalty to accurate ethnographic reporting. There is considerable potential in our work for important information to be obscured because it is problematic; however, there is a strong possibility that we may also at times be promoting fictive information as if it were entirely unproblematic. Although our relationship with ustāds and gurus magnifies this dilemma owing to the extreme degree of obedience that is expected, it is easy to see how conflicting loyalties may be present in all ethnomusicological field research—indeed, in all research in the social sciences—where researchers engage in social relationships with their informants.

The Problem of Responsibility to Tradition

A few years ago I returned to Lucknow after nearly a decade's absence. Afaq Husain, with whom I had continued to study throughout the 1980s, had died in 1990 and his son Ilmas Husain—with whom I enjoyed a more symmetrical relationship because we were guru-bhāīs of a similar age—had become the new *khalīfā*, or head of the tradition. One evening as we sat drumming and chatting with old friends, Ilmas was asked by another guru-bhāī if he recalled a rare composition Afaq Husain had once played: our guru-bhāī could remember only the distinctive opening pattern. Ilmas, who learned more from his grandfather than his father, knew of similar pieces but not that specific one. Fortunately Afaq Husain had once taught me this piece and I was able to recite it on the spot using the customary drum syllables, then play it using the special techniques it required. Everyone was overjoyed. In that moment I felt for the first time that I had become an active link in the chain of tradition, not just a passive observer who had learned, stored, and analyzed musical data but a true participant capable of giving something back, of strengthening continuity with the past, and of playing a role, however minor, in shaping the future. Many other instances would follow where I dredged up things that hadn't been brought to mind in many years, or where my own recollection of musical fragments triggered Ilmas's memory of whole sequences of specialized compositions created by his ancestors. Moreover, like my guru-bhāīs, I also composed my own pieces, some of which were considered good enough to become part of the repertoire.

I knew that my responsibility to the academic community and the world beyond was—to paraphrase During—to paint a portrait of a musical tradition: to fine-tune an image of the richness and subtlety of that totality according to my experience of it. I also knew that I had to stretch to the limits my obligation to accurate ethnographic writing, even if that proved to be uncomfortable at times. Others would judge whether my work was credible, whether it inhabited its subject without succumbing to hagiographic nearsightedness, and whether it preserved the aura and mystery that had so captivated me to begin with. But what was my responsibility to the tradition itself, and to the community that valued that particular body of musical skills and knowledge? After all, I had originally traveled to this community in search of knowledge, I had spent many years learning and practicing, and I had in many ways matured into an adult within the tradition: someone who sat next to his brother, Ilmas, rather than down in front of him.

I found myself surrounded by Ilmas's students and by some of the younger members of the extended family. Now they touched my feet and asked with great deference: "Uncle, will you not teach us some of the things you learned with Afaq *dādā* [paternal grandfather]?" I knew Ilmas trusted me completely, and I wanted—indeed, felt a strong duty—to teach, to inspire, and to help perpetuate the tradition.

I also knew that it was by being an active link in the chain that I was best able to contribute to the tradition's vibrancy and vitality, and could most effectively represent my ustād and honor the generations of ustāds who preceded him. But what did these boys know, and how could I help them without spoiling them by giving out things they were not yet ready to understand? My mind flooded with the memories of how I felt when I first auditioned for Afaq Husain, and of how unprepared I was for what he had to offer. I now felt the full weight of an ustād's responsibilities; I had also gained unprecedented insight into the almost sacred duty of maintaining continuity by linking the past to the future. "Why don't you play for me," I began.

Note

1. As Daniel Neuman has argued (1980:155), three generations of accomplished family practitioners is also one of the key ingredients in the formation of the stylistic schools known as *gharānā*.

The Ethnomusicologist, Ethnographic Method, and the Transmission of Tradition

However else they may identify themselves and each other, fieldworker and subject are first and foremost human beings. It is this shared identity that makes fieldwork, with both its problems and its accomplishments, a meaningful mode of mutual learning.

R. A. Georges and M. O. Jones, *People Studying People*

Most ethnomusicological discussions of the transmission of tradition attempt to document and interpret the manner in which music is communicated over time within a particular setting, giving attention to both the interpersonal dynamics and communication technologies of these processes.[1] However, I will focus my inquiry neither on the native carriers of tradition nor on the materials these traditions convey. Rather, I propose to take a reflexive turn and to discuss the role of the ethnomusicologist who, while seeking to document the transmission process, becomes a part of it.[2]

I will approach this subject by drawing on instances from my own field experiences and those recounted by colleagues in the literature. I wish to move discussion beyond an appreciation of the impact of "relational knowledge" (Rosaldo 1993 [1989]:206–8)[3] on ethnographic interpretation and writing to explore more deeply a type of reciprocity and grounded action that is a surprisingly frequent outgrowth of the ethnomusicological research process. I will suggest that an idiosyncratic theoretical stance and working methodology give rise to this outgrowth of ethnomusicological research and that it likely has its roots in the close but conflicted relationship of ethnomusicology with other disciplines.

It is necessary to sketch a brief disciplinary perspective, to which I will return again later in this essay. In terms of its intellectual history and the training of its researchers, ethnomusicology has been shaped by the often contradictory worlds of

historical musicology and anthropology. For the historical musicologist, the transmission of tradition is such an implicit aspect of her activity that it has largely escaped critical scrutiny. Any card-carrying historical musicologist would readily acknowledge that she is implicated in the continuation of the tradition studied. From its inception in 1885 as one part of the larger field of musical scholarship within which comparative musicology was subsumed, historical musicology has had as an important adjunct to its scholarly mission the (re)discovery, interpretation, and perpetuation of musics of the Euro-American art music tradition. Indeed, the American Musicological Society each year presents the Noah Greenberg award "to stimulate active cooperation between scholars and performers by recognizing and fostering outstanding contributions to historically aware performance and to the study of historical performing practices" (*American Musicological Society Directory* 1993:7). The annual conferences of the society feature special recitals and concerts of compositions not otherwise widely heard and performed.[4] Thus, musical manuscripts surviving only in scattered archives have been unearthed, reconstructed, edited, and performed by historical musicologists as a matter of course. To quote Joseph Kerman's appraisal of musical scholarship in *Contemplating Music*, "any scholarly edition of music is an invitation to a performer, and musicologists have been known to press such invitations quite hard, lobbying, consulting, and masterminding . . . concerts when they are given a chance" (1985:185).

Indeed, the central polemic among historical musicologists vis-à-vis the act of performance and their own role in transmitting (and even reinventing) tradition seems to center largely around issues of authenticity versus creativity in the act of musical reconstruction and performance practice.[5] Musicologists do not generally question whether they *should* be active in the process of transmitting musical tradition; rather they simply debate *how* closely they should adhere to historical precedent and in what manner the questions arising from lacunae in their sources can or should be answered. Most ethnomusicologists have been trained as undergraduates in music departments operating under the system just described, the same venue in which the vast majority of ethnomusicology professors eventually find their institutional homes.

This long-standing interaction between scholarly documentation and the act of performance has had its influence on ethnomusicological theory, most notably in the notion of "bi-musicality" advanced by Mantle Hood. The founder of the first major ethnomusicology program at UCLA, Hood felt that "the training of ears, eyes, hands and voice and fluency gained in these skills assure a real comprehension of theoretical studies" (Hood 1960:55). Hood was secondarily concerned that training and performance in Western music constrained ethnomusicologists studying other traditions. Hood did not just write about the importance of becoming bi-musical (or multimusical) and gaining cross-cultural musical experience through performance. He established an ethnomusicology curriculum including native

performers, who were brought to UCLA to instruct students in a range of musical traditions. Early Hood students went on to found other programs at Wesleyan, Michigan, Seattle, and elsewhere. Becoming bi-musical became an increasingly common norm among ethnomusicologists, who capitalized on their bi-musicality by carrying out truly participatory participant-observation in the field.

In contrast, raising the possibility of the involvement of the anthropologist in the transmission of tradition evokes a response quite opposite from that in historical musicology. Anthropologists have also generally not addressed this issue explicitly; only in 1990 did the revision of the American Anthropological Association Principles of Professional Responsibility strengthen and personalize the statement of responsibility to the "people whose lives and cultures anthropologists study," mentioning for the first time the possibility of both the "positive and negative consequences of [the anthropologists'] activities and the publications resulting from these activities" (Fluehr-Lobban 1991:274–275).[6] Indeed, although the record shows that anthropologically trained ethnomusicologists have also actively participated in musical performance in the field, they have done so most often to ensure reciprocity and/or to test their understanding of musical data they have gathered. To cite an example from an ethnomusicological study carried out by a scholar trained primarily in anthropology, Steven Feld allowed himself to be represented as a "song man" within his own culture to the Kaluli (for whom he played recordings of Charlie Parker) (Feld 1990[1982]:11). Feld also composed and performed Kaluli songs for his research associates to test hypotheses about "constraints upon form" (p. 13).

Yet even anthropologically trained ethnomusicologists have been influenced by Hood's maxim. In a study advocating a "musical anthropology," Anthony Seeger moves somewhat beyond Feld in incorporating musical performance for heuristic purposes. Seeger dedicates his book *Why Suyá Sing* (1987b) "in memory of the songs we sang," and describes in some detail the folk music styles ranging from bluegrass to African songs that he and Judith Seeger taught to the Suyá. In some cases, Seeger acknowledges that he altered folksongs learned from his uncle, Pete Seeger, "to fit a pattern easily recognizable to the Suyá" (Seeger 1987b:20). In honoring a request by the Suyá that he collaborate in publishing a recording of their music (pp. 23–24),[7] Seeger's activities in fact come very close to the sort of ethnomusicological participation in the transmission of tradition I seek to examine here.

Thus, ethnomusicological activity in the transmission of tradition appears to draw on musicological commitments to the preservation of musical tradition wedded to anthropological concerns regarding reciprocity and social responsibility. Apart from the disciplinary implications and the insight they provide into the values of different fields of study, discussion of the role of the fieldworker in the transmission of tradition lays bare an aspect of the intensely human nature of fieldwork and raises at the same time slippery issues in the ethics of ethnographic

research that have been little discussed.[8] Most discussions of ethics have tended to focus on interpersonal relations both during and after fieldwork, and only incidentally to address the impact on the musical tradition itself.

Ethnography and Transmission

My concern with this subject did not emerge initially on a theoretical level. Rather, an experience in the field several years ago pushed me toward a new set of considerations concerning the role of the ethnomusicologist. Let me describe in some detail the ethnographic event, and its broader context, that served to throw this consideration into relief.

For nearly a decade I have been doing fieldwork with Jews of Syrian descent who live in Brooklyn, New York. The project began as a team effort with my New York University graduate students and the Syrian community (detailed in Shelemay 1988). I have continued research on my own since concluding the team project in 1986 and expanded its boundaries to incorporate multilocale fieldwork among Syrian Jews in Mexico and Israel discussed in Shelemay 1998.

Some background is needed to frame the following discussion. Some seventy years after their migration from Aleppo to the New World, a community of more than 30,000 Syrian Jews in the New York metropolitan area sustains a strong Judaeo-Arabic identity expressed, in part, through many aspects of musical performance. The central musical repertory is a corpus of paraliturgical hymns called *pizmonim* (sing. *pizmon*), which have newly composed Hebrew texts set to borrowed Arabic melodies. The *pizmon* tunes are adopted from popular songs in the Arab music tradition, whereas the Hebrew texts contain biblical and liturgical allusions, as well as veiled references to individual members of the community for whom the songs are composed and to whom they are dedicated. The multivocality of the songs and the memories sustained in separate channels of text and tune provide wonderful material for social and historical analysis, but that is not our subject here. The focus of the original research project on the *pizmonim* emerged directly from suggestions of knowledgeable Syrian community members who wanted to record as many of the 500 extant songs as possible. The initial team research project recorded performances of nearly two hundred *pizmonim*, and deposited copies of all in a community archive, which members of the research team also helped catalogue and organize.

The event that highlighted issues concerning transmission took place in the Syrian community on March 14, 1990, and was mounted to honor Meyer "Mickey" Kairey, a man then in his late sixties who for many years has been a mainstay in the Syrian community's religious life. One of Mickey Kairey's most notable activities has been the teaching of *pizmonim* to Syrian young people. Mickey Kairey played an important role as one of the chief research associates for the *pizmon* project and on many occasions shared his expertise.[9]
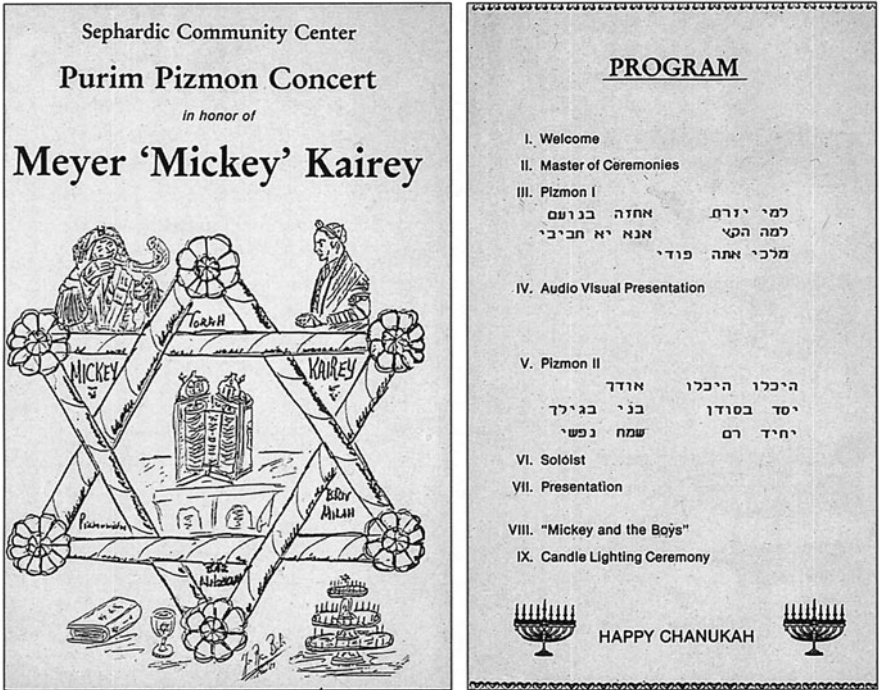
Fig. 9.1. Program for Pizmon *Concert in honor of Meyer "Mickey" Kairey. March 14, 1990, Sephardic Community Center, Brooklyn, New York.*

The concert (fig. 9.1), attended by an overflow crowd of approximately 350 people, was held at the Sephardic Community Center in Brooklyn, the institutional center of the Syrian Jewish community. The program[10] included two different "sets" of *pizmonim* sung by a choir of young boys accompanied by an ensemble of Middle Eastern instruments; a third group of solo songs was performed by Isaac Cabasso, Mickey's uncle and himself a beloved lay cantor. The climax of the evening was the presentation of an oversized framed certificate to Mickey containing the signatures of some 1000 of his students trained between 1955 and 1990. See figure 9.2 for an example of congratulations placed in a dedication book produced by families grateful for Mickey Kairey's role in transmitting *pizmonim*.

Midway through the program, there was an audiovisual presentation about Mickey's life and work. Slides traced Mickey's career, including pictures of his synagogue and of his *pizmon* teacher and mentor, Eliyahu Menaged. There were images of his family, photos from his years of military service during World War II, and innumerable references to his love of music of all kinds, including Stan Kenton and the Big Bands of the 1940s. Slides showing Mickey training young boys for their Bar Mitzvahs were accompanied by commentary and recordings made by

students of these lessons. Suddenly, I heard on the recording my own voice asking Mickey a question about a *pizmon;* it had obviously been taken from the tape of a session we had held some five years earlier when Mickey taught *pizmonim* to me and my students. Although the concert overtly celebrated the role of the individual in the transmission of tradition, in this case Mickey Kairey's enormous contribution, I had not realized that I was a part of Mickey's experience just as he was part of mine. The ethnomusicologist had been folded into the experience of Mickey and his community, one very small link in the chain of transmission leading from their past to the future.[11]

If I had any doubts that the ethnomusicological presence had become a factor in the transmission of tradition, they were resolved quite coincidentally during an interview that took place shortly after the concert. The young Syrian cantor with whom I spoke discussed the revival of *pizmon* singing he believed to be present among young people in the Syrian community. He further suggested that a catalyst for the revival was events like the concert the prior evening, a performance genre that had emerged in the wake of the team project (personal communication, B. Zalta, 16 March 1991).

Clearly, the Syrian music project had left more of a trace than recordings of music and oral histories in an archive. Six years after its inception, it had been absorbed into the fabric of both community activity and individual memory. At this intersection of life and scholarship converge formal, institutional relationships, such as that established between the Sephardic Community Center and New York University in terms of copyright and royalty agreements for the record we coproduced (Shelemay and Weiss 1985) and a complex network of close individual friendships between me, several of my students, and some two dozen individuals within the Syrian Jewish community.

Reviewing my journal, project correspondence, and other residue of our long association, I found other instances of my own activity that directly touched on the processes, personnel, and politics of transmission. Let me briefly set forth a few examples.

In June 1986, I was asked to write to the United States Immigration Service on behalf of a visiting cantor from Israel whom the largest Syrian synagogue in Brooklyn wished to retain on a permanent basis. Here I invoked my authority as a professor and used my knowledge of the tradition to aid the community in a matter of great importance to them. In fact, as an ethnomusicologist, I was actually quite concerned about this turn of events, since the distinctive Aleppo musical tradition sustained in Brooklyn was under pressure and undergoing a significant amount of change precisely because of the influx of talented Israeli-born cantors who carried different streams of Sephardic tradition. In my letter, I purposely omitted this information and consciously played a role directly affecting transmission in a direction about which I was personally ambivalent but that the community desired.

IN HONOR OF

OUR BELOVED PIZMONIM TEACHER

MEYER "MICKEY" KAIREY

The Graduating Class

of 1970

Magen David Yeshivah

שיר ושבחה הלל וזמרה

DEAR MICKEY,

Just wanted to thank you for teaching us Ta'amim and Pizmonim, and for preparing us for our Bar-Mitzvahs. We sing the Pizmonim every Shabbat, and now our children are beginning to pick up the tunes.

You should be very proud that our great traditions are being carried out throughout the community due to your tireless efforts.

Sincerely,

Jack and Bert Hidary

Fig. 9.2. Tributes from Dedication Booklet for Meyer "Mickey" Kairey distributed March 14, 1990, Sephardic Community Center.

In 1987, I was asked by a leader of the community to give him information that would help defuse a growing concern among local rabbis that so many of the melodies used in Syrian *pizmonim* are of secular and/or even of Christian or Islamic origin. Specifically at issue was the *pizmon* "Mifalot Elohim," which borrows the melody of the well-known Christmas carol "Oh Tannenbaum." It is almost certain that rabbinical skepticism about the broader issue was provoked in part by the publication of our recording two years earlier and the subsequent high profile of our collaboration in what had become known as "the Syrian music project." In response to this request, I wrote a letter giving my associate information on several controversial, borrowed melodies, and provided a rationale that could be used in justifying the tradition:

> I do not think the original sources of these melodies should be of any concern to you or the community. There is a longstanding tradition in Jewish music (both sacred and secular) of borrowing melodies from the surrounding society. This tradition is as widespread in Ashkenazic circles as in your own pizmon tradition, only the sources of the melodies differ because of the different geographical settings. Music is always part of the surrounding cultural milieu and I know of no tradition that is "pure" and does not borrow a variety of things with which it is in contact. The very nature of musical expression is that it is transmitted from person to person across geographic, social, and cultural boundaries. (Kay K. Shelemay, letter of June 9, 1987 to a member of the Syrian community)

In this case, then, I explained the transmission process in order to justify, and ultimately, to preserve it.

The publication of the record *Pizmon*, which included a selection of *pizmonim* taken from recordings made during the first year of our joint project, had other unanticipated outcomes as well.[12] The record was chosen by the American Folklife Center for its Selected List (American Folk Music and Folklore Recordings: A Selected List 1985), and it also won a prize from the national association of Jewish community centers. Both awards were a source of shared pride for everyone involved. However, the publication of the record, and the "first annual *pizmon* concert" around the same time in 1985, raised perceptions outside the community that the Syrian men who had come together for music sessions comprised a group.[13]

My interaction with these individuals outside formal recording and interview sessions increased as we began to get invitations from area cultural institutions and universities: Generally, the Syrian men were asked to perform and I was asked to give a lecture or long introduction that explained the music to be sung. The men with whom I worked were quite comfortable with this arrangement despite the obvious asymmetries; indeed, they had invited me to speak at the first *pizmon* concert and suggested to the sponsor that I participate when they received the first invitation to perform outside the community.

I now realize that this was only the beginning of my increasingly active role in the transmission process, one that paralleled the deepening friendships between myself and several individuals in the community. A pivotal event took place on November 15, 1987, when we were invited to perform a combined lecture/concert at a community center on the Lower East Side of Manhattan. As I led off the session with a talk explaining the Syrian musical tradition, the elderly audience comprised largely of Jewish immigrants of Eastern European descent got very restless. Every time I mentioned the connection of the Syrian Jewish tradition to Arab music and used the word "Arab," members of the audience hissed.[14] After several such incidents, Moses Tawil, the nominal leader of the Syrian men who were to sing *pizmonim*, stood up from where he sat behind me on stage and joined me at the microphone: "We are businessmen and we don't have to be here," he said emphatically. "We are interested in what Professor Kay has to say and want to hear it. Please be quiet."

I can't say that Tawil's admonition improved the audience's deportment—I still consider this talk the worst single public lecture experience I have ever had— but it was an enormously important moment of warmth and bonding between myself and the Syrian men present. From that moment forward, I received invitations to family events, Bar Mitzvahs, wedding anniversaries, and holiday celebrations. And the closer we became, the more I was called on to play a role in perpetuating the tradition.

Therefore, I would like to argue that as ethnomusicologists become engaged in research with living musical traditions and the people who carry them, they both intentionally and unwittingly become caught up in the processes and politics of

transmission of tradition. Sometimes their interventions support continuity; at other times they engender change. I believe that these interactions are not conceptualized as formal, scholarly acts, but are carried out relatively unconsciously on a much more personal level as the study of tradition shifts almost imperceptibly to occupy a relational space situated between scholarship and life. As relationships "in the field" mature from the initial formality of scholar/research associate (if indeed there is the luxury of ample time and access) to more collegial and personal ones, the fieldworker inevitably moves beyond the management of cultural capital into the negotiation of human relations in the field.

Transmission and Tradition

As I began to reconsider what in retrospect appears to be my surprisingly active role in the transmission processes within the Syrian community, I reviewed my other past fieldwork projects—multiple urban and rural research projects in Ethiopia, a combined archival/ethnographic experience at an American synagogue in Houston, a notably unsuccessful six-month experience with a new music group in New York City—and looked for similar patterns. Indeed, they were there and I can only conclude that such patterns are much more prevalent than ethnomusicologists generally acknowledge. In what follows I would like to identify and briefly discuss three ways in which the fieldworker is most frequently implicated in the process of transmission: preserving tradition, memorializing tradition, and mediating tradition. No doubt there are more, and any one ethnographic experience might give rise to varying combinations of the three at one time. As part of the process of definition, I'll set forth some brief examples from my own experience and those gleaned from the ethnomusicological literature. Almost without exception, these situations inevitably arise at the point of intersection of life and scholarship—they begin at moments when the study of a tradition becomes part of the life of the tradition itself and relationships in the field deepen to a more interactional model.

Preserving Tradition

If any aspect of the ethnomusicologist's entry into the transmission process is generally acknowledged, it is the presumption that ethnomusicological activity works on one level to preserve. Although the ethic of preservation was long an unquestioned part of the ethnographic process, and older paradigms led earlier scholars to seek out and study certain traditions since they would otherwise "be lost," it seems clear that the very process of studying any musical tradition is tantamount to participating in an act of preservation.

Frequently the role of the ethnomusicologist as preserver of tradition is acknowledged or even desired by people within the tradition itself. To take but one example from the literature, Barbara Smith recounts how she learned bon-dance

drumming among Japanese immigrants in Hawai'i, became a member of a club, and "beat the drum" at bon dances one summer (1987:211). A few days after the second bon dance at which she performed, she was told that a member of the community had commented: "Now it is safe for us to die, because if Professor Smith is drumming there will always be someone to drum for our souls" (p. 211). Smith goes on to relate that her drumming encouraged some young people to learn to play and that there has not been a shortage of drummers since!

There are most certainly instances where the ostensible "informant" charges the "ethnomusicologist" with the responsibility of transmitting tradition. A graphic example occurred in my own work in northern Ethiopia among the Beta Israel (Falasha). One day, an elderly Beta Israel priest looked at me solemnly and said: "In twenty-five years, only you will know our prayers" (Shelemay 1989:xviii). He was both acknowledging a reality of the transmission process within his own community and making me aware of my responsibility to preserve his tradition.

It strikes me that preservation is therefore not just an outgrowth of now dated scholarly paradigms, but at least in some circumstances, both an acknowledgment of the realities of a musical change and part of an implicit contract between the ethnomusicologist and the tradition's native carriers. This contract may be particularly crucial in the case of "insider" research, when the scholar shares wholly or in part the identity she studies.

The bon-dance drumming example cited earlier also highlights a type of preservation I have not experienced,[15] but that is most common in the field at large. Although all ethnomusicologists transmit speeched knowledge and recorded music, many further transmit music tradition through re-creating the act of performance itself. In this manner, the performative nature of the ethnomusicologist's unit of study lends itself to replication, both before and after the ethnographic research period. Many ethnomusicologists today teach the music they learned in the field. Although one may question this activity as an act of appropriation, I believe that it is not generally regarded as such by the native carriers of the tradition or by the ethnomusicologist. Rather, it can be viewed as part of the very human process of passing on a world of expression that is inordinately private. How can one read John Miller Chernoff's description of his acquisition of Ghanaian drumming techniques (Chernoff 1979) and not acknowledge that he, like his teacher, sustains this music as "a bodily memory?" (Connerton 1989).[16] Theories such as Mantle Hood's "bi-musicality" allow for much more than an entry into musical learning; they implicitly move the ethnomusicologist toward the preservation, replication, and active transmission of tradition.[17]

Memorializing Tradition

Although we tend to conceptualize transmission in terms that are communal and social, in fact the workings of the process are intensely personal and idiosyncratic,

the source of the tradition being a teacher (the informant, or more appropriately, the research associate), the receiver a student (the ethnomusicologist). The tendency of ethnomusicology to extrapolate from the individual to the group combined with longtime anthropological traditions supporting anonymity for subjects of research, has resulted in fewer traces of memorializing in our literature. But there are examples. Bruno Nettl has written an ode to his favorite teachers (1984) in which he discusses and memorializes the men who once would have been called his informants.[18] Likewise, the autobiography of Frank Mitchell, *Navajo Blessingway Singer,* was in part "a realization of Frank's wish that a book on his life would live after him" as well as a sense of "family unity" growing out of his long years of collaboration with the editors (Mitchell 1978:5). Frisbie and McAllester acknowledge that their relationship with Frank Mitchell was an intensely human one that progressed from an initial development of rapport, through work on various projects, to a lifelong friendship with mutual obligations and responsibilities (p. 5). That this book is dedicated to the memory of Frank Mitchell is not coincidental.

Mediating Tradition

Navajo Blessingway Singer also leads us into a third mode of transmission—mediation. In addition to memorializing Frank Mitchell, Frisbie and McAllester mediate between him and the wider world: "Frank, of course, is the author of *Navajo Blessingway Singer*. Our job has been to collect the data, edit the narration and, with the assistance of able interpreters, put it into English" (Mitchell 1978:8). In one sense, every time a scholar quotes or paraphrases an interview or conversation, he mediates tradition. Some researchers have in fact referred to themselves as mediators. Alan Lomax doesn't consider himself a "reviver so much as a stander-in-between," perceiving an important part of his responsibility to "find the best folk singers . . . and get them heard everywhere" (cited in Sheehy 1992:329). Beth Lomax Hawes put it even more strongly in comments made at a 1981 Folk Arts Panel meeting: "That's right, we're meddlers!" (Titon 1992:316).

Mediation takes many forms and may not be restricted to an intermediary zone between the community and outsiders. In addition to "mediating" for my Syrian research associates, giving talks to introduce their performances to audiences unfamiliar with the *pizmon* repertory, I also was asked to assume this role within the community. At a gathering of the extended Tawil clan and several hundred other Syrian families in a Catskill mountain resort one Passover in the late 1980s, Moses Tawil arranged for me to give a public lecture on the Syrian Jewish musical tradition; the majority of the audience at my talk were Syrian Jews. Mediation can therefore entail not just translating for those outside of the tradition, but also participating in raising awareness of the tradition within the community itself.[19]

I suggest then, that many (if not all) ethnomusicologists preserve, memorialize, and mediate traditions on a fairly regular basis, in large part because of what I would term the "bracketed performativeness" of the materials they study. Both in the field and afterward, this is emphatically not a theoretical issue. One learns music by doing and remembers by repeating, whether through live performance or sound recordings. Ethnomusicological data in the musical domain are replicable in a way in which other types of ethnographic data are emphatically not. I would propose, therefore, that the involvement of the ethnomusicologist in the transmission of tradition is an old and deep aspect of the ethnomusicological research process, emerging in large part from the nature of its data.

From a disciplinary perspective, here we encounter head-on the ethnomusicologist's bifurcated identity, which draws at once on musicological commitments to performance and anthropological tenets of noninterference. The tension between these approaches has surfaced intermittently in the literature, moving one past leader of the field to write that the ethnomusicologist "does not seek the aesthetic experience for himself as a primary goal (though this may be a personal by product of his studies), but rather he seeks to perceive the meaning of the aesthetic experience of others from the standpoint of understanding human behavior" (Merriam 1964:25). Only in more recent ethnomusicological writing are there explicit acknowledgments of the shared involvement that emerges in the field. In the words of one ethnographer:

> There is no substitute in ethnomusicological fieldwork for intimacy born of shared musical experiences. Learning to sing, dance, play in the field is good fun and good method. Being an appreciative audience is an especially important form of musical exchange. Savour the joy of being a student again; establishing a close relationship with a master musician is a common and successful approach in ethnomusicology (Myers 1992:31).

But if "play" in the field is to be based on good methods, then ethnomusicologists require guidelines for a situation rife with ethical and practical problems. Moving to a more prescriptive mode, it would seem that ethnomusicologists and others who become implicated in the transmission process could well consider the following points:

- If we are bound explicitly or implicitly to preserve what people have taught us, we must document it carefully and deposit it faithfully in archives.
- Our work should be timely, to permit both dialogue with the living and meaningful memorial of the dead.
- We should honor confidence and protect it when necessary, but be equally ready to acknowledge and celebrate individual expertise and artistry when they are freely and openly given.

- We should share the fruits of our labors, whether by repatriating tapes, providing materials for use within the community and/or by individuals within it, or through sharing financial rewards such as royalties.
- If fieldwork is to be a truly humanistic pursuit, we must erase what has been termed "the mistaken dichotomy," the false divide thought to separate academic research from public sector work. We must accept responsibility not just for the impact of our entry into the field, but for our abiding relationship to it and our teachers long after we have "left" (i.e., discontinued research).
- If we are to be truly coeval in time with the men and women who are our teachers (Fabian 1983), we must engage in collaborative processes. Collaboration in turn helps to reduce power assymetries and assures greater congruency between ethnographic goals and the sensitivities of individuals and communities.
- We must acknowledge more openly that in many situations, the entering scholar may at times be perceived to be an authority and to possess a degree of power that will inevitably be invoked in the pursuit of real life. We are obligated to use our knowledge and power, should they be conceived as such, in the best interest of the people with whom we work.
- Occasionally, we will encounter situations in which our goals are not those of the community or in which we are entrusted with materials that we are explicitly charged with keeping secret. In these rare instances, our best course is silence or withdrawal. We need to preserve a place in the oral tradition of pedagogy where we can discuss the unsuccessful and the discarded.

Ultimately, the acknowledgment of fieldwork as a problem in human relations offers a pathway through the thicket of issues surrounding the ethnographic process and the potentially intrusive role of the fieldworker. This seems to be congruent with a trend in both ethnomusicology and anthropology to develop a practice-informed theory (Titon 1992).

Most of us are well aware that we do not study a disembodied concept called "culture" or a place called the "field," but rather encounter a stream of individuals to whom we are subsequently linked in new ways. Given the increasing interest in what Arjun Appadurai has called "deterritorialization," I would suggest that human relations may be the most promising residue of a field once conceptualized as local, stable, and bounded (Appadurai 1991:192). We can begin by teaching and practicing an ethnography that acknowledges a reality of sharing and interaction, one predicated on negotiated relationships.

Postlude 2007

More than twenty years after the inception of the Syrian music project, I remain in close contact with members of the Syrian Jewish community. The project continues to resonate in the Syrian community, with a younger generation stepping

forward to teach and perform *pizmonim*. Some of these individuals were students I taught or came into contact with during my years at New York University.

The human relationships built in the course of this project have moved beyond the domain of research to become part of the fabric of my life. I exchange cards, telephone calls, and e-mails with members of the community and last fall received as a gift from Moses Tawil and his family a series of *pizmon* recordings that had been remastered in Israel to contain the accompaniment of a full ensemble of traditional Arab instruments. In August, 2006, Tawil will celebrate his ninety-first birthday at a party at his home in New Jersey that will feature the music of an ensemble constituted of Arab and Jewish musicians.

I remain engaged with processes of preservation, memorialization, and mediation of the Syrian Jewish music tradition, continuing to mine the extraordinary storehouse of data gathered over the course of a decade of research with Syrian Jews, moving on to new topics that seem to emerge on their own from the data (see Shelemay 2006a and forthcoming). These activities are happily shared with my former doctoral student, Hebrew Union College Professor Mark Kligman, who entered the NYU program the year after the team research ended and subsequently wrote his doctoral dissertation on music of the Syrian Jewish liturgy. Within the last several years, as a postdoctoral fellow at New York University, another former student, Judah Cohen, has established his own ethnomusicological relationship with members of the Syrian Jewish community. His essay discusses this continuing chain of ethnomusicological transmission of tradition.

Notes

1. In one sense, the "transmission of tradition" is a tautology, since the etymology of the word, from the Latin *traditum*, refers to anything that is transmitted or handed down from the past to the present (Shils 1981:12). By "musical transmission" I refer to any communication of musical materials from one person to another, whether in oral, aural, or written forms, without regard to the time depth of the materials transmitted. For the sake of discussion here, I will focus primarily on the role of live musical performance in this process, and secondarily on musical materials mediated and conveyed by technologies such as the LP, cassette, or compact disc.

2. This chapter, written during a 1992–1993 fellowship year funded by the National Endowment for the Humanities, is an expansion of an article published under the title "The Ethnomusicologist and the Transmission of Tradition," *The Journal of Musicology* 14(1): 35–51, 1996. The initial version of this paper, titled "Intersections of Life and Scholarship: Human Relations in the Field," was delivered at Brown University in 1992. I thank Gregory Barz and Timothy Cooley for both the invitation to Brown and their subsequent comments on the resulting chapter.

3. Rosaldo has suggested that "relational knowledge," which constitutes a shared expressive form on the "borderland" between ethnographer and "subject," "should be regarded not as analytically empty transitional zones but as sites of creative cultural production that require investigation" (Rosaldo 1993 [1989]:208).

4. Even such prosaic forums as business meetings can celebrate the reentry of a composition into the repertory. The 1993 annual meeting of the AMS featured the first performance of a recently edited and published chamber work by Ruth Crawford [Seeger] (Tick 1993). Editor Judith Tick describes herself as a type of musicological midwife in the rebirth of this composition, which she was thrilled "to send out into the world" (Tick, personal communication).

5. For a rare and explicit critique of the search for authenticity in musicologically inspired performance, see Richard Taruskin (1982). Taruskin comes close to acknowledging the role of the musicologist in the transmission of tradition, in his comments on a story credited to Dmitri Shostakovich: "What's a musicologist? I'll tell you. Our cook, Pasha, prepared the scrambled eggs for us and we are eating them. Now imagine a person who did not cook the eggs and does not eat them, but talks about them—that is a musicologist. Well, we're eating them now, and even cook up a few on occasions, as when we do a little discreet composing to make a fragmentary piece performable . . ." (Taruskin 1982:349). That discourse about authenticity in editions intended for performance is still a very lively issue can be seen in Frederick Neumann's article, "Improper Appoggiaturas in the Neue Mozart Ausgabe" (1992).

6. Ethical guidelines or codes of ethics adopted by various American anthropological societies since 1949 are printed together in Fluehr-Lobban 1991:237–69 and 2003: 247–66.

7. *A Arte Vocal dos Suyá* (1982) jointly published by Seeger and the "Suyá community."

8. Mark Slobin has pointed out that ethical issues were not discussed at all in the ethnomusicological literature until the 1970s, and that ethical awareness in the field remains in an "embryonic state" (Slobin 1992a:331). Slobin's discussion, however, does not move beyond "the bounds of problems raised by the earlier modes of inquiry" (p. 332).

9. I also worked closely with and interviewed his brother, Hyman Kaire (the two brothers spell their name differently), and late sister, Sophie Cohen.

10. The cover of the program booklet contains symbols of Mickey Kairey's active musical role in liturgical and life cycle events. Alert readers will note that the table of contents contains the inscription "Happy Chanukah" at the bottom. The concert, originally planned to coincide with the Chanukah holiday in December, was postponed to the March Purim observance due to a family emergency.

11. The individual who had prepared the commentary accompanying the slide show later told me that she had originally included my own singing of a *pizmon* on the tape, just like those of the young boys Mickey trained, but later deleted the excerpt in fear that it would offend more traditional members of the community who adhered to religious prohibitions concerning the hearing of a woman's voice.

12. This in contrast to its planning and execution, which were quite straightforward and largely without complications. All the performers signed consent forms, and the royalties (of which in the end there were none!) were to be divided between the Sephardic Archive and New York University. Assignment of royalties to the performers was not an issue; they were for the most part affluent musical amateurs for whom the receipt of money would have been highly unacceptable.

13. This is a surprisingly common phenomenon, the development of what has been termed new "performance frames" growing out of the impact of ethnomusicological

fieldwork. See Sheehy (1992:332). Also, see Dyen (1982), for a detailed case study of this phenomenon in the sacred harp tradition.

14. The Arab music tradition is generally unfamiliar to Jews outside of the Middle East and, for some, including members of that particular audience, evidently carries negative associations stemming from the ongoing Arab-Israeli conflict. This contrasts markedly with the great pride in and nuanced understanding that many Syrian Jews have of Arab music.

15. Largely because I've been a woman studying esoteric male music traditions, whether in Ethiopia or Brooklyn, my own opportunities for performance in the field have necessarily been limited.

16. See Chernoff's account (1979:104) of studying drumming with Alhaji Ibrahim Abdulai, who remarked that "teaching with the hand is more than teaching with the mouth."

17. Long before the notion of bi-musicality spurred performance by ethnomusicologists and sparked revival, fieldworkers had actively intervened in the transmission of tradition. A notable example is that of John Lomax, whose studies of cowboy songs and frontier ballads (1910) "aimed to feed back song lore into the stream of oral tradition" (cited by Sheehy 1992:326).

18. I would note that the most striking examples of preserving, memorializing, and mediating tradition have been recounted initially in the oral tradition of the field, i.e., in lectures only later published. This is true of Nettl (1984) and Smith (1987), both of which were first presented as Charles Seeger Lectures at annual meetings of the Society for Ethnomusicology.

19. Repeated mediation of these different types also led to one of the most interesting acts of exchange in my academic career. In April 1991, Moses Tawil was invited to speak at the meeting of the Coalition for the Advancement of Jewish Education. He telephoned to ask if he could borrow a copy of my Passover Catskill lecture along with the accompanying tape of musical examples to use for his talk; he reassured me that he would credit me at the beginning of the presentation. I sent him the materials he requested; he reported that the talk went well.

Shadows in the Classroom

Encountering the Syrian Jewish Research Project Twenty Years Later

What happens when the shadows we chase appear in our classrooms? From one perspective, I offer this essay as a reflection on central ethnomusicological questions regarding the subjects, methods, locations and ethics of conducting ethnographic field research, particularly when traveling in the footsteps of earlier scholars. In another sense, however, I use this essay to explore the pragmatic and theoretical issues involved in bringing students into contact with musical cultures they call their own, through the deceptively hermetic space of the university classroom. That fieldwork and classwork can complement each other, as I hope to show, offers an argument for understanding the first perspective. That the constructed border between the two can fade spontaneously into a multilayered and theoretically complex sense of ethnographic experience, as I will suggest, adds nuance to the second. Stepping into the classroom, in other words, can become an intimate, self-reflective, and sometimes startlingly unplanned trip to the field.

In 1984 and 1985, Kay Shelemay led a three-semester team research project involving a total of 17 graduate students. Conducted in conjunction with the Sephardic Archives in Brooklyn, New York, the project had a twofold purpose: first, to record, document, and analyze the Syrian Jewish *pizmon* tradition of paraliturgical *contrafacta* for posterity, and second, to offer graduate students the opportunity to practice ethnographic and musical skills intended to serve them throughout their careers (Shelemay 1988). Shelemay and her students—a group that included, at different times, ethnomusicologists Geoffrey Goldberg, Rolf Groesbeck, Amy Horowitz, Ingrid Monson, and Sarah Weiss among others—cultivated relationships with several prominent musically involved members of the community over their period of collaboration, and forged through these ties a synergetic relationship between the Brooklyn community and New York University. Shelemay retained these ties personally as well. Although she left New York University in 1990 for Wesleyan University and eventually Harvard University, the

Syrian Jewish population and its musical authorities retained a prominent place in her academic life—eventually leading to her study *Let Jasmine Rain Down* (1998) and additional publications (1988, 1996, 2006[2001], 2006a).

In 1996, I began my doctoral studies in ethnomusicology with Shelemay at Harvard. Over the years I worked with her, I, too, became immersed in Syrian Jewish culture to a degree. I served briefly during the summer before matriculation as a research assistant for *Let Jasmine Rain Down*. When participating in a team research project for a class on early music in the Boston area during my first year of graduate work, I read her article chronicling the Syrian team fieldwork project (1988). Her ethnomusicology seminars would invariably include footage or sound examples from her Syrian Jewish fieldwork, either to illustrate the research process or to expand upon a particular aspect of memory or Syrian Jewish music. I read *Let Jasmine Rain Down* in proof form as I studied for my post-second-year general examinations. And I twice taught undergraduates the rudiments of *pizmonim* as a teaching assistant in her *Soundscapes* course (see Shelemay 2006[2001]: 232–253).

Thus, thanks to a combination of advisorial choice and personal study, I found myself gaining a broad secondary knowledge of Syrian Jewish culture and musical practices. Yet like many instructors teaching outside their realms of personal experience, I needed to rely on the work of others; the "expertise" I developed depended mainly on the descriptions and theoretical constructs of my teachers and more senior ethnomusicology colleagues. I therefore graduated with relatively extensive—though entirely inherited—experience in Syrian Jewish music.

This situation began to change in fall 2003, when I started a three-year postdoctoral position in New York University's Hebrew and Judaic Studies department, becoming the first full-time Jewish music specialist on faculty since Shelemay thirteen years earlier. As part of my course offerings, I prepared a new class on music in Jewish life, mainly to hew out my own voice and perspective within the subfield of Jewish music. I later came to realize that by offering this course, I placed myself in a position to dwell in the same field my advisor and her students had explored before me. Progressing through my teaching plan, I would find myself retracing the footsteps of my Syrian Jewish research forebears. My experience with Syrian Jewish life and music, until then achieved through contact with its researchers, would require me to return to the field through the medium of the classroom.

Scholars such as Nazir Jairazbhoy have made attempts to revisit the fieldwork of their predecessors, sometimes several decades after such research had been completed (1991). To do so, however, these scholars have often had to go searching far from the academic world, back into an imaginary and intensely theorized sense of the field: a realm of experience long perceived to be at odds with the established platform of university teaching. In my case, however, I found the "field" overlapping with the academic world, a situation in one sense as commonplace as it is extraordinary. While teaching my Music in Judaism course, I found I not only had

the ability to observe the impact of the Syrian-Jewish team research project twenty years later, but I also served as the next link in a chain of relationships between the Syrian Jewish community and scholars at New York University. In a sense, I found myself initiated into Shelemay's team fieldwork project in ways that both offered an extended view of its impact and deepened questions about the ethnomusicologist's place within broader discourses of musical tradition.

Entering the Field

New York University has long served as a place of education for children of the Syrian Jewish community, due in part to its proximity to Brooklyn and what appears to be a growing Syrian Jewish presence in Lower Manhattan (indicated by the recent establishment of a Syrian synagogue geared specifically for young people). I had already come to recognize NYU's status among Syrian Jewish students in my previous semester of teaching, when a student from the community openly discussed his background in his written assignments. The pre-enrollment for my Music in Judaism course, moreover, included at least two names I recognized as Syrian Jewish from Shelemay's writings. Thus, in constructing my syllabus, I made sure to include a date for Syrian Jewish music—to address that population, which I knew to be living in the area and present on campus, and to recognize the significant amount of work published on the community by scholars of Jewish music. My intent, however, went little beyond offering a somewhat standard lecture, based on the materials I knew from my earlier teaching experience.

Such plans changed dramatically right after the opening class session. Amid the normal flurry of questions, one student approached me and said: "My grandfather is a Syrian Jewish cantor. Would you like me to invite him to come on the day of your Syrian Jewish music lecture?" Although I had invited other practitioners of Jewish musical traditions to present for my class on other days, I was immediately awestruck. It was one thing to teach about the musical practices of a community of people based on what I had read; but it was yet another to do so face-to-face with a person I would later recognize as a primary informant for that literature. I excitedly said "absolutely," while thinking of the new care I would need to take in composing my lecture for that day. Though I did not realize it at the time, that classroom exchange also constituted the start of my "return" to the field, as I unwittingly picked up where Shelemay and her students left off.

By the time rosters had settled down, I found myself teaching a class of thirty students, three of whom identified as members of the Syrian-Jewish community. For the first several weeks, lectures proceeded according to plan. By the end of March, however, a rescheduling of the class sessions caused me to move the Syrian-Jewish music lecture to the seventh day of Passover, a religious holiday for observant Jews. Immediately upon recognizing this shift, one of the Syrian Jewish students sent an e-mail asking me to change the date of the lecture:

id just like to say that I dont think its fair to schedule the class on Syrian Judaism [on the seventh day of Passover], when the three syrian jews in the class wont be there . . . if there is any way you can reschedule that class for [after Passover], I would really appreciate it. Im very interested in the topic and would hate to miss it. (e-mail communication, April 1, 2004)

The dilemma presented here provided a first glimpse into the complex relationship the Syrian Jews in the class would have with those who studied their musical traditions. I viewed my Syrian Jewish students as veterans who had spent their entire lives immersed in *pizmonim* and *sebet* celebrations, and therefore far more knowledgeable on this topic than I. To them, however, *I* had become the authority figure who would teach *them* about Syrian Jewish musical practices, and perhaps even publicly legitimize the tradition within the larger context of Jewish music. Jennifer Kotilaine has recounted how a Ghanaian student in her section of an African music class contradicted her lesson for the day by claiming it did not conform to the practices in his village (1998). In contrast, my Syrian Jewish students looked to me to tell them about their community's musical practices. Regardless of my own (nonexistent) personal experiences with *pizmonim*, my professional status as an academic instructor and my knowledge of the literature seemed to afford me an implicit sense of authority. What I did not fully grasp at the time was the extent to which such authority was associated with my former advisor's team research project. I would later understand that the work produced by Shelemay and others had crossed into the community, becoming an important source of documentary information for Syrian Jews. Accessing this information meaningfully, however, seemed to require another trip to the field—in this case, the classroom—to recreate the academic/Syrian Jewish ethnographic encounter that had fostered the studies in the first place. Suffice it to say I reshuffled my class on *pizmonim* so the Syrian Jewish students could attend.

The bilateral nature of my classroom/field encounter gained sharper focus shortly after the e-mail exchange, when all three of my Syrian Jewish students submitted proposals to write their final course papers on Syrian Jewish music and its role in maintaining their community's religious traditions. Each also asked me if I could provide appropriate sources. Their requests set up another classic dilemma: I wanted students to know "the literature" on the topics they wanted to study; I felt responsible for helping the students gain analytic insight into the culture with which they identified; and yet, I did not want them to forsake their own experiences and observations in favor of "authoritative" material. I thus dutifully made reference to Shelemay's work, Mark Kligman's dissertation (advised by Shelemay after the Syrian Jewish team project), and a few other articles; Joseph Sutton's works on Syrian Jewry had apparently become commonly known in the community (1979; 1988). By mentioning these materials, I tried to frame the resources as vessels for offering a perspective on the students' own experiences.

Students more generally seemed wary about incorporating their own experiences into an academic essay, a disposition I had noticed while grading the class's first paper. I thus hoped the Syrian Jewish students' conscious roles as active participants in the community they intended to study would help them create dialogue with the secondary sources, rather than perceive those sources as immutable fact.

In the meantime, I prepared for my lecture.

To Teach and To Learn: The Classroom in the Field

I arranged for my student's grandfather to come in on the day of the Syrian Jewish lecture. He did not wish to present formally, he told me. Rather, he preferred to come to the front of the classroom and say a few things about the music and the community, mainly as an addendum to a presentation I described to him in advance. My challenge thus presented itself: to create a lecture on Syrian Jewish music (with a focus on *pizmonim*) that would not only correspond to the Syrian students' perceptions of the music but also would not contradict the knowledge of one of the field's principle informants. For weeks I envisioned making a wrong statement, or framing the subject in a way that would lead to open dissent, inviting a "not in my village" moment that would reveal me as a fraud in the class and alienate me from the Syrian community.

Thus, when the day of the lecture came, I found myself in a place that seemed to reek of ethnographic backwardness. I was delivering my first class on Syrian Jewish *pizmonim* to my students, three of whom were wide-eyed members of the Syrian Jewish community, one of whom sat beside the person who provided much of the information in the first place. To me, the day had the potential for colossal failure (particularly when I realized about an hour before class that I had left my lecture notes at home); but the reality was far different. While wildly trying to reconstitute my notes and gather my materials in the lecture hall, my student and her grandfather walked in. I greeted the grandfather—a real gentleman—and he greeted me and presented me with more ethnographic material: a recently recorded CD of *pizmonim* and an equally recent book of *pizmon* texts dedicated to a prominent member of the community. Just after the pair took their seats, the grandfather reached into his bag and pulled out a personal copy of Mark Kligman's dissertation, complete with Kligman's heartfelt dedication on the front cover. Then he asked me if I was familiar with the work. Though the connection seemed quite sensible, I could hardly speak. The study I had seen as important for understanding Syrian Jewish music was not just a piece of scholarship to my esteemed guest, but also a memento of a personal relationship.

The class began. Everyone sat quietly through my 45-minute presentation; none of the Syrian Jewish attendees provided any technical gloss on my words whatsoever, unless I made specific requests for confirmation. Even the grandfather/cantor was attentive, enjoying the musical examples and *pizmon* histories I had

taken from Shelemay's works (1998, 2006[2001]: 232–253). And although I invited any of the Syrian Jews present to add anything they wished on their own experiences with music in the Syrian Jewish tradition, they mainly stayed silent. When they *did* comment, however, their utterances presented a much more personal form of verification and contextualization, heightening the ethnographic encounter for me as much as it did for the rest of the class. With nearly every musical example I played, the student who had brought in her grandfather would respond excitedly, recognizing a relative of hers singing, and once recognizing the grandfather himself. Another student, after looking at the book of *pizmonim* the grandfather gave me, pointed out after class that his father had been one of the organizers for the event that facilitated its publication. Each of these comments generated a moment of cultural intimacy, powerfully portraying the "musicians" I presented in class as beloved family members, and adding depth well beyond my presentation of the texts and recordings. Other students took note of these responses as well, with one later expressing near astonishment at the vibrancy and immediacy of the tradition. On a personal level, however, I began to question my own legitimacy as someone trying to "get away with" presenting the music of this community when my knowledge of it clearly paled in comparison to four of the people in front of me.

My feeling of guilt intensified as I asked the grandfather to present musical examples for the class. He sang a few selections, and then asked for questions. Two students raised their hands; he answered their inquiries. Then silence. The grandfather did not seem fazed, but I tried to supplement with my own questions to maintain what I thought was a golden opportunity for the students. After answering one more student's question, he thanked the class and sat down to light applause. He seemed content to be there mainly to support both his granddaughter and the musical tradition he had practiced for so long.

Once the class ended, I thanked both grandfather and student. The grandfather engaged me in conversation about broader Sephardic culture and music, asked me to send regards to Shelemay, and then mentioned that this had been the *second* class visit he had done: the first had been to one of Shelemay's classes nineteen years earlier. He invited me to contact him any time, and then exited the room with his granddaughter. Once the two had left I heaved a huge sigh of relief, feeling as if I had dodged a bullet. I had not been assailed for ignorance of the tradition I had attempted to present; the presentation, I later reflected, served more as an ethnographic initiation. Picking up where the research team had left off, I became the next person to "work" on Syrian Jewish music, if only because of my status as an NYU professor specializing in Jewish music, and my place as a descendant of the academic ensemble that had conducted the initial research. Conversant in the scholarly materials produced on the community up to that time, as well as the process by which they were created, I became a vessel through which one of the main informants could provide updates, and through which the children of

the community could understand their past. My follow-up e-mail messages to the grandfather and granddaughter followed similarly: they were as thankful to me for their presence in the class as I was to them.

Conclusion: A Life in the Field

Anthropologist Heike Behrend comments: "It is already becoming apparent that in future, anthropologists will increasingly be confronted with an (ethnographic) reality that they themselves (together with the subjects of their research) have created" (1999:5). I found myself in an ethnographic arena facing such a reality when I entered the classroom for my Syrian Jewish music lecture. My decision to include Syrian Jewish material in my syllabus had come in large part from my work with Shelemay; and once the class had begun, I looked forward—idealistically—to the opportunity of building up to, and then deferring to, my Syrian Jewish students' experiences. Yet when facing the young members of the community and teaching the material I had experienced secondhand, I felt I was looking in a mirror: their knowledge seemed to come mainly from the publications I had assigned and suggested to them. Had the classroom become a surrogate or supplementary venue for transmitting the tradition? Had I, who learned about the tradition from outsiders and had striven for a layered, if overly academic, understanding of it over the years, become a sanctioned transmitter of Syrian Jewish musical ideas?

Many ethnomusicologists, including me, would never be so bold as to say yes to such a question. Yet perhaps, when framed differently, that answer is worth rethinking. As ethnomusicologists have argued since the formation of the field, musical tradition constitutes more than just the sound people produce, or the technique required to produce it; it also comprises the swirling complex of discourses *about* that sound that give it context and meaning within a community. Such discourses often define not only the sound itself but also the spaces within which the music can continue to exist; and whether intentionally or unintentionally, maintaining these spaces has become an important (if not foundational) aim of ethnomusicology as well. The collaborative nature of ethnomusicology thus tends to mean that informants may aspire to be as comfortable in ethnomusicological spaces—both discursive and actual—as ethnomusicologists in their informants' spaces.

Ethnomusicologists, in short, have often earned the confidence of the communities they study by presenting themselves as people adept in preserving and discussing a community's practices—in print, on recordings, and at public events, regardless (or even in spite) of any intellectual or theoretical interests they might also harbor. The choice of a community's musicians to collaborate with a representative (or, in this case, a team of representatives) from academe thus can lead to two entwined streams of tradition. Researchers, in one stream, can add a well-documented and meticulously analyzed case study to an ever-growing body of

research meant to address broad questions about sound and human nature. Leaders of a community, meanwhile, can find in academic partnership a safe haven for reinforcing the borders of sonically fostered identity, satisfying their own moral imperatives to preserve those cultural materials considered important to the community leaders at the time of research. Each stream of discourse can exist independently; but the two also complement and cross over at frequent intervals, as Shelemay described in her *Shadows in the Field* essay (see also Rees 2002).

In the case of Syrian Jewry, the academic study did not thus constitute an end in itself. Rather, based on my classroom experience, it gradually transformed into one stream of Syrian Jewish musical transmission—a stream reliant on academics and academic discourses for propagation. Shelemay published *Let Jasmine Rain Down* over a decade after completing her initial team field research; and by the time I had assigned a section of her book for my class, the earliest material from the study was nearly twenty years old. Yet my students made no attempts to update the *pizmon* scene for me, or even assert the aged nature of my introduction to the tradition. Rather, they met my lecture with interest, respect, and enthusiasm— and, most important, they saw this material as the basis for additional personal research on Syrian Jewish musical traditions. What had been *descriptive* in the context of the Syrian Jewish music team project had become *prescriptive* material for continuing the tradition within the community, even to the descendants of the informants.

I thus realized the long-term impact the team research project had on the Syrian Jewish community. The materials produced from the project had turned into a legacy, codifying practices kept by older generations, and helping teach these practices to younger generations even as they ventured outside their community into the university lecture hall. While the project's immediate activities benefited the Brooklyn Sephardic Center's collections and mission, its longer term effects had produced an institutional space for Syrian Jewish ethnic identification within the collegiate environment: my classroom. By attending a lecture on Syrian Jewish music, then, both Syrian and non-Syrian students could learn about Syrian Jewish life through academic materials also valued by the Syrian community itself.

This episode poses a conundrum within the world of the social sciences. Can fieldwork (in which the ethnographer poses as a problematic learner) and teaching (in which the ethnographer poses as a problematic authority) coexist dynamically within the same space? Certainly ethnomusicologists have not had problems framing the classroom as a "field." Typically, they examine educational experiences as cultural systems in themselves, usually while posing as disinterested observers (Kingsbury 2001[1988]; Nettl 1995; Wolberger 1993; Campbell 1998). Yet ethnomusicologists resist roping their own classrooms into their conceptions of the field. J. Lawrence Witzleben, for example, derived his critique of ethnomusicology's Western-ness from years of teaching Chinese music to Chinese students at the Chinese University of Hong Kong—yet when writing the article, he favored a

more theoretical level of discourse, eschewing material from the classroom experience itself (Witzleben 1997). Ted Solís's edited volume *Performing Ethnomusicology*, moreover, largely constructed the teaching of world music ensembles as a process of bringing a musical tradition from afar into an institutional home environment that represented a contrasting culture, even as many of the accounts included in the collection could be read as calling for the collapsing of such a construct (2004; see also Gupta and Ferguson 1997:12–15). Ambivalent, multivalent constructions of the field can prove problematic within the institutional research culture of the university as well. University internal review boards often emphasize a strict division between the classroom as an educational forum and the classroom as a fieldsite, rejecting issues of ethnographic ambiguity outright over concerns about student coercion.[1] Yet my experience with the Syrian Jewish project suggests just how muddled such an attempted separation can become, particularly when reentering a field already defined as crossing those borders through the work of forebears. As much as ethnographers would like to frame, delimit, and theorize the "field," therefore, they also must consider what might happen if the field suddenly appears in the very places they perceive to be most immune to it. We as researchers and teachers must consequently consider how to address this murky, theoretically ill-defined area, which can offer rich perspectives on the lives and afterlives of our research projects. While aiming to teach the findings of our colleagues and mentors, we may find ourselves suddenly re-immersed in their projects, engaging in dialogue with informants who also happen to be our students. Thus, the field-work process reopens whether we will it or not.

To ethnomusicologists, the field often comes and goes through music: the shadows, in a sense, that confirm the field's presence. But these shadows also bring with them additional layers of memory. The sound of the *pizmonim* I played in class, in the presence of a new generation of Syrian Jews, seemed to energize a new phase of transmission within the Syrian Jewish community—with me implicated as an agent in the transmission process. Had I not chosen to cover Syrian Jewish *pizmonim* in my course, I never would have become involved in reviving the ethnographic encounter Shelemay had initiated. Yet Shelemay's work comprised an important part of my professional training, as well as a compelling case study within my course's subject matter. I, consequently, found myself playing Syrian-Jewish music at a crossroads of knowledge where academic and ethnographic streams flowed together. My Syrian students and guest reminisced with each example I played; but I, too, hearkened back to my student years with these sounds, reminded of the experiences that initiated me into the discourses of fieldwork and ethnography.

So I became, at once, a political figure, an informant, a fieldworker, an advisor, and a descendant of the ethnographic "family" that had started the process in the first place. I had unwittingly planned and then created the space for my field encounter with Syrian Jewry, mediated the exchange, and then marveled at the

aftermath. Like my students and the day's guest, I constantly shifted in that classroom: from observer, to authority, to learner, to presenter, and eventually (here) to author. Yet I also recognized that my role was in some ways pre-determined, given shape by those who had come before me, and realized through the trajectory of my career path. I thus entered the space left by Shelemay and the team research project, continuing, however briefly, where they had left off.

We, as ethnographers and ethnomusicologists, are essentially chroniclers of relationships: between people at least as much as between people and sound. Although it is now relatively common for ethnographers to speak of the long-term relationships they hold with their informants/research partners, it is important to consider that these relationships also constitute encounters between communities: and that these encounters do not stop at the borders of some imposed concept of the "field." Shelemay's project had marked an initial meeting between Syrian Jewish musicians and ethnomusicologists, and helped create a warm rapport and common ethnographic language that continued to maintain itself decades afterward. I, in turn, represented a later generation: a member of the academic community legitimated to the Syrian Jewish community by both my position at New York University and (more important) by the identity of my dissertation advisor. Fitting into the space the Syrian Jewish project had inhabited years before, I gained the opportunity to pick up where the team had left off, forming relationships with past informants, using my inherited knowledge to instruct the Syrian Jewish community's own progeny, and bringing the classroom back into the field. Thus, almost two decades later, I became a part of the "team," continuing my predecessors' work and adding my own chapter to this rich and long-standing partnership.

Note

1. See, for example, chapter 11 of the tutorial for New York University's Committee on Activities Involving Human Subjects, http://www.nyu.edu/ucaihs/tutorial/11/ (accessed on 7 February 2008).

What's the Difference?

Reflections on Gender and Research in Village India

In 1974, coincidently about a year before I first heard of a field called ethnomusicology, Edward Ives wrote the following in a manual on field research. About setting up the initial interview, he said, "There are two questions that students often ask at about this point. The first, and by far the more common, frequently comes out something like this: 'Do you think it's going to make a difference that I'm a girl when I go talk to Mr. Bilodeau about lumbering?' My answer is usually, 'Of course it's going to make a difference, but I can't tell you what kind of difference.'" Ives goes on to say a bit later, "Just about every time I have predicted how the man/woman of it would work out in some particular case, I have been wrong, which means that I have stopped predicting" (1980:37). This may be the wisest statement that I've come across concerning the difference that gender makes in field research, although it probably wasn't the answer the student was looking for.

This chapter concerns the differences that gender, or more precisely gender identity, may have made in my own research in village India.[1] I say *may have made* because even in hindsight we can't always tell. I'm considering the issue with respect to two rather different field research projects. The first was field research for my dissertation, carried out for five months in 1981 and another thirteen in 1983–1984 in villages in the southernmost region of Bihar, an area of mixed Hindu and indigenous (the preferred English term there is "tribal") population. This is research that exists now only as recollections of various sorts. There are my journal notes, in which the roles and identities discussed here (researcher, writer, female, musician) are freely intermingled and undifferentiated; there are moments, musical and otherwise, frozen in tape recordings and still photos; and then there is that very particular distillation of recollections, the dissertation. The second field project to be considered here is one that, at the time of this writing, was yet to be—a research project eventually carried out during spring and summer 1993 concerning

a predominantly Hindu performance tradition in the same area. When this chapter was originally written in November 1992 this was research that was yet only imagined, but very actively imagined, and so in some ways more real than the earlier.

The two projects are quite different in their subject matter. In the first, I was interested in the musical exchange between two groups of people, the tribal Muṇḍas and their Hindu (Nāgpuri-speaking) neighbors. I was interested in concepts of culture and ethnicity, and constructions of otherness, from a Muṇḍāri point of view. In the course of my field research for this first project, I became acquainted with a few women within the Hindu community who sang and danced professionally at village weddings and festivals, and who are also what we might call concubines, courtesans, or in some cases prostitutes. These women, called *nacnis*, and their performance tradition were the focus of the 1993 research that was in its planning stage at the time of this writing. The issues I'm confronting in this second research project specifically concern gender identity and the female musician in India.

It was the subject matter of my second research project that prompted these reflections about the impact of a researcher's gender roles and identities on her work, not simply in "the field" but also in the translation of that field experience into a written form. I became particularly interested in the issue as I set out to do fieldwork again in India in 1993 because of the intense disjuncture I experienced in the earlier project between the experience *there* and the writing that followed, *here*.[2] Clifford Geertz and other anthropologists reassessing the relationship between fieldwork and ethnography have noted the difficulty of this enterprise, "the oddity of constructing texts ostensibly scientific out of experiences broadly biographical" as Geertz has put it (1988:10).

In my own case, I think I speak of field research and writing as separate activities, at least in the earlier project, because that was my expectation going in. This is often the case for those of us who do research outside North America. Our field research is clearly bounded by time, space, "culture," and language. We experience a very real dislocation when we go to "the field." We know that our time there is finite, and it will be difficult to return once we leave. Every moment should be spent doing research, attending events, talking to people, and making music, rather than writing. What's more, when I work in villages in India, I am also relocated ouside the academic world. My friends and research partners there don't spend their time sitting alone, reading and writing. Such activity is downright antisocial. And everyone knows you can't learn music or dance by reading and writing about them. The physical circumstances of my research and the very nature of living in village India certainly contributed to the disjuncture I experienced in making the transition back to academic life upon my return.

One could argue that such disjuncture between the research experience and the writing are common among anthropologists and ethnomusicologists, regardless of the gender factor. As Mark Slobin put it in a recent article in *Ethno-*

musicology, we all grapple with the "problem of welding the disparate strips of observation into a finished work of analysis" (Slobin 1992b). An aspect of that problem is our imperative to spin generalized analyses of culture and traditions out of intense, particular experiences with only a few individuals.

Still, I think that the differences in my gender identities, both chosen and assigned, within my academic world here and in "the field" in India were also a factor. The disjuncture was particularly intense because central aspects of the experience—related to my roles and identities as female and as musician and dancer—were left out of the writing (or, you might agree by the end of this chapter, were deeply buried in it). I should note that it was never a matter of consciously suppressing or editing out the experiential in the writing. But I knew as it developed that the written report was much more distant from the experience than I would have liked it to be. Not incidently, those buried experiences were also the most emotional, sensual, and physical aspects of my being in India. I don't want to simply rechew old experiences here. I am interested in the implications of such a distillation of the experience in the writing as I proceed with the second research project, one that explicitly concerns issues of women (including myself) as musicians and dancers. Can I avoid the same disjuncture? Should I?

The Ungendered Researcher

Although it isn't my central purpose here to examine why I feel that these aspects of my experience—as female, musician—were suppressed in the first project when it came to writing the ethnography, I will digress for a moment and offer a bit of speculation. It concerns gender identity in the American academic world.

In my academic studies, somewhere along the way, I developed a conceptualization of the scholar (researcher, writer) as ungendered, or gender neutral. The idea was reinforced by both personal experiences and institutional paradigms. In graduate school, first at the University of Minnesota and then at the University of Illinois, some professors questioned the ability of female students to do field "work" (lacking the necessary physical strength) or even to pursue a career (distracted as they were by marriage and children). My female classmates and I came to understand that being female was risky in this environment, so we often chose not to expose our gender. We were working hard to enter an obviously male-oriented profession, and we wanted to be perceived as no different from the male students. Gender-neutral scholarship, we thought, worked to our favor.

Our perception of the gender-neutral scholar was reinforced by official discourse in ethnomusicology. In my student days, gender was discussed sparingly, if at all, in textbooks and courses on fieldwork. Edward Ives notwithstanding, most of our written guides to doing field research—Hood, Nettl, Merriam, Goldstein, Karpeles—did not mention gender at all.[3] In some of my courses, the gender of the researcher *was* discussed in terms of access, rapport, and role expectations.[4] We

were encouraged to be flexible and sensitive to the impact of our gender, but the field methods and techniques themselves were universally applicable and ungendered, as were the models for writing.[5] After all, we were all after the same information, the same knowledge. Managing gender relations in the field was a personal, private matter. Those aspects of doing field research that were most closely related to the experience of being female in the field were, and to a large extent still are, discussed not in the official discourse about field research but in a sort of unofficial, underground discourse in which women (and sometimes men) shared experiences and advice about managing sexual miscommunication and harrassment, conflicting role expectations, gender relations, female hygiene, and sexual relations in the field. To some degree I think this was and is our choice; we still have a stake in maintaining the illusion of the ungendered scholar.[6]

The paradigm of the ungendered scholar has been sustained almost uniformly in our models for the written ethnography. In many of the classic ethnographies of music-culture, the subjects themselves (the 'Others') are unmarked for gender, though most often, in reality, they are male. And even when the subject *is* gender, the author's gender typically is not part of the text. Like her male counterparts, she's an omniscient voice, an ungendered observer, reporter, and interpreter.[7] Although intense participant-observer-based ethnography was once regarded among feminists (sociologists, notably) as a qualitative, "feminine" alternative to more positivist, abstract, "masculine" methods, the social science paradigm for written ethnography has remained unchallenged until recently, when it has come under attack by feminist scholars such as Lorraine Code (1991), Katherine Borland (1991), and Judith Stacey (1991). As they see it, the paradigm, with its objectification of experience, "denigration" of emotion, potentially exploitative methodologies, unidirectional flow of information (source to scholar to academic audience), and imperative to "take a stand and defend it" is not gender neutral at all, but inherently androcentric.

Well, probably so. It *is* the paradigm, modified a bit, upon which much of my dissertation, my written distillation of those field experiences of 1981 and 1983–1984, is based. In an effort to avoid the appearance of writing the "truth," I contextualized my very particular interpretations of Muṇḍāri and Nāgpuri musical interaction within my own experience in an account of fieldwork that conventionally prefaces the body of the work as a long section of personal, reflective remarks; the remainder of the dissertation is punctuated occasionally with vignettes that remind the reader that "I was there." Throughout most of the text, however, the ungendered scholar prevails. This is not to say that it does not represent something of my research experience or that it is somehow dishonest or invalid. A written account of field research can be nothing more than a sorted and sifted reconstruction of that experience. In fact, the dissertation passed several "validity" tests: it rang true to my Muṇḍāri mentor, Dr. Ram Dayal Munda's own interpretations,

it did not offend or embarrass anyone, and it met the expectations of research partners and scholars in India whose support I value. Leaders of local political parties and tribal youth organizations now quote from it to support and validate their struggle for cultural identity and autonomy. But because of its bow to the social science paradigm, it exists quite separately from the field research in my recollections, particularly with respect to my experiences in Bihar as female and as musician and dancer.

Gender in the Field Experience

To consider how gender identities shaped my previous field experience in villages in Bihar in 1981 and 1983–1984—and to let the reader in on what was left out of the written work and why—I'd like to quote from the introduction to the dissertation. I've chosen one of the more personal, reflective statements, one that tells the reader something of the nature of my interaction with Muṇḍas (an indigenous or "tribal" group) on the one hand and Nāgpuri-speakers (predominantly Hindu) on the other. In this passage I describe a party thrown by myself and the three tribal women with whom I lived in a rented, tin-roofed, bug-infested house in Ranchi city. The house was a duplex, with separate entrances to each side, and two rooms per side. This was a sort of farewell party, as I was approaching the end of my stay. We had invited many of the people with whom I had worked during the year, Muṇḍāri and Nāgpuri, and a few other friends.

> The way in which our guests arranged themselves on the night of that farewell party revealed much about the nature of my field experience of the previous thirteen months. Nearly all of our *ādivāsi* [tribal] guests, most of them Muṇḍas in roughly equal numbers of men and women, gathered on the side of the house in which Charia, Madhu and Asrita had their rooms. The Nāgpuri and Bengāli guests, mostly men, as well as our Nigerian friend [also a man], gathered in the music room, next to my room. On one side, then, was a socially homogeneous group, nearly all Muṇḍas, all of the same status, belonging to one endogamous social group, speaking the same language, but of mixed gender; in its make-up it was a group typical of Muṇḍāri social gatherings. On the other side, not coincidentally my side, was a predominantly male but socially heterogeneous group of people of different statuses and occupations, different endogamous groups, and different native languages, conversing with each other in two common languages, Nāgpuri and Hindi. At some point, as the night progressed, the guests on both sides of the house began making music. There was Muṇḍāri drumming and group singing and dancing by the *ādivāsis* (Muṇḍas and a few Oraons) on the one side, and Nāgpuri drumming and solo singing, without dance, by individual Nāgpuri men, most of them specialists in stage performance, on the other. (Babiracki 1991:7–8)

The description of the party continues a bit later:

> Throughout the night of the farewell party, I moved back and forth between the two sides of the house, trying to distribute my time and attention equally between these two different social and musical groups, as I had during the course of my research. Even more interesting, those individuals who had been most closely associated with me as research companions and assistants—Mukund Nayak [a popular Nāgpuri stage musician] and the three women with whom I lived—likewise moved between the Muṇḍāri and Nāgpuri sides of the house. Mukund and Madhu [one of my Muṇḍāri housemates] even made attempts to participate in one another's music making, just as I had been doing during the last thirteen months. For the most part, though, those who "crossed over" simply observed the music making of the other side. My participation in both was more complete, as it had been throughout my research. (Babiracki 1991:9)

The excerpt tells you a good deal about what I perceived to be differences in male and female roles in Muṇḍāri and Nāgpuri music-cultures. It represents Muṇḍāri music-culture as unsegregated by gender, and communal and egalitarian in participation, and Nāgpuri music-culture as segregated by gender, and dominated by male instrumentalists and soloists. It is an accurate representation of what I still consider the significant differences between these two music-cultures, and also of what was my very different participation in and experience of each, but it tells you nothing about how my personal, gendered experiences and my various gender identities may have shaped my perceptions in the first place.

By gender identity (as opposed to biological, sex identity), I mean the particular constructions of female, male, and things in-between that one chooses and/or is assigned in particular situations. As you will see, the identity I chose for myself and the identity others thought I had chosen were not always the same (or so I think now). I also want to add that gender was only one of many factors that shaped my experience. Age, status, race, language, education, physical appearance, political ideology, concepts of individual and group, and many other factors, all of them interconnected, contributed.

Several months into my field research in 1981, Mukund Nayak, a Nāgpuri friend and research partner, confessed to me that when they (my new Muṇḍāri and Nāgpuri friends) first met me, they had concluded that, since I was apparently unattached, not "going with" any man *or* woman, I must be "neutral" (he used the English term). Looking back on it now, I see that I eventually established my humanness among Muṇḍas and Nāgpuris in very different ways, in both cases through gendered, personal relationships involving active participation in music and dance.

Among Muṇḍas

Muṇḍas establish new relationships with each other and with "outsiders" (a loose translation of their word *diku*) on the basis of existing relations. I was first in-

troduced to the women who became my housemates by Dr. Ram Dayal Munda, a linguistics professor with whom I had studied at the University of Minnesota and who became the head of a department and for a time Vice-Chancellor of Ranchi University. Dr. Munda introduced me as his sister-in-law, the "sister" in a fictive or extended sense of his American wife at the time.[8] My extended kinship relations with my housemates, and through them with every other Muṇḍa I met, were all determined by that first introduction, and from then on kin relations determined with whom I could talk or joke, those I had to avoid, and those I could count on for help—in other words, those to whom I had access for research purposes. After Dr. Munda introduced me to my housemates, he had little to do with the day-to-day activities of my research, though he continued to guide my interpretations. In village situations in which I felt I could not fulfill the expectations of my assigned, female roles and still accomplish my research objectives, my housemates, who traveled to villages with me, became mediators, explaining me and my work to others, and sometimes even taking on my expected household roles themselves.

Maintaining my assigned, female identity while documenting and participating in Muṇḍāri communal song and dance events proved more difficult. At times their expectations of me and my behavior conflicted with my need to be a researcher. These dance occasions are highly gendered events. In the dancing ground the roles of men and women are clearly defined—men play instruments, dance in a sort of freestyle, and introduce songs; women dance a repeated step pattern together in a tight line and respond to songs—and there is a very specific etiquette of coded male-female behavior. One of the purposes of communal singing and dancing is to bring together potential marriage partners at periodic festivals to give them an opportunity to get to know each other. I was expected to join the women's line, usually somewhere near the head, and eventually to take my turn at leading. It was a role I gladly accepted and thoroughly enjoyed, but one that precluded documentation of the event or conversation with others—particularly men—in attendance. My solution was to document as much of an event as I could early in the event, before my age and gender identities were clearly established and understood. With tape recorder in one hand and microphones in the other, I positioned myself between the head of the women's line and the group of men dancing in front of them, then simply moved around the circle with the flow of the dance, neither male nor female—the ungendered researcher. Once I put my equipment away and joined the line, in effect declaring myself female after all, there was often no going back. The women usually refused to let me leave the line or pick up my tape recorder again for any length of time.

My research methodology among the Muṇḍas was also defined by the ethnographer's imperative, a desire and need to understand all aspects of their music-culture. It is probably in the ethnographer's attempt to fulfill this directive that his or her gender has its most noticeable impact on the final representation, although that impact often goes unacknowledged.

I had already played with assuming both male and female roles in Muṇḍāri music making long before my first research trip, as a member of a Minneapolis-based music and dance troupe established by Dr. Munda to present stage performances of the songs and dances of his people in performances throughout the Midwest. I alternated female and male roles in those performances: singing and dancing with the women in one half, and playing flute and dancing with the men in the other. Dr. Munda insisted, for the recreation to be authentic, that I must take on the role of a man completely when playing the flute. I dressed in a *dhoti* and *kurtā*, with my hair in a topknot, and shadowed his dance movements until I could move like a man. Troupe members dubbed me "Little Munda." Once in India, with Dr. Munda's encouragement, I continued to play Muṇḍāri songs on the flute in stage programs and with small gatherings of men in his village. And I learned to play the large kettledrum (*nagāṛā*), like the flute a male instrument. There was no taboo against women playing these instruments, and no penalty for doing so, but women normally didn't. Dancing as part of the group of men in the dancing ground would close off the kind of playful interaction with men that women seek in the dance. My housemates were intrigued by my "boldness," my crossing of gender roles in this way. Eventually each one tried her hand at playing the flute. Only one continued to play after I left, and then seldom in public. Only in Dr. Munda's native village did I feel free to play the flute and kettledrum in the dancing ground, dancing with and like the men, though without my earlier change of costume. The men seemed amused and accommodating.

It occurs to me now that my crossing of gender boundaries, mixing of gender roles, and creation of new roles (the ungendered researcher) may have contributed to my perceptions of the equality of men and women in the Muṇḍāri communal dancing ground. This characterization of Muṇḍāri music-culture as egalitarian was also influenced by Dr. Munda's own representation of his society. His desire, in light of current political movements, to represent it as egalitarian is a way of distinguishing it clearly from hegemonic Hindu society. The fact that my play with roles was acceptable, albeit unusual, to other Muṇḍas lent validity to his interpretation. I accepted readily the illusion, created in part by Dr. Munda, that I had participated in both male and female aspects of Muṇḍāri music-culture. In reality, I am sure that Muṇḍāri experiences of the dancing ground, whether as men or women, are to a great extent individual and situational, as were my own.

Among Nāgpuris

The picture of Nāgpuri music-culture I presented in the excerpt from the beginning of my dissertation, that of a music-culture segregated by gender and dominated by male solo performers, in its own way reflects the nature of my gendered experience of that music-culture and my relationships with Nāgpuri musicians. It

represents only that part of Nāgpuri music-culture in which I most actively participated: public stage performances, dominated by men. My role among these musicians was mediated by one man, a musician named Mukund Nayak, who is one of the stars of the Nāgpuri stage. Mukund is a singer, poet, drummer, and dancer—and a Ghāsi, a member of the traditional village musician caste. It was my close, romantic friendship with Mukund that probably first made me human in the opinions of my Nāgpuri acquaintances.

Mukund and I met during my first, "preliminary" research trip of 1981. I say preliminary because I thought I was only looking around, not doing "serious" research; as it turns out, much of the dissertation was based on events documented and impressions formed during that first trip. Mukund quickly pulled me into his stage troupe, a group consisting for the most part of Ghāsi men, and I subsequently traveled throughout the area performing Nāgpuri songs on bamboo flute with them in village stage performances. Consequently, although I am aware of the village world and music of Nāgpuri women and occasionally participated in it, I do not have the intensely familiar experiences to draw upon in developing an understanding of them that I do in the case of Muṇḍāri women's culture. My perceptions of Nāgpuri music-culture were colored by Mukund's own representation and interpretation, just as my perceptions of Muṇḍāri music-culture were influenced most by the interpretations of Dr. Munda and my housemates.

During my first research trip to the area in 1981, the nature of my friendship with Mukund had remained private, or so I thought at the time. After five months, I returned to the University of Illinois to finish coursework and preliminary exams, write a dissertation proposal, and apply for a grant. In short, I settled back into my academic persona. When I returned to Bihar in 1983, I seriously considered breaking off my relationship with Mukund. I knew that if the relationship were to continue, it would have to become public knowledge, and I had some vague notion that such a relationship was a breach of ethics. I feared that some Muṇḍas might not approve. And I think I sensed intuitively that such a relationship would be incompatible with the role I had now embraced as "serious" researcher. The paradigm of the ungendered researcher, untainted by the female musician, had reasserted itself with determination. It didn't take long for me to realize, with Mukund's help, that to break off my relationship with him would not only sadden us both, but would betray the trust and expectations we had established during my first trip. Humanism won out; our friendship continued.

It did indeed become public knowledge, although it remained unofficial, that is, without public acknowledgement. Our friendship determined my further relationships with Nāgpuris, particularly musicians, as well as my own experience and understanding of Nāgpuri music-culture. Because Ghāsis, people of Mukund's caste, are considered by others to occupy the lowest rung in the caste hierarchy, my access to many Nāgpuris of high caste was effectively closed or constrained. To pursue close relations with them would have been taken as an insult by Mukund

and many of our friends. My friendship with Mukund also established an avoidance relationship between me and his "older brothers" in an extended sense. But the younger men who performed and traveled with Mukund and learned music from him became my "younger brothers" too. Not only did they stop soliciting my romantic attention, but they honored their kinship obligations to me of assistance and protection with amazing generosity. Muṇḍas, as it turned out, seemed little concerned by our relationship (with one exception). They had never considered me a potential partner for Muṇḍāri men to begin with, due to their strong ideas of tribal endogamy. I was a *diku* no matter what I did.

My travels with Mukund and the troupe were a source of great pleasure. Although I was still busy documenting, observing, asking questions, and so forth on these jaunts, I looked forward to them as a kind of break from my research. Mukund was the only person I saw regularly who spoke English, and my participation as a soloist in Nāgpuri stage performances was comfortable, more consistent with my self-identity as a musician here in the United States than was my participation in the Muṇḍāri communal dances. Although I took joy in singing and dancing with Muṇḍas, my travels with the Nāgpuri troupe offered me a release from the stress of adapting to the Muṇḍas' intense communalism and their censorship of behavior that calls attention to oneself as an individual. Although few other women participated in these Nāgpuri stage programs, aside from a few *nacnis* (professional village entertainers) or former *nacnis*, I rarely felt uncomfortable with this. Instead, I followed Mukund's cue, concluding that my identity as an educated white woman somehow lent respectability to my otherwise questionable behavior as a female entertainer.

My relationship with Mukund also gave me a unique insight into Nāgpuri music-culture, an insight that, like the other experiential aspects of my research, is only suggested in the dissertation. In 1981, shortly after we met, Mukund began composing songs inspired by our friendship. By the time I returned to the United States in 1984, he had composed eleven of these. Since then he has composed several more, so there are perhaps fourteen or fifteen altogether. By composed I mean that he set new stanzas of words to traditional seasonal tune types. All of these songs could be considered part of a larger tradition of songs of love in separation or unattainable love, a tradition related to and perhaps derived from Vaiṣṇava devotional poetry, in which the unrequited longing for the beloved is a metaphor for the devotee's longing for union with the divine. For Nāgpuri poets, the ostensibly devotional tradition is simultaneously, even primarily, a vehicle for the expression of sentiments that cannot otherwise be made public.

Mukund composed the first of these songs, "*piyo mainā*" (blue and yellow maina-bird), in September 1991 as I was leaving Ranchi city for a week-long stay in a Muṇḍāri village. The song is about the poet's loss of his beloved *piyo mainā*. In the song, Mukund is also invoking my eventual return to the United States, which we both knew was coming:

bahute jatana karī re	Taking great care
posalō mōe piyo mainā	I kept a piyo maina.
kāle re piyo mainā,	Why did piyo maina,
piyo mainā uṛi bana jāe	Piyo maina fly away to the forest?
bana jhāra chāin delō re	I searched every corner of the forest,
gach khuṭ se pūich lelō	I asked all the trees and stumps,
kāle re piyo mainā	Why is piyo maina,
piyo mainā nahī to bhẹtāe	Piyo maina not found?
nita dina khojī lorhī re	Night and day, searching and gathering,
khudī cunī ṭhurāe rahō	I collected bits of grain.
kāle re piyo mainā	Why did piyo maina,
piyo mainā gele bisurāe	Piyo maina forget?
tana ke pījarā mānī re	Thinking my body a cage,
man mē basāe rahō	I kept [piyo maina] in my mind.
kāle re piyo mainā	Why did piyo maina,
piyo mainā karejā bīdh jāe	Piyo maina shoot my heart [with an arrow]?
mukund binu pākhī re	Mukund, without wings,
guni guni prāṇ taij delō	Thinking and thinking, lost his life-breath.
tabe re piyo mainā	Just then, piyo maina,
piyo mainā ghurī pachatāe	Piyo maina came back and was full of remorse.

(*Translation by M. Nayak and C. Babiracki*)

The song became a hit in village stage performances and even inspired several parodies. Each of Mukund's songs that followed "*piyo mainā*" either related to a specific event in our friendship or reflected Mukund's state of mind at a particular moment. Significantly, there is no mention of these songs in the dissertation.

Identity Conflicts

The theme of love and longing in separation also characterizes the Vaiṣṇava songs sung by the *nacnis*, the professional female singers and dancers with unorthodox lifestyles who are the subject of the second research project considered here. *Nacnis* are non-"social" women of lower status, who are "kept" by men of higher status as professional entertainers. They live outside the conventions, restrictions, privileges, and protection of socially sanctioned married life. At the same time, each is Radha incarnate, free to experience and enact fully the goddess' passion of pure love and devotion. A *nacni* often sings in the voice of Radha longing for her beloved, Krishna. She herself is regarded as an unattainable object of desire, particularly when she is performing. It is her singing and dancing that make her attractive, just as it is Krishna's flute and dance that attract Radha, and Radha's dance in turn that attracts him. A *nacni*'s songs are simultaneously interpreted as secular, sensual and divine, and her performances are valued as a manifestation of

the relationship of Radha with Krishna. The *nacni* and her partner (who may or may not be her "keeper") are hired as auspicious performers at village weddings and festivals. Yet for all her cultural value, the *nacni* herself is degraded socially for her unorthodox life-style, for her flaunting of societal norms, and for singing and dancing alone in public.

During my research trip of 1983–1984 and for some time after, I thought that I had created a unique identity for myself as a female musician within Nāgpuri music-culture, or at the very least, that I had succeeded somehow in changing Nāgpuri ideas about the impropriety of a woman performing in public. I continually ascribed a gender identity to my role on the Nāgpuri stage based on my identity as a female musician in my own country, and Mukund reinforced this notion. He believed so strongly in the power of my respectability that he persuaded me to work with him to reintroduce group dancing to Nāgpuri stage programs. We began with brief interludes of dance performed on the tiny stage by a handful of musicians. Other female singers sometimes joined me in the women's line, ironically with the exception of a former *nacni*, who was trying to establish herself as a respectable stage singer. By the time I left in 1984, we were ending our all-night performances with communal dancing at dawn in a makeshift dancing ground among the audience.

Nevertheless there were moments when I was jolted out of my comfortable perceptions of myself as a female musician among Nāgpuri musicians, moments when I realized that the people around me weren't perceiving me in quite the way that I would have liked. At a village festival in 1984, late in the night, Mukund coaxed me out of a Nāgpuri women's dance celebration (segregated from the men) to observe a group of men, in another part of the village, dancing *mardānā jhumar*, a genre they usually performed with a *nacni*, though none was present at this event. I positioned myself among them with my tape recorder, moving with the flow of the dance, once again the ungendered observer. At some point I became aware of the fact that I was the only woman in this dancing ground, and that the men seemed to be singing with and to me—not to the tape recorder, but to me. I had the sudden and uncomfortable realization that perhaps they thought I was the *nacni* in this dance. I immediately began suppressing my own response to the music and avoiding eye contact in an attempt to reassert my researcher's identity. Although the ploy worked for me, I wonder now if it produced any shift in the identity they ascribed to me.

At the end of my stay, after many attempts to arrange an interview with a member of the lineage of the Mahārāja of Choṭanāgpur about his family's patronage of Nāgpuri music, I finally got the opportunity following a stage program in his village. The interview was a model of cross purposes. The rāja was rather drunk, insisted that I drink rum with him, and became increasingly aggressive in his flirting; his wife scowled at me in open hostility from across the compound; and I relentlessly, futilely, tried to invoke my neutral, ungendered researcher identity. I

realized too late that this rāja—and his wife—had both regarded me, in their own gendered ways, as they would a *nacni*. This was later confirmed by Mukund, who was there but had assumed a traditional role of court musician and so did not want to intervene.

It is quite possible that, while I thought I was constructing a new identity for the Nāgpuri female performer, one that Mukund felt would serve the advancement of Nāgpuri music, many people had in fact simply assigned to me a familiar female identity, that of the *nacni*. I must admit that the thought did cross my mind more than once while I was there, and I greeted it with ambivalence. I valued and admired the *nacni*s as musicians and performers, and their life-styles and performances resonated with some of my own ideals: behavioral freedom, individuality, economic independence, and artistic achievement. On the other hand, I was and am mindful of the disdain that most Muṇḍas and Nāgpuris (nonmusicians) I knew had for the *nacni*s as individual women. My Muṇḍāri acquaintances, particularly, see the *nacni* tradition as the legacy of a patriarchal feudal system that degrades and exploits women. The scorn was clear in the voice of Gandharva Munda, Ram Dayal Munda's father, as he responded to my question about who becomes a *nacni*:

> "Who becomes a *nacni*? Those who have no shame, who don't respect anybody. *Nacni*s are spreading. [something unintelligible] They become them all by themselves. They absorb it. They don't have any training. They won't make an [artistic] impression."
>
> Then he stopped and looked at me, "Okay, you'll become one. You could easily become one" (Field notes, Diuṛi village, 9 Dec. 1984. Translation by Carol Babiracki).

He could speak to me in this way, because he was like a "father" to me, but I *was* taken aback. Once again, the identity I had constructed for myself had run up against one assigned to me.

Implications for Future Field Research

My past acquaintance with the *nacni*s and their performances has moved me to research them and their tradition further. I was fascinated and probably a little flattered during those past research trips to find that the *nacni*s themselves were drawn to me, though in retrospect I have a better understanding of why. I am intrigued by the ambiguity of their status, the multilayered interpretations suggested by their performances, and my own ambiguous responses to them. *Nacni*s were the site of conflict in my past research between identities of female and musician chosen by me and those assigned to me, and as promised, they became the site of conflict between my models and ideologies of musician, female, and the gender-neutral social scientist in my new research project of 1993, which was anticipated at the time of this writing.

I am also intrigued by the possibilities this new project offers for exploring research methodologies and writing strategies that attempt to bridge the chasm between the field experience and writing about it. This new research presents an opportunity to put the female musician back into the ethnographer.

The paradigms outlined by feminist scholars of oral history, psychology, sociology and other disciplines seem at first glance to be promising alternatives to the social science paradigm of ethnographic research. They are also familiar to those of us who know them in their incarnation as postmodernist ethnographic methods (e.g., Clifford and Marcus 1986; Clifford 1988). These paradigms shift the object of representation from culture or a people to "fleeting" moments of discourse (Salazar 1991). They favor dialogic forms of representation that place multiple voices and interpretations into the written work and that reflect the dissonance of the research process itself (Stacey 1991). They propose interview strategies based on models of women's natural conversation, especially small-group conversations in which everyone participates simultaneously and nobody holds the floor (a nightmare to transcribe!), in a effort to shift the focus away from activities and facts (information) and "attention-getting, well-polished monologic narratives" (Minister 1991). And they teach us to extend the conversation (as opposed to the interview) between researcher and subject into the later stages of interpretation in a process of collaborative story making (Borland 1991).

These strategies are enticing but are not without their own shortcomings. The intersubjective and self-reflexive approaches tend to place the researcher herself in the center of the story, potentially marginalizing the subject and subordinating her story to the method itself. Some feminists argue that dialogic, self-reflexive approaches tend to privilege the researchers' ideologies and agendas over those of the subjects (Hale 1991; Borland 1991). And, of course, the intersubjective interview strategies that I mentioned earlier are based on observations of women's communication in North America, in the English language.

Dialogic interview strategies can create what Judith Stacey has called a delusion of alliance, a search for the self in the other, based on assumptions about the unity of women that the subject may not share with the researcher. Stacey concludes that, in her experience, the more intimate, intersubjective methods actually place subjects at greater risk of exploitation and manipulation than the more positivist research methods. She argues that ethical questions of authority, exploitation, and the inherently unequal relationship between researcher and informant/subject are not eliminated, or even minimized, by the postmodern ethnographic strategies, but simply acknowledged by them (Stacey 1991).

My difficulties in writing this chapter only underscore the concerns voiced by these scholars about the risks inherent in intersubjective, self-reflexive methodologies. I suspect that my Muṇḍāri and Nāgpuri friends in Bihar would neither understand nor applaud my self-absorbed musings, and they certainly wouldn't find that I had served *their* needs.[9] The subjective, reflective paradigms of writing

may better represent *our* experience as researchers, but for my Muṇḍāri and Nāgpuri research partners, representing the *memsāhab*'s experience was not the objective. In their view, my research was about their people and traditions. They were and are keenly interested in *how* I represent them and their traditions, that my representations are consistent with their self-image, and that they serve their personal and political aims.

It is no accident that I have chosen not to write about my more personal encounters with Muṇḍāri and Nāgpuri music and musicians before this; I did not want the writing to impinge on or trivialize my remembrance of the experiences themselves, and of course it has. I struggled with the ethics of discussing my personal relationship with Mukund in this public way. On my return to Ranchi in 1993, Mukund gave his approval of the discussion here, with some pride, although he found my asking for it rather ridiculous. Our friendship *is* public knowledge over there, after all.

What we must strive for, I think, is a keener awareness and acknowledgment of the objectives and audiences of our writing and a set of paradigms from which we may choose the most appropriate. This, too, has implications for our field research. Can one, for instance, produce intersubjective *and* paradigmatic social scientific works based on the same field research?

Just as I approached my new research project in 1993 with an assortment of paradoxical, ambiguous interpretations of the *nacni*s and their performances and a willingness to add more, I also carried a collection of possible research strategies and a willingness to be flexible.[10] But my plans and expectations were tempered with the hard-won wisdom that choices would be made for me and that what is probably most interesting about the "man/woman of it" is its unpredictability.

Notes

1. This chapter was originally written in November 1992 for presentation at a colloquium on Fieldwork in Contemporary Ethnomusicology organized by the graduate students in ethnomusicology at Brown University.

2. I want to make it clear that I see both my research experiences in India and the writing of them back in the United States as facets of my life as a scholar. Both *are* scholarship. But qualitatively they are vastly different, hence the disjuncture. Paradigms of academia, including that of the ungendered researcher, encourage such a false dichotomy between scholarship and life.

3. Thankfully, this is no longer the case. Notable collections on gender and the field research experience include Whitehead and Conaway's *Self, Sex, and Gender in Cross-Cultural Fieldwork* (1986); Gluck and Patai's *Women's Words: The Feminist Practice of Oral History* (1991); and Bell et al.'s *Gendered Fields: Women, Men and Ethnography* (1993).

4. This was particularly true in my first formal courses in field research with the folklorist Dr. Ellen Stekert at the University of Minnesota.

5. See also the growing literature exploring a feminist approach to writing, all of which have implications for ethnographic writing (Caws 1986; Cixous 1991; Finke 1992; Heilbrun 1988).

6. In the fall of 1992, I was asked to write a recommendation for a student who was competing for an AMS50 grant from the American Musicological Society. I was admonished to make no reference to the applicant's gender, race, or origins in my letter. In grappling with such an awkward task, I found that, in the end, I had also left out any indication of my own gender. We are all ungendered in the academy.

7. As examples, see any of the essays in Ellen Koskoff's collection *Women and Music in Cross-Cultural Perspective* (1989).

8. Muṇḍas do make a conceptual distinction between fictive and blood kin, although that distinction is not revealed in the terminology they use.

9. This has since been confirmed by the reaction of my friend and coresearcher Mukund Nayak to this chapter. He found it acceptable, but not very interesting or useful.

10. After this chapter was written in 1992, I did indeed undertake seven months of field research in 1993 on the new project anticipated here, a study of ideologies of gender, religion, and music among village *nacni*s of Chotanagpur with my coresearcher Mukund Nayak. Just as the on- and off-stage personas of the *nacni* and her partner are seemlessly and simultaneously intertwined, so were Mukund and my identities as partners in daily life, stage performance, and research. The flow of our research included a continual "off-stage" dialogue about whether and how I was—and wasn't—like a *nacni*. A full exploration of the play of gender in that research properly requires a separate chapter, which might address the problems of integration of the identities of female, musician, and ethnographer for the researcher (neither an easy or natural process, even when consciously sought); the consequences of privileging gender over other social identities; the benefits and constraints of cross-gender collaborative research; and the ethical questions raised by intersubjective, individual-centered research. It is rich ground for future consideration.

(Un)doing Fieldwork

Sharing Songs, Sharing Lives

In the ethnography of musical performance we are particularly challenged, as writers, to present or re-present the experiential since performance *is* experience. The project of aligning form and content—writing and experience—is one way in which a focus on field research is reshaping ethnography. One might argue that all ethnography be considered ethnography of performance, since culture itself is at some level inevitably enacted. But the relative specificity of music, while always embedded in and enabled by other performance modes, can provide a heightened example of performance processes. A focus on the ethnography of musical performance—overdue in the ethnographic arena—can suggest incisive ways of researching, writing about, and understanding cultural processes.[1]

The renewed emphasis on experience is part of a continuing seachange in the humanities that is moving us toward reflexive, nonobjectivist scholarship (and, not by coincidence, distancing us from historically colonialist approaches). During our most in-depth and intimate field experiences, ethnographers and the people among whom we learn come to share the same narratives (as Edward Bruner has noted, 1986:148; also Geertz 1988); the deeper our commitment in the field, the more our life stories intersect with our "subject's," until Self–Other boundaries are blurred. The "field" becomes a heightened microcosm of life. When we begin to participate in music and dance our very being merges with the "field" through our bodies and voices, and another Self–Other boundary is dissolved.[2]

In this chapter I address three interrelated questions, drawing illustrations from my own experience with the singing, dancing, and everyday lives of BaAka pygmies in the Central African Republic:[3]

1. Is there a way to determine what is or qualifies as field research, or to distinguish between who is or is not a field researcher? Should there even be a distinction, and if so, why?

2. What new approaches to writing are suggested by the changing, developing relationship between field experience and the ethnography of performance, particularly musical performance?

3. Are there aspects of experience that might not be appropriate to an ethnography; how do we determine when to include or describe personal matters or controversial situations?

The first question can illuminate what field research is by discovering what it isn't. How might it differ from other kinds of research, or from tourism, missionizing, or journalism?[4] What *is* "the field"—is it spatially or temporally defined, or defined by a state of mind or attitude, an openness and readiness to see, to experience, to interpret? Who does or does not do fieldwork, and why might we say so? Ethnographers use tactics different from those of travel writers, for example, to define who they are in the "field." They also create themselves as ethnographers within the narrative itself, and thereby define their experience as "fieldwork." But are we using the term "fieldwork" to bring us closer to—or to distance ourselves from—our "real life"? Fieldwork *is* often intensified life, but part of a life-flow all the same, and it is inseparable from who we are. We might, therefore, begin to look for a term other than "fieldwork" (field research, field experience?) that implies seriousness and rigor without a scientist/objectivist or colonialist connotation—incorporating the simultaneous vulnerability and responsibility of fully human relationships.

The second question suggests that one of the goals (or results) of a renewed emphasis on field experience in ethnography is to erase the dichotomy between "experience" and "scholarship," between "fieldwork" and "writing." The question is: How does ethnographic writing, and field experience itself, need to change and develop in order to facilitate the kind of writing we need to evoke experience fully? How might we integrate ethnopoetics, ethnoesthetics, and reflexive, narrative ethnography, along with forays into the ethnosensorium (Stoller 1989; M. Jackson 1989; Howes 1991), moving toward more effective strategies for describing performative interaction, feeling, sound, and movement?

With the third question I come to wonder what can or cannot, should or should not be included when translating from field experience into ethnography. Since performance-oriented scholars have acknowledged that experience is central to both research and writing, and have thereby dismantled the taboo against the "subjective," the floodgates of experience have opened. We may need to stem the tide, to rethink and perhaps redraw the boundaries of the ethnographic. Where is the border between getting at *truth* and going into a realm of the personal that is unnecessary or inappropriate for ethnographic purposes? The politically or personally sensitive, intimate points, or serious and profound self-doubt that throws one's whole project into question, spiritual crises and transformations, ethical dilemmas—when and where must they be included to present a full, evocative ethnography and how do we determine? Ethnography, like any creative enterprise,

is a re-presentation, a re-formation of experience, and we need to develop tools that help us sense when and what to include when re-presenting a part of life—of our lives.

The Elusive Field and Fieldworker

The construction of "the field," and ourselves as "fieldworkers," helps us to frame and delimit our inquiries and our identities. But the fiction of these constructs has become increasingly more apparent, to the point where the edges and borders crumble and we allow our identities and our inquiries to flow between the cracks. While in the field, we are constantly in the process of defining ourselves, of modifying and deepening our identities in relation to others. Life itself is, of course, such a process as well, but when we remove ourselves from a home environment, pay special attention to culture and identity in our research, and grow to become participants in cultural performances, the process of identity making surges to the forefront of awareness.

Following is an edited excerpt from the middle of my ethnography (Kisliuk 1998b), at a critical moment when questions of identity, research method, relationships, and theory all peaked at once. I chose to focus on a BaAka women's music and dance form, called *Elamba*, and took a long journey to the Congo to meet the originator or "mother" of that dance. To best learn and participate, I had been initiated into *Elamba* (this entailed receiving special herbal mixtures rubbed into tiny cuts in the skin at strategic points on the body). On returning to the home region of my research, however, I had some trouble finding how to proceed with my chosen focus. This excerpt takes place at a dance event hosted in a camp several kilometers away from the camp where I was living with an extended BaAka family (Bagandou region, Central African Republic):[5]

> As the Mabo *dancing continued, Djolo and Sandimba arrived at the host camp. During a long break between rounds of dancing, the two of them appeared to be negotiating with the hosts about something. Soon I realized that they were trying to stir up enthusiasm for* Elamba. *This effort, I feared, was expressly for my sake. I suspected that they had interpreted my numerous questions about* Elamba, *coupled with my pilgrimage to Mopoutou [in the Congo], as a request to organize that dance especially for me. But now I just wanted to settle in slowly and get to know the people and the dances better. I found, however, that what I actually wanted was rarely of much consequence. In Mopoutou my wishes seemed relatively compatible with those of my potential teachers, but here in Bagandou signals ended up crossed more often than not, and events simply took their own course.*
>
> *After some resistance from the hosts, it looked as though Sandimba and Djolo—with the assistance of Elanga and Bondo—had succeeded in mustering some cooperation for* Elamba. *I wanted to let them know that I did not want them pushing* Elamba *for my sake, but I appreciated their intentions and was hesitant to*

Fig. 12.1. The author in home camp near Bagandou in 1989. Her neighbor, Koma, sits at center with her twin babies. The other women and children are relatives visiting from a nearby camp. Photograph by Roy Kisliuk.

discourage or confuse them. I decided to relax and see what would happen, so I moved to sit on the ground with the women who had gathered to sing. Bondo sat at my side and asked whether I would dance. I said that I probably would not because I first wanted to watch a few more times. She seemed disappointed and I wondered if I had made the right choice. Maybe they had already promised the others that I would dance—since, after all, I had been to Mopoutou to be initiated—and my declining would make them look foolish. But I wanted to dance well on my first try because I believed that how well I danced would establish my reputation as a serious learner. I had not yet seen enough to dance well.

It took a while for the singing and drumming to warm up. "Mama Angeli" was the opening song. I listened to the drum rhythms, trying to memorize the pattern, which I sometimes found elusive.[6] As I sat there singing along, a young woman who happened to be sitting beside me suddenly pointed to her leg and told me to tend to her small, dirty cut. I was taken aback. I gave her an exasperated look and told her that we were in the middle of a dance and, besides, I was not a doctor (I did often spend hours, mornings, trying to meet the constant demand for first aid). She looked

back at me defiantly and I kept singing. Sitting on my other side, Bondo had not noticed the interaction and smiled approvingly at the part I was singing.

My frustration and confusion as to how the BaAka viewed me and how I should view myself had peaked. In retrospect, the young woman with the cut may simply have approached me in the only way she knew how. But I was frustrated by being pulled into an encounter more consistent with the earliest stages of fieldwork, and especially during a rare Elamba *dance, while I was trying to communicate with Sandimba and the others. I had a sudden sense of panic that interactions like these would repeatedly interrupt my access to performance as it unfolded. I felt squeezed within a paradox. My experience as a researcher of music and dance helped me feel close to BaAka as performers, while strangers like the woman beside me resisted my efforts to move beyond being stereotyped. She approached me as though I were a nurse-on-demand, undermining, I felt, my developing role as an apprentice by insisting instead that I conform to her image of white people with medicine. Even my friends from Ndanga, who had tried to understand what I wanted (that is, "to participate in* Elamba"), *and had attempted to arrange it for me, did not realize that I—unlike most other non-pygmies they knew—hoped to pursue my interest not by grabbing at it greedily, but through patient interaction with them. Though I had developed what felt like effective communication with people like Sandimba and Elanga, it was becoming clear to me that even they had yet to understand what I hoped to do as well as had Bongoï and Kuombo in Mopoutou, and perhaps they never would.*

But what really baffled me was the challenge of merging two roles: the silent new apprentice and the interacting partner in a cross-cultural dialogue. To learn a new expressive form, I first had to watch and listen. I wanted to absorb the repertoire as a quiet apprentice, but at the same time I puzzled with how this stance could fit with the interactive model of ethnographic enquiry within which I had also framed my project. My aim as an apprentice was to experience BaAka performance culture without radically transforming "it." I did not want to block my own access to learning about music and dance because other people's preconceptions about me were making my presence disruptive. At the same time I knew that I needed to understand those disruptions as part of a palpable context I had helped to create, set within historical circumstances beyond my control. (Kisliuk 1998b: 84–85)

What does it mean to define oneself as a field researcher, ethnographer, or apprentice? The dialectic of defining oneself or being defined by others is the cornerstone of social and cultural politics (see Williams 1980). In any role or profession, in order to act upon the world we need to continually re-express our identities; we get to know other people by making *ourselves* known to *them*, and through them to know ourselves again, in a continuous cycle. In field research this task is broken down to its basics, and magnified, and the micro and macro politics of social life are revealed. When I first began interacting with BaAka, I named myself (rejecting the name "white person," which some assigned to me) and re-defined myself, so as to try and break from the legacy left by other people with white skin (colonialists, missionaries, and anthropologists) and local visitors

(villagers and people from the capital city). It was a long road getting to a place where I could define myself as an ethnographer and student of music and dance, let alone assert my particular personality. The basics of language acquisition and just the time it takes to get to know people made for long-term and formidable obstacles to identity building, and there were continual setbacks.

Having a close friend and research assistant to help me define myself was crucial. A man named Justin Mongosso, from the village of Bagandou, had worked with BaAka and researchers in the past and was particularly interested in my project. He taught me the BaAka language, guided me through the forest, and often mediated between me and people's assumptions about me. But this relationship, which was to transform later (as I will address) also had to stand continual tests. Establishing the economic and ethical base of our partnership was the first, early challenge:

> *After the day's heat had dissipated, I took a walk down the road with Justin. We discussed how to arrange the logistics and finances of our working together. The forest loomed on each side of the mud-tracked road as we walked, and black-and-white toucans crossed above, cawing. . . . The way to oppose the lingering effects of the colonial past, it seemed to me, was to take hold of the historically defined relationships imposed upon myself, Justin, and the BaAka with whom we would work, and knowingly struggle against that history, reshaping our relationships to fit our respective values and actual situation. Justin and I decided that the money for my project would be available for our collective necessities instead of me paying him a "salary." For Justin, this arrangement had several advantages. It liberated him from a social obligation to give his money to undeserving but insistent relatives who would otherwise assume, because he was working with me, that he always had extra cash. This way we could instead apply the funds to our projects (my learning, his farming) as required, while keeping on hand emergency resources—first aid supplies and petty cash—for family, friends, and neighbors in need.*
>
> *Through this arrangement I was spared the untenable role of being my host's employer and was better situated to construct my own identity and relationships free from the weightiest colonial baggage. It might have been simpler (and in fact cheaper) just to establish a fixed salary, the way other researchers and business people usually do. Our way, by contrast, would require a constant effort to renegotiate financial matters according to changing mutual obligations, fluctuating priorities, and emerging circumstances. But, I felt, such negotiation would arise in response to those very real circumstances, and would therefore suit our living relationship.*
>
> *Only a few days after our talk along the road, however, a gap between theory and practice was already emerging. As I watched the last of my recently purchased wheat flour being baked into pan-bread for Justin's children, I was wondering why Justin and his family could not seem to keep provisions around for any length of time. Why did they need to use the flour I had bought all at once? I ended up sharing the flour and other provisions with everyone in Justin's family compound, not to*

mention visiting passersby. And I noticed them giving away my emptied "ziploc" baggies. I would have liked to reuse them. "I know sharing is the thing here," I wrote in my journal. After all, they were sharing most of what they had with me (and what I had with others). "But how can I keep my head above water this way?" I wondered. I could not spend all of my time and energy worrying about provisions. "And I hope the money will hold out," I wrote. But my concerns were as much about the social interpretation of property as they were about money—about culturally defined boundaries of private property and its connection with definitions of "Self" or community. How I would construct my "Self" here depended on being flexible and examining those boundaries, first with Justin and his family, and then with BaAka.
(Kisliuk 1998b: 21)

Gradually, over two years and more, shared experiences and defining moments helped me to situate myself. The actual writing of the ethnography was also a process of identity formation, one in which I could sift my experiences and frame them ethnographically. I returned to my research area after having written the ethnography, with a strong but ever-evolving sense of my place in that particular social landscape.

But in fact the borders of my research area—the field—were not fixed but mutable. During visits to the capital city, Bangui, for example, I learned about the relationship of villagers to pygmies in the national context of the Central African Republic: I watched the children of Bagandou farmers—now at high school in the city—produce amateur comedies about the pygmies back home. And when Justin visited the United States in 1993, I saw in his reactions yet another set of reflections on his home world and mine. So, although we may imagine a "center" to our research area, the field is a broad conceptual zone united by a chain of inquiry.

Time itself plays a role in shaping the field and the fieldworker. The relationship we have to past research experiences tends to change, and the changing (hopefully maturing) theoretical and intellectual environment of the mind affects how we take in and interpret new field experiences. The following extended example illustrates this process: During my initial project—which consisted of two years of research between 1987 and 1989—I became familiar with and participated in the current repertoire of hunting dances and women's dances in the area where I lived—the Bagandou region of the southwestern Central African Republic (Kisliuk 1991). I spent most of my time with one particular extended family, but I also traveled to gain a sense for the flow and exchange of new dance forms and songs coming in and out of the area. In 1989, during the later part of those two years of research, I encountered the effects of recent missionizing efforts by evangelists from the Grace Brethren Church. An American woman, named Barbara but called Bala-bala by the BaAka, focused her "church-planting" work on a permanent BaAka settlement, called Dzanga, west of the area where I had spent most of my research time. I briefly visited Dzanga to get a sense for the choices BaAka in different areas were making in response to this new missionizing activity.
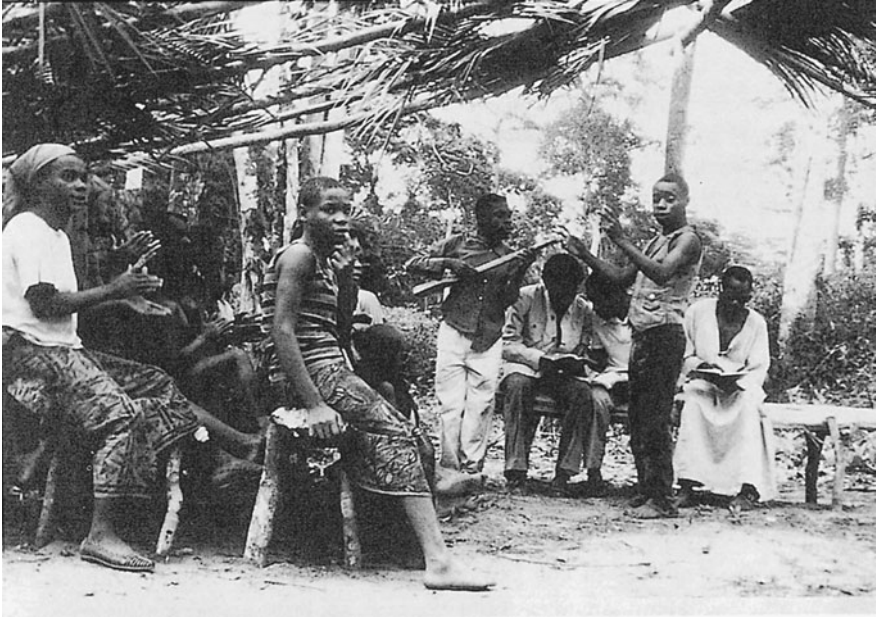
Fig. 12.2. BaAka at Dzanga during a church service in 1989. The man at far right wears a Muslim gown and sunglasses. The man standing at center tries to accompany the singing with a homemade guitar. Photography by Michelle Kisliuk.

I tried to keep an open mind, but when I got to Dzanga I could not help but be shocked and saddened by what I saw. The BaAka there had completely stopped performing the current repertoire of music and dance forms—with which I had become very familiar over the two years. Whereas in neighboring areas BaAka were hotly debating the value of what the "Christians" were saying,[7] at Dzanga all of the BaAka had been convinced by Bala-bala and her Central African evangelists that their own music, dance, and traditional medicine were "satanic." BaAka at Dzanga told me proudly—assuming that I would approve since I am white like Bala-bala— that they now performed only one kind of *eboka* (the word meaning singing, dancing, and drumming—*beboka* plural). Now they would only sing hymns to the Christian god in "church." These hymns were not in their own language, Diaka, but in Sango, the national language and the language of missionaries, which many BaAka do not understand (especially the women). What I saw and heard then looked to me like a slavish imitation of the missionaries—like a kind of cultural genocide—even though I tried to focus on improvisational aspects such as one BaAka churchgoer wearing a Muslim bubu gown, another sporting huge sunglasses and holding his Bible upside down, and the "preacher," trained by Bala-bala, reading haltingly and uncomprehendingly in Sango.

When I next returned to Central Africa in 1992, I saw a somewhat different situation. Three years earlier, the BaAka from my home camp had been arguing the validity of the Christian material, whereas now the controversy had settled. My old friend Djolo explained that the "god dance" is just one among many *beboka*, that they could dance their own dances and still "pray to god." They had begun to place the "god dance" within a BaAka system of values.

At the permanent camp at Dzanga, although BaAka were still rejecting BaAka song and dance forms, they too had begun to recontextualize radically the Grace Brethren Church material. At a "god dance" one evening I saw the dancers, mostly children and teenagers, move in a circle, using steps and drum rhythms just like the recreational dances popular among non-pygmy teenagers in neighboring villages. Many adults stood by, some joining in the dancing, others watching enthusiastically and singing along. The Grace Brethren songs were preceded and followed by hymns from various Christian sects practiced by neighboring Bagandou farmers, including Baptist, Apostolic, and even Catholic hymns. They not only blended all that into the same dance, but also mixed in recreational song styles and rhythms from the neighboring Bolamba people, and even pop song snippets in Lingala (from radio tunes from Zaire and the Congo). They called the entire mixture the *nzapa* (or "god") dance (*nzapa* meaning the Christian god in Sango). Ironically, Barbara and the Grace Brethren do not allow dancing in their religious practice, but they do introduce hymns; and since BaAka do not draw a line between music and dance, in Bala-bala's absence the hymns provided the basis for a new dance form.[8]

As I listened to the performance at Dzanga, I saw this developing expressive form as a means of addressing *modernity*. In an effort to reinvent themselves as competent in a changing world, these BaAka were claiming any "Otherness" that surrounds them and usually excludes them, and mixing it into a form they could define and control. While I found this change hopeful, I was still uneasy that at Dzanga BaAka continued to trade in distinctively BaAka expressive forms whole-sale for an idea of the "modern."

While making my way deeper into the forest beyond Dzanga (along with my research partner and friend Justin from Bagandou village), I met BaAka who, never having seen Bala-bala but only having heard of her, assumed I was she and clapped their hands over their mouths in wonder as though encountering a living legend.[9] I told them I was not Bala-bala, whom I heard them refer to for the first time as a *ginda*, the BaAka term for master teacher of an esoteric dance form! These people didn't even know the real Bala-bala, and although the disturbing idea (to me) that BaAka things are satanic had made it as far as this forest hunting camp, something else seemed to be going on if enthusiasm for the "god dance" was catching on at this distance—budding into a BaAka fad. At present, the majority of BaAka I know are including the "god dance" within a wider, dynamic BaAka repertoire, where it is poised uneasily among several expressive forms vying to define an emergent identity.

Over time, the "field" itself—the ongoing cultural landscape—changed, as did my ideas about how missionary culture was affecting BaAka. Also significant and more difficult to realize, my effort to understand was inextricably linked with my struggle to distinguish my own identity from that of the missionary. As unsettling as it was, at first I could not articulate that distinction, even to myself. To local people I appeared similar to Bala-bala, even if at Dzanga they did observe that unlike the white evangelist, I helped cook my own meals, and unlike the missionaries, Justin and I thought to bring them emergency medicine. Of course, my involvement with BaAka *beboka* also distinguished me from Bala-bala, but sometimes only perceptive people or those who knew me well understood that difference. Over time, however, my experience broadened and deepened enough for me to establish my position, and by extension to better comprehend the developing ethnographic situation.

One obvious difference between ethnographers and missionaries is a difference in ideological and vocational ancestors, though one can argue similarities as well, especially when considering the colonial history of Europe and Africa. As ethnomusicologists, our ancestors and our roles both diverge from and unite with those of anthropologists, missionaries, tourists, and journalists, among others. But in each comparison there is a crucial difference, I've found, and that difference is rarely generalizable, but changes depending on particular circumstances and particular people. In one circumstance, excerpted earlier, I needed to show I was not a nurse, in others not a missionary, and in January 1992 I found that even a "performance artist" had a radically different agenda from my own. I was asked to help an African-American artist research a performance piece about "pygmies," and at first I thought we might share some interests: art, performance, cultural politics, and the richness of BaAka expressive culture. As it turned out, however, this person seemed not at all interested in BaAka themselves, but was interested in how they might serve her performance piece and her own romanticized version of what "pygmies" might symbolize. One anecdote sums it up: One day during her two-week stay in a BaAka settlement, the actress wandered off alone to find the "real pygmies," as she said. She returned a few hours later with a young BaAka man following her. She sensed that he had something very important and spiritually significant to tell her, but she could not understand him and needed help. When the young man spoke his mind, it turned out he just wanted her to give him a cigarette. Disappointed, she found him a cigarette and he left.[10] She spent the rest of her time snapping photos and trying to buy BaAka household objects, seeming more like a tourist than a researcher.

The location of the field, then, does not depend on geography, but on the self-constructed identity of the ethnographer in a given social landscape. Similarly, the emergent identity of a fieldworker depends not on a particular location or apparent resemblance to other investigators and interlopers, but on the quality and

depth of research relationships and ultimately on the way we each intend to represent our experiences.

Ethnographic Writing: Framing and Translating Performative Experience

The task of bringing experience to paper is in some ways like telling a story to friends, only more difficult—especially if one is attempting to interweave theoretical and aesthetic themes and analyses within an extensive, intricately crafted ethnography. The amount of space required to evoke experience exceeds that of other ethnographic modes such as the presentation of predigested theoretical observations or the "writing-up" of quantifiable "data." An ethnography of performance is in itself a meta-performance, requiring all the care, self-confrontational honesty, and detail that the subject matter—people and their expressive lives—demands. A focus on experience also helps to ensure that we as ethnographers explain both the entryways and the barriers to knowing. Being explicit about what one could not come to know, and why, can often be more useful than ostensibly unsullied cultural information.

Rather than seeing experience as two sided (either "my" story or "theirs"), it is more helpful to see the ethnography of experience as a conversation within which learning is located, both during research and while writing (where the metaphorical conversation is with the material and the reader—I take up this point again in the section following). The pretense of much anthropology—and some ethnomusicology in its footsteps—is that it claims to interpret reality *for* its "informants." Ethnomusicologists and other ethnographers have since learned to be suspicious of writing that adopts a self-appointed but unexamined ethnographic authority (Clifford 1988). I can only presume to speak from my own experience, hoping that I have been a rigorous and sensitive enough researcher to have gained insight into a mutual dialogue. If I provide enough relevant information about my experience within the ethnography, the reader can decide whether to trust my insight and how best to use it.[11] Because of our participation in performance, ethnomusicologists are especially aware that there is much one can only know by doing. If, as noted at the beginning of the chapter, we have come to partially "share the same narratives"—and songs—with those whose expressive lives we hope to understand, then an account of our experience—including the inevitable partiality and mis-steps—is indeed exactly where we should focus.[12]

Another argument in favor of experience brought to the forefront by the ethnography of performance is that research is to a great extent particularized by time, place, personality, and social circumstance. One of the most common errors in conventional ethnography is the tendency to generalize into theory based on experiences particular to a certain interpretive situation. The focus on experience

helps us to situate readers within the fluctuations and particularities of performative circumstances. This leads us to the task of writing about performance in a way that evokes this immediacy and particularity; that means finding ways to capture what we've learned via our senses, our bodies. We must make our writing specific enough to convey in detail the social and technical aesthetics of a group or style, and perhaps most important, to evoke the (interpreted) meaning of a performed moment. The following edited excerpt from my ethnography is one attempt to convey performative experience ethnographically:

> In Elanga's camp [my home camp], on January 5, 1989, I joined the dancing of
> Mabo for the first time. That evening I had finally decided I had been waiting in
> vain to be invited to join in the dancing. Elsewhere in Africa people had always
> called me in to dance, even when they knew I was not familiar with the steps. So I
> had been hesitant to impose myself on BaAka—who as yet had not asked me to
> join—without being reasonably sure that they really wanted me to dance with them.
> When I mentioned this problem to Justin's uncle, the Mayor of Bagandou, he
> laughed, saying that the BaAka would probably not ask me to dance with them.[13]
> They would assume that if I wanted to dance I would get up and join them. He
> added that he was sure they would be honored if I did. This shed a new light on
> things. But aside from this, I suspected that my campmates were hesitant for another
> reason. Justin once mentioned to me that some [non-pygmy] villagers think that a
> pygmy can place a curse on them by touching them imperceptibly if they join a
> BaAka dance. As with the Elamba and sorcery issue [addressed earlier in the eth-
> nography], the hesitation could have been that if they invited me to dance and then
> something were to happen to me afterward, they might be blamed. Whatever the
> case, I was restless and felt the time was long overdue for me to start dancing [. . .]
>
> It was a cool night. As Mabo got started I stood near my tent watching, and
> considered whether to put on a single raffia skirt like some BaAka women wear while
> dancing Mabo [the special costume for Elamba, by contrast, requires at least three
> layers of skirts]. I had acquired some skirts in Mopoutou, where raffia is more
> plentiful, and now I pulled one out of my bag. The singing and dancing continued
> but I felt eyes on me, especially because I had, as requested, left a lamp sitting
> between my tent and the dancing circle. I tied the skirt over my jeans and moved a
> little self-consciously to join the dancing, stepping into the circle among Ndoko and
> Kwanga, women of about my age, who did not react visibly to my joining them.
> After the first short round three men, Djubale, Ndanga, and Duambongo, sur-
> rounded me, smiling broadly. They shook my hand vigorously and thanked me,
> "merci, merci. . . ." Then, to my bewilderment, Duambongo suggested that maybe I'd
> had enough. He might have been worried that something could happen to me
> for which they would be blamed. I did not think of this possibility at the time,
> and wondered instead whether despite his apparent enthusiasm, he just wanted me
> to stop.
>
> But as the dancing started up again Ndoko immediately called me to join. She
> addressed me as "beka," a friendly term that BaAka usually reserve for each other.[14]
> Ndoko led me through some Mabo variations, along with pregnant Kwanga, and

young Mbouya and Ndami in front of them. Mbouya introduced a variation, keeping up the heel-sole stepping to the dance beat emphasized by every third stroke of the drum. She crossed her wrists and swivelled them to the beat, crouching down progressively lower as she continued stepping, then gradually straightening upright again. Those of us behind her copied the movement in follow-the-leader-style, breaking into separate upright dancing when we tired of the variation.

Ndoko glanced at me, called my attention again by saying beka, *and suggested that I move my neck more, loosen it up. She demonstrated, letting her own supple neck follow through as she stepped. This was the most specific dance instruction I ever got. After a while the distracting novelty of my dancing seemed to wear off. The focus shifted from me to the whole group, or maybe I just relaxed to the point where I could notice the whole group.* Oka, oka! *people called out, meaning "let's go!" [literally, "listen!"].*

My senses tingled; I was finally inside the singing and dancing circle. The song was "Makala," and singing it came more easily to me while I danced. As I moved around the circle, the voices of different people stood out at moments, affecting my own singing and my choices of variations. Ndami sang a yodeled elaboration I had not heard before. I could feel fully the intermeshing of sound and motion, and move with it as it transformed, folding in upon itself. This was different from listening or singing on the sidelines because, while moving with the circle, I became an active part of the aural kaleidoscope. I was part of the changing design inside the scope, instead of looking at it and projecting in.

The physical task of executing the dance step melded with the looking, listening, smiling, reacting, that kept us all dancing. Since our camp was built on a hill, it took extra effort to dance the full-soled steps while going up or down hill. Running the bottom of my foot inchworm-like across the ground required the sturdy support of all the muscles in my leg. All this while trying to stay loose enough to follow through with my whole body—including my neck—and keep up with the beat. As I continued to dance, trying to refine my step, I noticed more fully the inward and delicately grounded concentration of the movements, like the mboloko [blue duiker] antelope. Someone cried out, sukele! *["sweet!" an interpretation of the French, sucré].*[15]

Suddenly, a few people shouted rhythmic exclamations that suggested a shift to the esime *[the percussive break section], and the singing stopped. Tina stepped into the center of the circle and walked in the opposite direction to the one in which we were dancing. He shouted* Pipi! *[imitating a car-horn], and the group answered* Hoya! *[an exclamation]. He continued,* O lembi ti? *["Are we tired?" in the Minjoukou language], and we answered* O lembi (o)te! *["We aren't tired!"]. As the* esime *continued people "got down" in their dancing, crying* heeya, heeya, *repeatedly on the dance beat, and sometimes jumping forward with a scoot instead of stepping to the beat. At one point the women grabbed the shoulders of those in front of them in line and began chugging ahead on the beat. I joined in, finding it hard to jump up the hill while trying to stay as close as possible to Ndoko, whose shoulders I held onto in front of me. Someone was behind me, I don't recall who, but she had to grab my waist because she could not reach my shoulders comfortably. It was unavoidably*
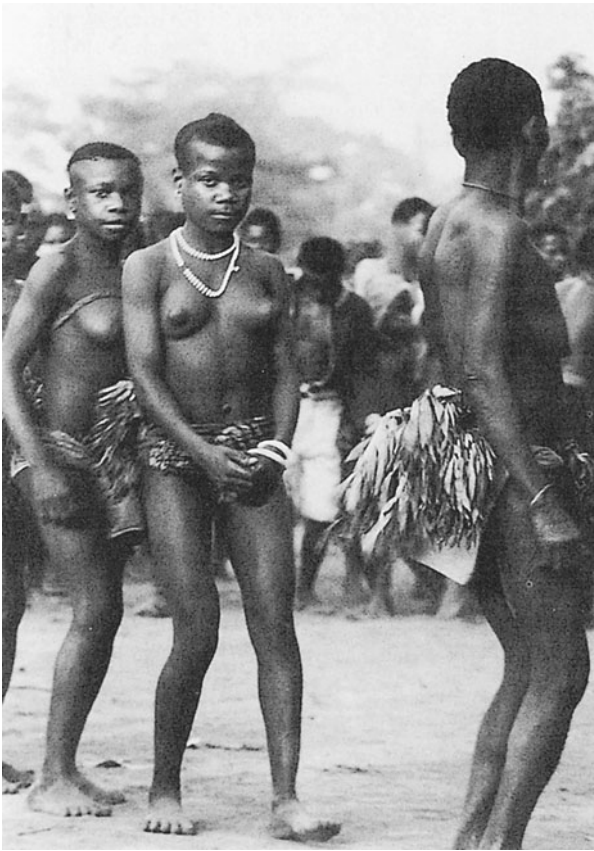
Fig. 12.3. BaAka women dance Mabo *in 1988 in a camp near Bagandou, not far from the author's home camp. Festive* mandudu *leaves that bob while dancing are tucked into the woman's G-string. The men's part of the circle can be seen in the background. Photograph by Michelle Kisliuk.*

clear at this moment that I was much bigger than everybody else [Ndoko, one of the taller women, comes up to my chin], and I didn't exactly blend in.

I sat beside Kwanga and other women who were taking a break from dancing to sing from the sidelines. I noticed that some singers repeated only one or two variations of a melody during a given song, or dropped out for a while and then rejoined the chorus later. Other singers skipped around between several elaborate variations and then joined friends in emphasizing and repeating one particular melody fragment. As a song continued, the entire group sometimes focused on only a few overlapping variations at a time, leaving out the initial melody entirely. This was sometimes confusing to me because I could not always recognize the variations as having been inspired by an underlying but now silent theme, and could no longer recognize the song.

During this eboka I realized that at least some individuals have signature song and movement styles, phrases or tendencies in movement or melody that suit them, and to which they return periodically. I first noticed this as I sat beside Kwanga while both of us were taking this break from dancing. Though I had noticed her singing a number of times before, this time I was fully conscious of her specially "bluesy" style.

When the evening of dancing was breaking up, several friends crowded excitedly around me. Djubale told the women to show me all the bisengo *[pleasures, i.e., of the dancing] because this is my* ecolie *[school, from French]. As I fell asleep I noted that it had been one year since that first dance at Ndanga when I had pictured what I might learn, and had determined to do so.*

The next morning people were very quiet. When I crawled out of my tent I felt eyes watching me from inside the huts. Duambongo came over and tentatively said bala èe *["hi"]. When I responded in kind as usual, he reported aloud that I was* bodi bona *["still like that," still myself]. Sandimba also greeted me with a relieved smile. Considering all the rumors, they must have wondered about the effects of my dancing. They were clearly glad to see that I was safe and sound, and that no blame for any harm would fall on them.* (Kisliuk 1998b: 99–103)

How should we proceed when we have experiences or flashes of insight that are essential to understanding, but which do not lend themselves to prose description? Occasionally metaphor can bridge the gap, for example at moments in Feld's ethnography, *Sound and Sentiment* (1990:216). The use of metaphor raises the question of whether we can presume to translate experience from one domain into another, possibly foreign one. But ethnography itself is such a translation—we're already in that game in other words. By moving directly into the realm of metaphor we boost the risk of missing the mark ethnographically or obscuring rather than clarifying experience. But if we proceed with caution (and practice) we can use poetics—steeped in experience—to convey in writing what otherwise might never come across.

Certain junctures in our writing can call for full poems rather than brief metaphors. Anthropology has a relatively long, if marginalized, history of poetics (see, for example, Brady 1991), but such efforts have been rare in ethnomusicological writing (with the more recent exception of authors such as Hagedorn [2001]). This seems surprising because one would think that music lends itself, even demands on occasion, embodied poetic description (e.g., Cantwell 1984). The avoidance of poetics could have been part of the effort by ethnomusicologists to legitimize our young field in the eyes of those who tended to see music as frill rather than as core culture, and a reaction against unsubstantive but flowery music writers, travelers, or dilettantes. Now, however, especially since an academic green light of legitimacy has come with the acceptance of interpretive and literary anthropology (e.g., Geertz 1973 and later Narayan et al. 1993, among others), we can begin to tackle the ineffable but crucial aspects of experience that can only be addressed poetically. Following is one attempt I made, early in my research time, to try to crystallize my field experience up to that point:

"To Ndanga and Back"

A stream to wash in.
On my way I displace three blue

Birds of paradise.
Through soapy hair

A monkey eyes me from above.

BaAka children run singing down the path
To the stream,
Leaving tiny raffia skirts
Perched on bushes.

At midnight I wake to a mother's
Heart crying mourning songs.
Later, sprawled on her daughter's grave:
"Ame na wa na mawa, mawa na mwana wa mou."
"I die of pitypain, pitypain for child mine."

Milk still drips.

The moon lights a dance for the baby's
Returning spirit.
Women move together,
Singing the collective mother's pain:
"Mawa na mwe,"
"Pitypain mine."

On the return trail we eat
Antelope dinners,
Pass villager hunters who
Hold a baby chimp
Captive—
Pieces of its mother packed in a
Basket of smoked
Meat.

The last day of walking,
Too tired to reach the village,
We camp near a stream.
Dangerous spirits
Move by in the night.

By morning we remain, the
Big green of the forest
All around. (Kisliuk 1998b: 44–45)

One challenge that often comes with the description of aesthetic phenomena is to walk the thin line between romanticization on the one hand and irony on the other. This issue is particularly present regarding descriptions of African pygmies, because writing in this area has been heavily romanticized. Following is an attempt I made to achieve a balance between romance and irony in an early description:

Periodically the forest path passed through BaAka camps and settlements. This being my first time in the deep forest, I was enchanted when I heard a falsetto BaAka melody, diyenge, *ring out through the trees as we approached one camp. A few steps later I saw the man, singing from high in a tree where he was cutting palm nuts. This is it, I thought, this is that romantic "pygmy-singing-in-the-forest" image I had come to expect from reading Turnbull, Lomax, and Arom. The clearing was actually fairly barren and dusty, but the path led to a shady stream that ran through the center of the settlement. As we approached to cross the stream, a teenage girl who had not seen us coming was singing a brief, open-throated song that echoed on the water and into the trees.* (Kisliuk 1998b: 37)

When writing about field experience we want to get as close to a truth as possible, but evocation means selecting among experiences and choosing among a variety of ways to convey them. When we move beyond an objectivistic style of writing, boundaries between fiction and nonfiction can become blurred. This blurring does not mean that we are now writing "fiction," it means that the *construct* of "nonfiction" has begun to crumble along with the objectivist model.[16] The more explicit we are in our efforts to evoke experience, the closer we can come to communicating that experience and what it might mean.

Ethnography: What's In and What's Out?

Critics of reflexive ethnography often point to the sin of self-indulgence as the fatal flaw of such efforts. These critiques have often been justified, since early attempts at reflexive writing often did not distinguish between a "confessional" mode and an experiential ethnographic mode.[17] The fear of self-indulgence and the label of unprofessionalism created an implicit taboo against writing that seemed too personal, but in the 1980s there was a turning point for some anthropologists. In his essay, "Grief and the Headhunter's Rage" (in 1993[1989]:1–21), Renato Rosaldo struggles with his realization that he only came to understand what Ilongot headhunting meant in the Philippines when Michelle Rosaldo, his wife and re-search partner, tragically fell from a cliff to her death. At that moment he un-derstood for the first time the grief and rage underlying the Ilongot practice. Even then, Rosaldo worried that he was being self-indulgent by invoking in his writing this realization and his personal loss. Most anthropologists and other ethnogra-phers have not been trained to distinguish between self-indulgence and ethno-graphically relevant experience, and have thereby impaired themselves and their readers. The way to distinguish, I suggest, is to ask ourselves whether an experience changed us in a way that significantly affected how we viewed, reacted to, or interpreted the ethnographic material (and to write with those connections in mind). For example, my choice to include my grappling with the issue of the presence of missionaries among BaAka was linked to a sense that my own struggle

paralleled how BaAka themselves were confronting the politics of expressive culture, power, and identity. My own confrontation with the situation significantly affected my interpretation and my choices while in the field.

Sometimes, however, we can sense that certain experiences are relevant to an ethnography, even though this relevance is not obvious at first. Field experiences can be like dreams or poems—overdetermined in pertinence to issues and ideas, but existing within a realm of intuition. In ethnomusicology we also may wonder whether our experience is pertinent to an understanding of "music." Musical expression is usually so interlinked with the very life that music and other expressive forms embody, that the intuiton of the ethnographer who lived a particular field experience is sometimes the only determining factor. The following passage and poem provide an example of an experience that I felt was relevant to my ethnography but which took place in a physical realm unconnected to BaAka. I had been on a journey to a different region of the forest to meet BaAka who live near the Cameroon border, and was on my way back:

> *I returned toward Bagandou from the west, obliged by the limited road system to travel far out of my way to the north. Riding in the crowded vehicle through the barren north country, I began to miss the trees. I sat in the cab next to the Chadian driver, the singing of the BaAka still filling my mind. We had left the town of Carno early in the morning in a blinding rain storm, and now we were whizzing down the road. I was apprehensive because I had just heard about a head-on collision between two* trafiques *[passenger vehicles] in this area. There had been no medical aid available and many people had bled to death. I breathed a bit easier, therefore, when the driver slowed as we approached a small village lining the road. The village looked eerily empty. We slowed more when we came upon a huge truck stopped by the side of the road. From behind the truck sprang nine armed bandits—their faces disguised by charcoal—who halted us. They pulled the driver out of the cab and then began shooting at us. . . . When I returned to Bagandou I told some BaAka about this ordeal, and they covered their mouths in horror.*

> *"Dream or Not"*
>
> *A dream:*
>
> *A smooth antelope, immobilized*
> *Surrounded by hunters.*
> *An antelope woman*
> *Brown, gentle, strong,*
> *Wearing fresh green leaves*
> *Bent forward, hands behind her,*
> *Moments pulsing into a final sinking capture.*
>
> *Waking:*
>
> *Cringing in a bus*
> *Seized by thieves.*

Motor running, knee on the gas.
Two sickening pops,
A quick breath,
Glass, blood, bullets
Aimed at someone
Black, gentle, strong,
Wearing leafy green fatigues,
Shot into
Unimagined death.

Me—but not the leafy ones—waking
briefly back to life. (Kisliuk 1998b: 171–172)

Why include this in my ethnography? My experience on the bus influenced profoundly how I remember my research time, especially at that juncture. Moreover, the metaphor in the dream connected the bus ambush to my experience among BaAka and their cultural struggles, and on yet another level with the hunting that I witnessed (I had tried to suppress my reactions to the undeniable brutality of slaughter). All these factors together determined my decision to include that passage. We continually move back and forth from experience to a perspective on cultural processes, and back again, until the intellectual and experiential come together. Trusting our intuition to tell us when occasionally to describe experiences that are not obviously relevant can help us later when we discover why they were relevant indeed.

Since the publication of the first edition of this essay, I have encountered critics who have asked why it is that I did not describe in detail in my book my relationship with Justin, which eventually became intimate (we married in 1998). Kofi Agawu in particular (2003) has challenged this aspect of my research and writing. The topic speaks directly to the question of "what's in and what's out" that this essay poses, so the second edition of this collection offers an opportunity to respond and expand briefly on the issue. When I prepared to write the dissertation version of the ethnography (1991), I spent a great deal of energy asking myself if or how my relationship with Justin should figure into the writing. I eventually decided that the intimacy of the relationship should figure inasmuch as it directly affected the topic at hand, BaAka musical life, but, given the attraction and distraction to readers of including a "love" narrative written by a female ethnographer (I allude to this issue in the text itself, 1998b:106), I decided that such detail would likely weaken the focus of the central narrative. In addition, at the time of publication of the book version of the dissertation (1998b), the future of our relationship was still uncertain; I was intent on protecting Justin's privacy as well as my own (at the stage of final page proofs—when I was still allowed to add a small footnote, it was clear that we would finally marry and I did make note of this—which Agawu noticed). Broader issues in ethnographic research and writing, in this

case feminist and ethical issues, are foregrounded by questioning "what's in and what's out," and the answers often fall within gray areas of judgment and aesthetics at the time of writing.[18]

Conclusion

In summary, a focus on field experience is clearly essential to performance ethnography. The challenge to ethnomusicologists is to create ethnographies of musical performance that are fully experiential. To that end I have developed a checklist that can encourage interactive, performative writing: There are at least three levels of *conversation* (literal or metaphorical) in the ethnographic process, and they each need to be addressed. The first is an ongoing conversation between the field researcher and the people among whom she works. This does not imply, as some critics have contended, that power relations are somehow level in such conversations. To the contrary, power relations are continually shifting, multi-leveled, and resonant with history and circumstance. A focus on conversation (or dialogue)—during which power relations are in fact negotiated—obliges the researcher/writer to address and examine those relations. (I discuss this paradigm in more detail in Kisliuk 1998b.) The second level is the researcher's "conversation" with the material of performance such as song, dance, storytelling, and ideas about politics, social life, and aesthetics. The third—the ethnography—is a re-presentation and evocation of the first two conversations, within an overall meta-conversation among the ethnographer, her readers, and the material and ideas she addresses. As noted earlier, there is no definable border between the field and the space of writing—we write when we are doing research, and we research while we write. An awareness, therefore, that field experience and ethnography are inseparable must infuse both.[19]

A final excerpt from the end of my ethnography might synthesize the three interrelated issues addressed in this essay: the identity of a field researcher, ways of writing about field experience, and the problem of sifting and determining the relevance of those experiences:

> *I had thought, at first, that I could exist as a lone researcher/apprentice, outside the legacy of colonialists, missionaries, and anthropologists. But I found instead that I had constantly to confront my predecessors, even at the end of two years of research. I spent my final week [of that research period], in June 1989, with the BaAka of Kenga—the first BaAka I had ever met. They were living in a hunting camp several hours into the forest from Kenga village. One afternoon the women in the small camp were sitting by a fire weaving baskets. Mumbling something among themselves, they turned toward me. Makanda asked me pointedly whether, where I come from, we have animals with bones, and, if so, whether we eat the animals and throw away the bones. Confused, I answered yes to both questions. They gasped in surprise. Soon I comprehended the reason for the question: An American archeologist had collected*

Fig. 12.4. Makanda, wear-
ing globe earrings, prepares
palm-nut oil in a forest
camp near Kengan in 1989.
Baby Molube steadies
himself behind her. Photo-
graph by Michelle Kisliuk.

animal bones in their abandoned camps a few years earlier and had left them
perplexed as to what she was doing and why—were bones worth something where
she came from? This conversation led us to the question of where "white people"
come from—they thought that we all live in Bangui. When I explained that the place
I come from is so far away that even Mongosso [Justin] had not yet visited my home
(they knew he sometimes traveled to the capital with me), they were flabbergasted. I
said that I have to take two airplanes to get to where I live (they sometimes see planes
flying above). I took out a pair of globe earrings that I had brought as a gift, and I
tried to use the little globes to illustrate this new concept. Makanda donned the
earrings.

That night, after we had all gone to sleep, a violent storm began to stir. I lay in
my tent listening to the wind roar in the canopy. Suddenly I heard Mabambo in the
next hut call out my BaAka name, Masoï, oupa, oupa! ("get out!"). Lungoo!
("violent winds that can fell trees"). I tied my sneakers and crawled quickly out of
my tent. Everyone was outside the huts, looking up. Lightening flashed intermit-
tently, revealing the turbulently swaying trees. Handing me a stiff duiker skin to hold
over my head against rain and falling branches, Makanda lamented how frightful it
was that we should have such a storm while I was visiting. Her husband, Mabambo,
stood holding their little baby Molube. Clutching Molube affectionately, Mabambo
looked up at the swaying, roaring trees with a concerned, expert eye, occasionally
telling me to move in one direction or another. Although I was afraid, I felt I could
trust Mabambo to know what best to do. Gradually the winds subsided, the rain
pelted down heavily, and we all got back inside our shelters. Still shivering, I had
gained a visceral understanding of BaAka vulnerability and resilience. For me,
Mabambo's vigilance was a lesson and a metaphor: survival depends on knowing

how, when, and in which direction to dodge in the political and cultural "forest" that sustains us. (Kisliuk 1998b: 174)

By the time I finished writing the ethnography, I learned that my friend Mabambo, young husband and father, had passed away from a sudden illness. The fragility and ephemerality of his life and our own became even more evident. My ethnography came to serve as memorial to those who had passed away since it was written, and the quality of my effort to capture the life they lived and shared with me was all the more important to me. Coming to "share the same narratives" also means that we have come to affect other people's lives, and that we ourselves have been fundamentally affected, often in ways we cannot control. Field experiences become worth writing about and reading as a result of full participation in the life of research. The challenge and opportunity of performance ethnography is to focus thoroughly on that aliveness.

Notes

1.   Ethnographies such as those by Feld (1990), Chernoff (1979), Berliner (1978), and A. Seeger (1987b) began to fill the void. Others have followed, such as by Reed (2003) and Wong (2004).

2.   There is an argument that a focus on "experience" simply relocates constructed authenticity or authority from an idea of an "original," to irrefutable claims about "experience." I suggest, however, that the critical evocation of "experience" can be used to strategically re-naturalize and thereby throw into continuous question constructions of experience and identity, and therefore hopefully to illuminate in detail the politics of cultural production and reproduction.

3.   The term "pygmy" should read here as "so-called pygmy." "Pygmy" is a problematic term often carrying a derogatory or belittling connotation until Colin Turnbull's loving celebration of the Mbuti pygmies of Zaire (1961). Nonetheless, it is the only term in English inclusive of the many socially, culturally, and historically similar peoples of the African equatorial rain forest, including the Efe, Mbuti, Twa, Baka, and BaAka, among others. These current or former seminomadic hunters and foragers name themselves in many different languages, but often use the general expression "forest people" (literally "offspring of the forest") to distinguish themselves from their village-dwelling neighbors. I use "forest people" and a variety of other terms here, but the term "pygmy" also becomes apt when invoking issues and attitudes that engage "pygmies" as a social and cultural category, defined both regionally and globally. "Pygmy" as a racial label is objectionable, however, and therefore I lowercase it.

4.   Journalists often refer to being in the "field" as "on assignment," but do not use the term "fieldwork," which is associated more with the social sciences and some natural sciences (where the "field" is opposed to the "lab").

5.   I have chosen the spelling BaAka instead of Aka—the root word used in much of the scientific literature to refer to these pygmies of the western Congo Basin (e.g., Bahuchet 1985; Hewlett 1991). BaAka themselves never say "Aka" but use a prefix, "Moaka" singular, "BaAka" plural, and I feel most comfortable using terms closest to theirs. BaAka have

varying accents; some call themselves "Biaka" (a spelling I formerly used), others say "Bayaka." BaAka is a spelling that accommodates these accents while indicating the prefix/root structure of the term (the second A after the prefix is capitalized so that readers will rearticulate that A). The BaAka language, classified as a Bantu language, is called Diaka.

6. For transcriptions and analyses of the music I refer to in this chapter, please see Kisliuk (1998b).

7. I had at first resisted showing BaAka in my home camp my bias against the missionaries, but I was eventually obliged to enter the debate myself (see Kisliuk 1998b).

8. I was at first confused about this transition from hymns in "church" to dancing, and asked a BaAka man whether, as some claimed, Bala-bala had taught them this dance. He said, yes, and when I asked incredulously if she actually *dances* he answered in the affirmative, demonstrating by imitating her bouncing body movements as she played the guitar to accompany the hymns.

9. BaAka have coincidentally associated dead ancestors—traditionally white—with people with white skin. Bala-bala's reputation and instruction, therefore, held a super-natural sway which, unknown to her, had nothing to do with the nature of her preaching.

10. The issue of distributing cigarettes to BaAka was already a burning one for me, since I had struggled at first to break with the longtime convention of trading cigarettes for hospitality, knowledge, or meat. I preferred to reciprocate with gifts such as spearheads, axeheads, salt, and first aid (see Kisliuk 1998b), and had asked explicitly that this visitor comply with my program in exchange for my help.

11. I have also found that readers, especially student readers, are much more likely to care about the people and the expressive culture described if the process of learning is an explicit and constant part of the ethnography.

12. Interviews, preferably informal ones, and direct quotation have their place in our research and writing. But, particularly when addressing the first few years of research using a new language, among new people, the focal point of an ethnography must firstly be with the experience of the researcher.

13. Months and years later, when I became generally known as someone who joins BaAka dances, people did venture to suggest I dance.

14. To the west, in Bayanga, the term *beka* is used by villagers to refer to pygmies, but has a derogatory connotation. Not so in Bagandou.

15. A metaphor in wide use in Africa, the concept of "sweet" or sugary is applied to good music and dancing (see, for example, Stone, 1982). While BaAka often shout *sukele!* during Mabo, they do not use the Diaka word for sweet and/or spicy, *bolembeiembe*.

16. Carlos Castaneda's early (fictional) work obliquely addressed this very question.

17. One of several notable exceptions is Colin Turnbull's classic book, *The Forest People* (1961), though the book is problematic for other reasons.

18. Babiracki's essay in this volume addresses related issues.

19. I address this question further in the introduction to Kisliuk, 1998b.

Confronting the Field(note) In and Out of the Field

Music, Voices, Texts, and Experiences in Dialogue

Field research is performed. The performance of field research is one of the most meaningful processes engaged by ethnomusicologists to define themselves. And writing about field research—ethnography, field diaries, fieldnotes—is often part of the process of re-performing field research. In this period of post-postmodernism the social sciences can no longer claim that the fieldworker escapes significant participation in the total cultural performance of field research. Performance is, after all, according to Johannes Fabian, "not what *they* do and *we* observe; we are both engaged in it" (1990:xv, emphasis added). In recent literature on field research and representation, ethnographers assign greater importance to writing *in* the field; experiences are transformed into texts, and fieldworkers, informants, friends, and teachers emerge as actors in a social drama.[1]

Reflections on ethnographic writing often neglect texts produced while still in the field—fieldnotes. Fieldnotes are for many ethnomusicologists an essential aspect of knowing; they are not only critical in determining what we know, but also illustrative of the process of how we come to know what we know. In this chapter I suggest that fieldnotes are part of the process that informs both interpretation and representation, understanding and analysis of experience—*in* and *out* of the field. As John Van Maanen suggests, fieldnotes are " 'of' the field, if not always written 'in' the field" (1988:95). Notes written in the field affect perception, memory, and interpretation and are a part of an individual's way of knowing (what do we know about musical performance?) and process (how do we know it?).

Throughout this chapter I draw on examples from my field research with East African *kwayas* (Kiswahili for "choir") to illustrate ways in which fieldnotes function as more than hastily scribbled lists, scraps of paper, sloppy or unedited observations, or the inscription of performance details.[2] Fieldnotes were an essential part of my emergent epistemology during my field research—fieldnotes inscribe action while simultaneously affecting and reflecting that action. It is a

simple premise, one most ethnographers would not question or deny. Yet, field-notes seldom appear in our ethnographies; only rarely when we write about musical performance do we allow our readers to experience our individual processes of knowing, those paths we took to arrive at understanding, interpretation, or analysis. In this chapter I argue for the inclusion of fieldnotes in our ethnographies and suggest one possible way of introducing fieldnotes into the dialogue between experience and writing.

I present selected fieldnotes in tandem with other voices: First, my voice while still in the field; second, a voice of reflection after the note was written; and third, a voice more distanced from experience. I use an *italic typeface* to represent the initial fieldnote, the first voice (presented in the present tense). The second, reflective voice is often an inscribed form of what Simon Ottenberg refers to as a "headnote," a memory associated with a specific field experience (1990:144). This second voice is represented in capital and small capital letters (presented in the past tense). The third voice, more analytical and removed from the first two field voices, represented by a roman typeface, illustrates my interaction with my fieldnotes "out of the field."

As the following fieldnote illustrates, the process of writing notes in the field presents a significant opportunity to pivot between experience and understanding, explanation and knowing:

> *After returning from a kwaya rehearsal, I sit at my small desk watching the pen in my hand, attempting to will it into motion. It is a warm morning—it has been warm for such a long time. The standing fan wheezes, moving the already thick air from one place to another. The smells of* ugali *and* mchicha *fill our small room quickly as Mona and Amani prepare our afternoon meal.* WHY DID I NEVER LEARN TO COOK UGALI AS MONA HAD? WHY DO WE "CONSTRUCT" OUR EXPERIENCES SO DIFFERENTLY?—WHY DOES SHE ENTER TANZANIAN "FEMALE" CULTURE IN SUCH A DIFFERENT WAY THAN I ENTER "MALE" CULTURE? I CONSTANTLY RETREAT INTO MYSELF, EXPECTING ANSWERS TO COME FROM WITHIN, FROM A CONNECTION BETWEEN MY HAND AND THE PEN. MONA RELIES ON "HEADNOTES" AND REVIEWS THEM LATER WITH ME. I CONTINUE TO KEEP FIELDNOTES AND DIARY ENTRIES—IS THIS THE BEST WAY OF "INTERPRETING" WHAT I EXPERIENCE? I read through my fieldnotes—stored in diaries and journals—and find myself frequently writing through my experiences, using writing as a way of triggering further thought and action as I reflect on notes taken "in the moment" and "out of the moment" [where do the borders begin and end?]. *I concentrate on two conflicting stories from the previous evening's rehearsal, but find it difficult to make sense of the issues, to resolve the disparity. The rain outside the screened windows distracts me for a while—constant and predictable for over a week now, cooling down the steamy afternoons.* I GLANCE DOWN AND MY PAGE IS NO LONGER EMPTY. MY HAND MOVES AS I CONNECT WITH MY EXPERIENCE, AND I FORMULATE A RESPONSE TO THE CONFLICT I ENCOUNTERED LAST NIGHT. I BEGIN TO INTERPRET WHAT MDEGELLA AND OBAMA WERE DEBATING, AND RELAX AS I REREAD MY FIELDNOTE AND UNDERSTAND MY EXPERIENCE.

This experimental form of re-presentation is just one of the many paths I could have chosen. I did not spend all my time writing in the field and do not mean to imply that I solved all the problems of my research agenda through writing. Nor do I suggest that writing notes in the field is the only such way of re-experiencing, processing, and representing experience. My observations of fieldnotes both in and out of the field create an ongoing dialogue for me that illustrates the process in which all three of these voices contributed to my understanding of and personal relationship with change and adaptation in Tanzanian kwayas.[3]

What Is a Fieldnote?

Before exploring the role fieldnotes play in the reflection on experience, I first pose a simple question—*what is a fieldnote?* Defining just what a fieldnote is and what it is not, however, is extremely complicated. According to Jean Jackson's survey of seventy social scientists, the concern of "what *is* a fieldnote" is widespread (1990). Jackson's informants generally agree that a fieldnote represents the mediation between experience and representation, but few agree on what form that note takes. For some, fieldnotes range from "'raw' data, ideas that are marinating, and fairly done-to-a-turn diagrams and genealogical charts to be used in appendixes to a thesis or book," whereas for others fieldnotes represent part of a larger process of interpretation, "a record of one's reactions, a cryptic list of items to concentrate on, a preliminary stab at analysis" (1990:6–7).

Fieldnotes—often personal and inconsequential, forgotten, and missing from archives and collections of field materials—seldom, if ever, assume an authority in ethnographic writing. Why then focus on something as mundane as fieldnotes—the scribbles in journals, copious lists of names and relationships on index cards, quickly written remarks in shorthand on scraps of paper, quick and dirty musical transcriptions and song texts, notations from pitch pipe readings? Why, indeed, if fieldnotes are so far removed from the experience of field research, the act of "retreating from data" as one of Jackson's informants suggests (1990:24)? Field-notes can be critical—whether implicit or explicit—in the ongoing analytical process of one's field research. Once the fieldnote is written (in whatever form), we enter a new process of interpretation. This process, an attempt to understand personal and social experience, is one that changes perspectives and relationships to experiences. Refocusing on the fieldnote in this way challenges how we represent interpretation, calling into question the very notion of "original" experience.

Field Research versus Fieldnote versus Ethnography

Margaret Drewal regards field research action as performance, "placing the emphasis on the participant side of the participant/observer paradigm" (1992:11). If ethnomusicologists act out their role as cultural participants, "performing" their

participation, then fieldnotes—the products of observation and reflection, participation and interpretation, voices and sounds in the field—are also an integral aspect of social performance. Fieldnotes often act as ongoing and changeable scripts for the mediation between experience and interpretation/analyses, and in this way, fieldnotes join the process of performance as we continue to engage them in an ongoing process of interpretation. In the following fieldnote, my writing parallels the learning or "knowing" of a member of a *kwaya* I am working with:

> *After the session at Buguruni I go outside and chat with the* mwinjilisti *[evangelist]—very young with five children. Solomon sends someone off for a* pepsi baridi *[cold soda]. I drink half and hand the rest to Sampson who finishes the bottle. I go to find the rehearsal of the* kwaya ya vijana *[youth kwaya]. They work in one of the Sunday School classrooms behind the church. I walk in and sit on a bench in back. Mjema is teaching a new song, an interesting process to watch, although I feel Mjema begin to "perform" a bit when I enter the room. Someone (Mjema?) had written the text for the new song on the front blackboard, and everyone uses that as a guide. As is usual for a* mwalimu wa kwaya *[teacher of a kwaya], Mjema sings all the different voice parts in the appropriate register, cuing and making corrections, jumping from voice to voice part. At one point Mjema goes up to one of the younger* wanakwaya *[kwaya members] who is busily writing down the text, and snatches the pencil out of her hand, telling her that to learn the song she must listen with her ears, not with her pencil.* I replayed Mjema's words over and over in my head during the long dala dala [bus] ride home, and began to wonder if he was attempting to communicate this same sentiment to me indirectly— I had also busily scribbled down the song text as I listened to the rehearsal. *The pencil, the moving hand, the fieldnote in process—I was not the only one who used writing for remembering, as a trigger to later aid memory. Why can't I trust my ears, as Mjema suggested, to listen and to learn?*

As I reflected on this moment—during the process of writing the fieldnote—I began to challenge my peculiar, inscriptive way of knowing. Could I learn about musical culture just as efficiently with my ears as with my hands?

In the following passage from *African Rhythm and African Sensibility,* John Chernoff outlines his own view of the process of representation that occurs *between* experience and interpretation, *between* field research and ethnography. Chernoff experiences this process as a form of cultural translation:

> *The most important gap* for the participant-observer, therefore, is not between what he sees and what is there, but between his experience and how he is going to communicate it. In attempting to do anthropological research, to translate the "structures" and "processes" which appear in another culture into the textual structures of his own, a social scientist must evaluate his own experience with flexibility. Finding the proper level of abstraction to portray with fidelity both the relativity of his own viewpoint and the reality of the world he has witnessed necessarily involves *an act of interpretation.* (Chernoff 1979:11, emphasis added)

Although contemporary ethnomusicology no longer embraces the utopian desire to interpret *the* reality of the world, Chernoff nevertheless accurately defines the interpretative act as a process of abstraction. In this chapter I posit that fieldnotes serve as just such a critical "textual structure" in the initial stages of *epoché*, or an abstraction from and back to experience. Philosopher Don Ihde suggests that this process of *epoché* is a way of stepping back from "ordinary ways of looking, to set aside our usual assumptions regarding things," expanding our experiential horizons—much in the same way as writing notes in the field (1986[1977]:32). Experience in this sense includes much more than an "original" moment in time— it includes the moment of "texting" one's experience.

Writing Field Research/Writing Ethnography/Writing Experience/Writing Fieldnotes

Writing notes in the field is a highly interactive process of cultural translation, the engaging of a dialectic between the axiomatic and the observational. Yet, in this translation the paradox of the fieldnote first appears; to produce a fieldnote one must project forward in order to glance backward. A hermeneutic circle circumscribing interpretation occurs in the field; the fieldnote, a deliberate *epoché*, "changes" whatever experience it focuses on, whether through magnification, clarification, examination, or reduction. The fieldnote, the "heavy glop of material," as Van Maanen characterizes it, is an attempt at understanding, textualizing, and thus reinterpreting original experience.

The position and importance of fieldnotes are not often given enough air time in ethnomusicological ethnography; fieldnotes are most often represented within a linear model, seldom more than a stepping stone bridging the gap between field research and ethnography.[4] I posit that a more interactive model, one that locates fieldnotes in a position straddling the ranges of both field research action and ethnographic production, needs to emerge in writing on ethnographic field research theory and method.[5] In my own experience I have found that fieldnotes are integral to both the processes of field research and ethnography—they function as an intermediary point that links the processes of ethnography back to the processes of field research. With fieldnotes acting as such a fluid and malleable intermediary point, boundaries between experience and interpretation become less distinct, allowing ethnography to become more directly linked to experience, and field research to become an integral part of interpretation.

As a fieldworker, my ability to allow fieldnotes to function in these ways calls into question statements that attempt to isolate fieldnotes as "simply a form of writing" (Lederman 1990:73). Writing notes in the field is a much more interactive process, mediating between experience and interpretation (and between preconception and reflection). In the following dialogue with a fieldnote, I began to

realize that my usual process of writing notes in the field could take on greater significance when it was expected, anticipated, or even requested:

> I LEFT FOR MSEWE LATE IN THE AFTERNOON TO INTERVIEW MACHANGE—THIS TIME I WAS BY MYSELF. THE ROUTE WAS SIGNIFICANTLY DRYER, YET SEEMED TO TAKE LONGER THAN BEFORE. TWO WEEKS WITHOUT RAIN MADE A CONSIDERABLE DIFFERENCE—I COULD CROSS THE RIVER WITHOUT TAKING MY SHOES AND SOCKS OFF. *I meet Machange outside the Holiday Bar, and we share a cold soda. After a while Machange decides that it is too noisy to have a discussion there, so we leave the village area and head down toward the Lutheran Church. We greet a group of elders at the entrance to the church compound, and finally settle on a bench outside the church since there is no electricity inside the building. I begin by asking him about the* uongozi ya kwaya *[the leadership or organization of the kwaya], and he guides me through the various offices of the kwaya's leadership and their responsibilities. At one point Machange stops the conversation and asks that I make a note of all this information to insure that I understand correctly, despite the fact that I am listening, responding, and recording the conversation.* EACH TIME MACHANGE FINISHED DESCRIBING THE FUNCTIONS OF A PARTICULAR OFFICER HE WOULD THEN ASK IF I HAD UNDERSTOOD HIM. EVEN THOUGH I REPEATEDLY RESPONDED AFFIRMA-TIVELY HE WOULD TAKE MY HAND AND PLACE THE POINT OF MY PEN DIRECTLY ON THE SPOT WHERE THAT OFFICER SHOULD BE PLACED IN THE ORGANIZATIONAL CHART I WAS OUTLINING. *Only after I can map out the individual offices of the kwayas, and repeat the entire structure to him in Kiswahili, is he satisfied that I understand. Before I leave, he asks if I could come earlier the following week to see his cows, goats, and chickens. I tell him that I would be honored to see his animals.* Why was it so important for Machange to "see" me writing a note, a document of responsibilities of the individual officers of the kwaya? It was as if only through creating a visual record would Machange feel that I had come away from our conversation with a true representation of his words.

It was clear in this situation that inscription was equated with understanding. On reflection, I find it odd that my initial reaction to Machange's request to diagram the officers of a kwaya was to become defensive—I could remember all that he said, and I could transcribe and translate our interview from the audio recording I was making. This defensive posture is equally odd given my admitted propensity for linking "knowing" with "writing" in the field.

My ways of knowing in a field research situation are numerous, but they are largely based on the approach of the participant-observation model. My interests in the musical performance of individual and communal spirituality in Tanzanian kwayas, for example, led my wife and I to become members of *Kwaya ya Upendo* [The Love Choir], one of the kwayas supporting worship services at the Azania Front Lutheran Cathedral, and one of most respected kwayas in Dar Es Salaam. A relationship of interviewing and documenting, being interviewed and being documented was not sufficient for my primary field relationship—joining and

singing with the kwaya became the only mutually satisfying role I could assume with this community. The following fieldnote, in the form of a letter sent home, was written as I waited for an interview with the *mwalimu* of Kwaya ya Upendo, Gideon Mdegella. In the letter, and in a subsequent entry in my field diary, I began to understand a particular connection I was making with one aspect of my relationship with the men in my *kwaya*:

> *As* wanakwaya *["members of the* kwaya,*"] "insiders," we [Mona and myself] are fortunate to observe, experience, sing, pray, and participate in the everyday life of an East African* kwaya. *We are often invited to share meals in the homes of members of the* kwaya, *events that honor Mona and I and also our hosts, and give us the opportunity to get to know one another at a more intimate level.* Kwaya ya Upendo *experiences solemnity as well as exhilaration and laughter, and I take great joy at participating in the laughter and humor that is always present with the men in the* kwaya. It has taken a while for my Kiswahili to catch up to the communication and understanding of humor, but now when Mbala slaps my hand after I tell a joke I know that I've communicated something (whether it is what I intended to communicate or not). It has been difficult not having humor be a part of my everyday life. I never knew before how much I depended on it to communicate effectively.

Writing this letter triggered further understanding of my ability to participate in an important part of the everyday life of Kwaya ya Upendo, the constant joke telling of the male members.

In addition to preliminary expectations, such as that of humor, many of my initial, pre-field research hypotheses about musical performance in postmission kwaya communities were misdirected; I needed to redirect my field research project to consider the complexity of musical styles embraced by urban Tanzanian kwayas. Many of my early experiences of writing fieldnotes guided the future course of my research, serving quasi-therapeutic purposes, enabling questions to be asked and answered, problems solved. Eventually, my fieldnotes aided in the formulation of new ideas and responses. In my experience, the practice of writing daily—notes, diaries, journals—served a cathartic function, almost as an inner/outer dialogue with myself. The daily exercise of engaging in this dialogue allowed me to proceed, daily, through my field research experience. The primary role of fieldnotes in recording data (names, places, songs, etc.) perhaps becomes secondary at times when the fieldworker requires an outlet for introspection.

The following fieldnote excerpt not only documents a growing awareness of my increasing influence on Kwaya ya Upendo, but the process of writing also enabled me to formulate a question, and served as a reminder.

> The experience of *[Kwaya ya Upendo]* learning an african-american spiritual: *Mdegella asked me to select a piece, an American wimbo [song] that the kwaya could perform during the Easter season. I was, of course, reluctant at first to make such a suggestion, not wanting to "interfere" with the repertoire of the kwaya.*

*Mdegella was serious—he has asked me on three separate occasions. I finally decided that I couldn't refuse his request, and I suggested an African-American spiritual, "Were You There When They Crucified My Lord?" Initially, Mdegella told me that he would translate the spiritual's text into Kiswahili. However, when he began to teach the piece several weeks later he informed the kwaya that we would sing it in English! The kwaya received the news very quietly [*SILENCE IS UNUSUAL FOR THE KWAYA ANY TIME DURING A REHEARSAL.*]—and I could sense some resentment within the kwaya. Why would we sing a piece in English when few in the kwaya speak or understand English? Prestige? Elitism? Or, as I now suspect, could it be some token form of appreciation of my presence in the kwaya? Simple solution— ask Mdegella!*[6]

Through writing, a process of reassessing or stepping back from my initial assumption of elitism, I began to see my experience from a different perspective. In this case the fieldnote began by serving a purely descriptive function. The time spent writing the note and reflecting on the experience, however, produced new insight and led to further clarification. What I initially perceived as elitism was, as I later confirmed by asking Mdegella, an attempt to include me—the English-speaking ethnographer (and his spouse and field partner)—and my presumed musical and cultural language in the repertoire of the kwaya. It was not the fieldnote, but a combination of experience, time, reflection, writing, performing, and question asking that brought clarity to my initial experience of confusion.

The process of isolation inherent in writing fieldnotes—both physical and experiential—facilitates a change of focus. In a liminal state we become further separated from the experience of field research—field research itself is a liminal act, a prescribed *rite de passage*.[7] Writing the fieldnote just cited helped me to recognize that I could become (and ultimately act as) a responsible member of the very kwaya community from which I assumed I could maintain an objective distance. This excerpt demonstrates the process I went through to realize that I could no longer deny the power and authority my presence had to affect change in this kwaya community.

Fieldnotes stimulate reactions and remain an abstracted site for personal reflection and for the formation of original ideas, differing from other forms of reflection in that notes involve the observer in a physical process of organizing thoughts, ideas, and reactions to events in a uniquely visual way. In the following fieldnote written directly following an interview, the way in which I created a visual, written image forced me to see my questions from a different vantage point. I observe now, for example, that I deliberately set certain words apart from the rest by placing them within quotation marks. As my hand began to "see" my questions transform into words I began to question the consequences of a specific approach I had been adopting:

I turned to Masanga, the mzee *[elder] of the group and asked him, "can music cross over to become* 'muziki ya kienyeji' *[indigenous music]? Can* kwaya *music ever*

become 'muziki ya kienyeji' "? *I was sure that he would respond negatively—but he surprised me. He told me that the missionaries brought music from Europe a long time ago to his people, the Wanyamwezi of the Tabora region. The earliest mission efforts attempted to indigenize music, adapt it to Kinyamwezi expression. He went on to say that he thought* kwaya *music of the Wanyamwezi* should *be considered* "muziki ya kienyeji." I INTRODUCED THE POLITICALLY CORRECT TERM, "MUZIKI YA KIENYEJI," AND MZEE MASANGA RESPONDED BY USING THE ENGLISH TERM, "TRIBAL MUSIC." YET IN MY NOTES I QUOTE HIM AS USING THE TERM, "MUZIKI YA KIENYEJI." WHY DO I PURPOSELY TRANSLATE HIS USAGE OF "TRIBE" BACK INTO "MUZIKI YA KIENYEJI"? WAS I TRYING TO PROTECT MASANGA BY SUPPRESSING HIS USAGE OF THE WORD, OR AM I MERELY AVOIDING THE ISSUE? *Now, was he saying this because he thought that was what I wanted him to say? Or, was his statement so emphatic because he suspected a hierarchy inherent in my question and deflected (it's not "one or the other, it's both")? I think the latter.* WHAT ARE THE CONSEQUENCES OF MY POSING THE QUESTIONS I ASK PEOPLE TO ADDRESS? AM I AFFECTING CHANGE THROUGH MY PRESENCE? BY ASKING PEOPLE TO ASSIGN LABELS TO THE MUSICAL EXPRESSION OF THEIR SPIRITUALITY AM I FORCING A JUDGMENT AND EVALUATION? By using the term "African" earlier in our conversation did I communicate a hierarchy, attach a level of judgment to the concept? When they heard me say "African" or "European" did they hear me saying "good" and "bad"? By extension, do I mean "African—good" and "European—bad"? If so, I might have communicated that.

If such a thing as "original experience" exists, I thought mine in the scenario just outlined to be one of innocence; I was interested in identifying the symbiotic relationship of multiple cultural discourses, distinct musical styles existing side by side in many Tanzanian musical performance traditions. During the production of this particular fieldnote, however, I came to realize that what I communicated and what I felt could, in fact, be two distinct messages. My perspective on the original event—the conversation with Mzee Masanga—was redirected after engaging in the production of this fieldnote.

Fieldnotes are typically analyzed as data "accumulated, jealously preserved, duplicated, sent to an academic advisor, cross-referenced, selectively forgotten or manipulated later on" (Clifford 1990:63). In this way, fieldnotes are a step taken directly *after* a given experience and *before* representation in the form of ethnography. A simplified model for this generative, nonreflexive stance could be represented in the following diagram (in figure 13.1).

This linear approach to the description process of what it is we do as fieldworkers denies a basic and continuing interaction between each of these three levels. At the same time, the model does not admit that changes in original perception may very well occur before the act of producing the fieldnote. Fieldnotes are locked into the original moment of writing in this model, not allowing for cross-influence(s). Where does "knowing" occur in this model? Although I
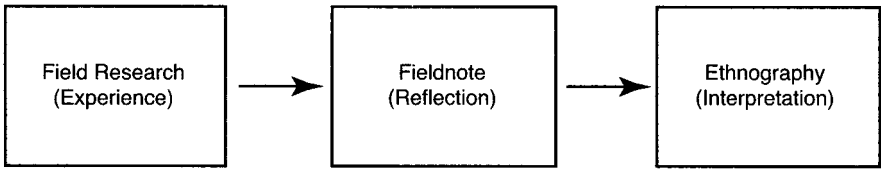
Fig. 13.1. A simple model outlining a typical placement of fieldnotes in the ethnographic process of "doing" and "explaining" fieldwork.

acknowledge that the model as I have outlined it is an overly simplified reduction, it nevertheless, reflects common treatment of the abstraction of reflection about experience from that experience itself.

Perhaps a more productive way, a better model for viewing the relationships that exist among field research, fieldnotes, and ethnography—relationships that are experienced by most fieldworkers—would include a more fluid interaction between the three elements. One of the principal purposes of any fieldnote is to support the foundation of both initial experience(s) and ultimate interpretation(s), acting as an adjustable fulcrum of sorts. If we extend this fieldnote–as–fulcrum metaphor to account for the constant flux of musical performance, then, as the position of the fulcrum's pivot point—supporting field research and ethnography—changes, so do the perspectives of initial experience and later interpretation. With the addition of an adjustable fulcrum, our model of field research becomes more interactive, allowing time, reflection, and change to assume greater roles in the mediation of knowing. The three elements of the model offered here—Field Research, Fieldnote, Ethnography—are no longer static and locked into place (fig. 13.2).

Although this second model conceptualizes the fieldnote as a fulcrum supporting experience and interpretation, it also illustrates the ability of experience
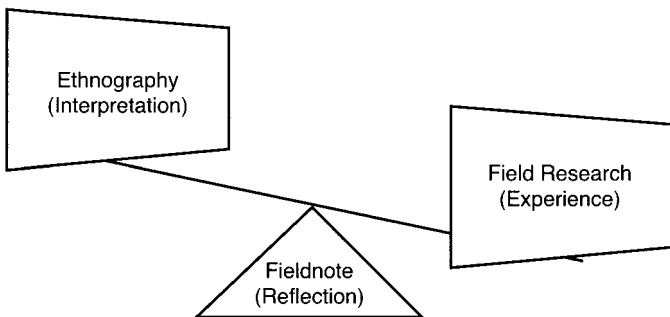
Fig. 13.2. A model that introduces the fieldnote as a fulcrum, supporting both interpretation and experience.

and interpretation to exist and interact without the aid of the fieldnote. This model also moves us beyond what James Clifford has suggested, that "turning to typewriter or notebook, one writes for occasions distant from the field, for oneself years later" (1990:64). Interpretation in this model is part of an ongoing process rather than a final product. There is, admittedly, something still missing from this model. It maintains individual experience in a position of alienation. This is rarely the case. Yet, the model does speak well to involving reflection in the overall process.

In the first, more linear model all arrows seem to point toward "Ethnography (Interpretation)" as the ultimate destination. Once reached, however, ethnography is seldom a comfortable resting place. One of the key reductionist points of the academic mission—to capture, categorize, structure, and discipline the practice of others into our own cultural system of the written ethnography—is downplayed in the second model. In this alternative model, the position of the fulcrum reflects the fieldworker's specific use of ethnography as an interaction with memory to understand how we can know what we know.

Talk of models, arrows, and fulcrums may obfuscate, however, what is understood by many to be a "natural process"—the fieldworker in the field doing field research. From my own experiences with musical performance, however, I am encouraged to explore an underlying concern with epistemology seldom approached by fieldworkers. The adeptness and artfulness behind the production of fieldnotes, specifically the inherently reflexive act of re-presentation, needs to be realigned with these epistemological concerns. What an individual fieldworker eventually selects to document is just as important as the methodology employed.

In *Powerhouse for God*, Jeff Todd Titon interacts with his fieldnotes in the process of writing ethnography. In the following excerpt, as Titon prepares his ethnography, his fieldnotes become increasingly interesting for what they do *not* contain:

> I failed to give enough thought to the likelihood that the members of the congregation would talk about us among themselves even after going along with our wishes. I look back over my field notebooks for signs of awareness and find almost nothing. An entry dated June 26 reads, "All extremely friendly." June 28, after attending a prayer meeting: "They were nervous about the recorder during the singing. As there were only 8 (& me) present I was conspicuous." They found out I played guitar and asked me to accompany their singing, but I refused, not wanting to intrude myself into what I was documenting (1988:18).

Titon's backward glances are a clear example of ethnography's interdependence with fieldnotes and memory, or "headnotes." Titon's fieldnotes function as more than "texts," more than words. In the example given here, his fieldnotes become a document of the absence of knowledge at a particular moment in the field. This example demands that we conceptualize fieldnotes in the second, more interactive model.

Other Voices, Other Texts

To support the alternate, more interactive model I now invoke the voices of others working within similar and different ethnographic models. The presence of the fieldnote can be felt in many of their ethnographic texts, and on closer examination we can determine the fluidity with which the fieldnote mediates between experience and interpretation. I begin with a brief didactic passage from an early popular introduction to the world's musics and peoples that outlines the uses of and need for fieldnotes in ethnomusicology:

> No matter how sophisticated your equipment is, you should carry a small pocket notebook. It will be useful for writing down names and addresses, directions, observations, and thoughts while in the field. . . . [N]otebooks are especially useful for preserving information learned in interviews . . . [Y]ou should make every effort to write down your detailed impressions of the overall field situation . . . your reactions and responses to the field experience as it takes place (Reck, Slobin, and Titon 1992:446).

Beyond the surface need for "preserving information," these authors suggest that fieldnotes—names, addresses, directions, and observations—support another, perhaps more significant process, the reflective process of responding to experience and interpreting experience through text. Although the process of writing notes in the field, specifically writing openly in front of colleagues, friends, and teaches, is not always considered the best method of documentation, the distance between jotting down information and taking the time to be reflexive is perhaps not all that great. It is also possible, however, to make the argument that writing fieldnotes while "in the field" can be a way of distancing oneself from experience rather than approaching it. As anthropologist Michael Jackson suggests, fieldnotes have their appropriate place, out of sight, or perhaps they should not be taken at all. Jackson suggests that only when we start to use our senses, "to listen, watch, smell, touch, dance, learn to cook, make mats, light a fire, farm," do we begin to make sense in our written notes (1989:9).

"Texting" one's experience is often a way of testing one's understanding of a situation. Anthropologist Edward Bruner suggests that an "ethnographic dialogue" develops when we enter into the practice of written interpretation (1986). The production of fieldnotes is a deliberate gesture, indicating a need for interaction, for "dialogue" with the various actors of social drama as well as with oneself. The "negotiation of the text," however, is just part of the reflexivity of fieldnotes, whatever form they take—whether entries in a field diary, scribbled notes on a scrap of paper, song texts, diagrams and outlines of performance spaces, or personal reflection (Bruner 1986:147–48). The process of producing a fieldnote is, of course, only one step in a complicated process of representation, and when combined with all the senses of experience a truly interactive ethnography emerges.

The transformation of experience into discourse leads to dialogue between one's knowing and reflection on that knowing, and the transcription is not easy.

An act of interpretation that goes unseen and unpublished, fieldnotes are often of use only to the individual fieldworker. They are produced for a specific, personal reason, to function in a specific ongoing and reflexive ethnographic inner dialogue within the fieldworker. Fieldnotes are intended to be links between experience and the later "text" of ethnography, and are commentary on the secret and private lives of the observer and the observed, yet the fieldnote often must be combined with headnotes in order to make sense. Introducing headnotes, a form of unwritten fieldnotes, into the first, more linear Field Research–Fieldnote–Ethnography model could cause havoc with the directionality of the arrows. When in "constant dialogue" with headnotes, fieldnotes facilitate an interpretation that is never final, but always engaged in an ongoing process of re-evaluation.

Not everyone views fieldnotes and headnotes in tandem, however. In *Tales of the Field*, John Van Maanen maintains an objectification of fieldnotes, locking them into one specific moment of time in the field by claiming they are "only a tiny fraction of the fieldworker's own memory of the research period" (1988:117–18). This attitude continues to divorce the fieldnote from the process that produced it, reducing fieldnotes to ink on paper. Perhaps the most significant response to this would be to reflect on the changes in shelf life of my own fieldnotes. They seem to make increasingly less sense as time goes by, conveying less meaning now that I have been back from my field research for almost a year. Will they continue to degenerate in this way? Are all of my fieldnotes really as "incomplete and insufficient" as Van Maanen suggests (1988:117–18)? Probably so, but here again, I have fallen into the trap of treating fieldnotes as texts rather than part of my personal, ongoing process of interpretation and understanding.

Perhaps one of the principal reasons fieldnotes are so difficult to deal with in a post-field research situation is that they served their main purpose while still in the field, yet these "secret papers" live and breathe new life into many later studies based on "original" field research experiences. The fieldnotes one protects as carryon luggage when leaving the field represent more than the negotiation between interpretation and experience; often, fieldnotes are a physical link, the trigger of memory, the sentimental reminder, or the source for new ideas and "translations." However, time seems to become the enemy of fieldnotes, creating an awkward distance between inscription and event. As Roger Sanjek has suggested, Malinowski was heavily dependent on his own fieldnotes while still in the field, using them as a form of analysis as well as for reflection and review, a way to spark new ways of interpreting experience, yet once out of the field they took on new meaning (1990:210). When I am in the field I ask many of the same questions that my historical colleagues asked. I believe that there are slight differences, however. Ethnomusicology now embraces so many wonderfully diverse ways of "being-in-the-world" musically, and I, as an ethnomusicologist, am comfortable

Fig. 13.3. *Male members of* Kwaya ya Upendo *during a break from a recording session at the Tanzanian Film Company. Front Row (seated): Sahdrock Kyambile, Goodvoice Materu, Jacobson Mmbaga, Gregory Barz, Charles Simpungwe. Back Row (standing and kneeling): Gideon Mdegella, Rishiael Tareto, Elioth Mujumba. Photograph by Mona Christenson Barz, also a member of* Kwaya ya Upendo.

exploring the periphery of shifting paradigms, so I am no longer concerned with defining what music is and what it is not.

Several not-too-distant disciplinary relatives in ethnomusicology entertained some of the same concerns with fieldnotes. In *The Ethnomusicologist*, for example, Mantle Hood includes several descriptions of the interaction between his headnotes and fieldnotes, outlining a fundamental interaction between experience and text. According to Hood, once ethnomusicologists involve themselves in the process of producing fieldnotes, they may "challenge" and further re-evaluate a particular experience (1982[1971]:229). The arrows in Hood's field research model—the negotiation between experience, representation, and fieldnotes—point in many directions.

The Fieldnote as Object

I look at several recently developed photographs that document my final recording session with Kwaya ya Upendo. *I eagerly make my way through the stack several times. Each time I linger on one particular photograph—a group of eight male kwaya members of the* kwaya *(including myself) relaxing during a break in the session. We meet to discuss whether to include a* pambio *[call-and-response chorus]*
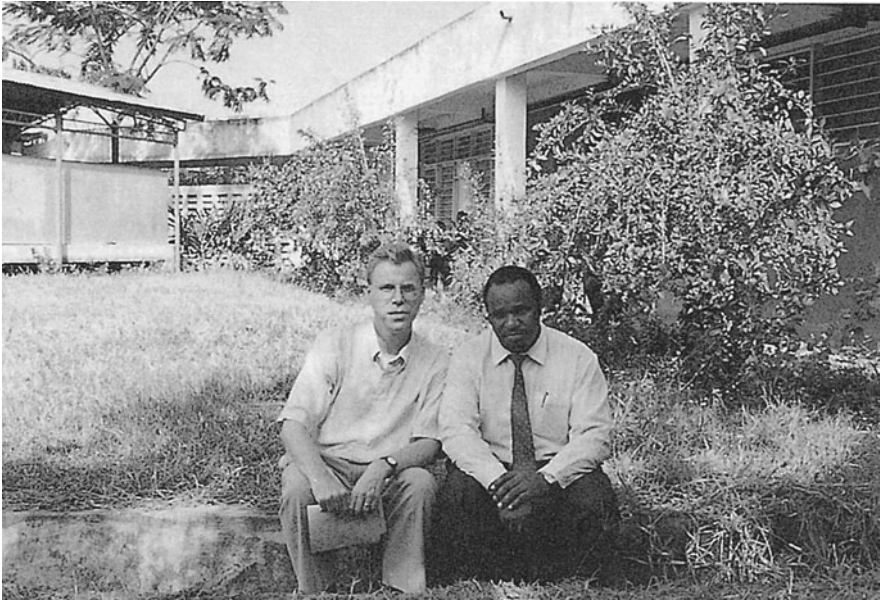
Fig. 13.4. James Obama, the mwenyekiti *[chairperson] of* Kwaya ya Upendo, *and Gregory Barz. Photograph by Mona Christenson Barz.*

on the cassette. The composition of the photograph is curious—we look strange sitting on the ground with our feet in the water drainage ditch. The photograph is slightly underexposed, and not everyone appears to be aware of when the shutter was actually released. *I remain focused on this photograph for quite some time.*

Just as I begin to move on through the stack I notice that I am holding a pen and a small notebook in this photograph. The notebook, an inexpensive type, mass-produced for Tanzanian school children, is folded open. I often carried a small notebook while working with kwayas to scribble down quick notes—observations, names, ideas, and thoughts to expand on at a later time. *I look at this photograph now and see the actual process of producing fieldnotes taking form in front of me. I am clutching this notebook as if preparing to write something down as soon as the camera is put away. Or, have I already jotted down a quick thought or observation?* I HAD NOT BEEN AWARE DURING THE ACTUAL RECORDING SESSION OF THE NOTEBOOK APPEARING AS PROMINENT AS IT IS IN THIS PHOTOGRAPH. Maybe it wasn't. AND, I CANNOT RECALL IF I DISTANCED MYSELF AT ALL FROM OTHER MEMBERS OF THE *KWAYA* BY CARRYING THIS NOTEBOOK. How would I know? How would I ever know unless I ask? *I move on through the stack and come to a photograph of the kwaya's chairperson, James Obama, and myself sitting in the same drainage ditch later in the afternoon at the end of the long recording session—the notebook and pen are still firmly rooted in my hands.*

Curiosity led me to search through my various field research journals and notebooks to find the particular fieldnote that would have been written at the time

of this photograph. Tucked behind a quick list of songs we were recording that day I found it. Even in its simplicity this note reveals an odd abstraction, an awareness achieved by pulling back.

> *How could I have ever seen myself as an intrusion? Kwaya ya Upendo doesn't need me to document—they are perfectly capable of documenting themselves.*

As if in answer to my earlier concerns as I worked my way through the stack of photographs, I uncovered a transition I had originally worked through during the recording session. I had addressed the very same issues at that moment as I do now when I look at this photograph. Did I really think that I wasn't an intrusion—notebook or no notebook? Surely all fieldworkers are. The quick-and-dirty fieldnote written hastily between takes at the recording studio suggests my realization that my personal documentation, or stepping back from experience, was normative within the kwaya; the kwaya was just as busy documenting itself—video and audio recordings. In this case, taking notes during the kwaya's recording session was not a way of stepping back from the kwaya, but a way of observing and reflecting in a manner that was not outside the experience of the kwaya itself.

Fieldnotes: Beyond Text

"*Having* notes . . . is one thing . . . [b]ut *using* them is quite another" (Lederman 1990:90). Most ethnographers would agree with Lederman that the fieldnote is ultimately supposed to be of some use, that it must serve some purpose in the later "writing up" stage, the construction of ethnography. A few authors have depended heavily on fieldnotes as a principal source of documentation, often published with little analysis, and there are also many who admit to not referring to fieldnotes at all during their "writing-up" stage. Whether referred to or not, fieldnotes move beyond text in their ability to communicate a sense of what was happening at a particular moment "in the field." Beyond documentation, fieldnotes often communicate to an audience many of the frustrations, reactions, conflicts, and troubles encountered in field research, communicating just how the fieldworker comes to know what she/he knows.

Recently, while preparing an essay—written both in and out of the field—on kwaya as popular music in Tanzania, I reviewed my fieldnotes looking for validation of what was to be the essay's main thesis: kwaya music is becoming a significant genre of contemporary Tanzania popular music. The strong feelings invoked when I (re-)read the following note challenged me to represent the multiple worlds in which kwaya music exists in Tanzania:

> *He is from Mbeya and didn't know anything about a Kwaya ya Maombolezo tradition there . . . When I asked him if his kwaya sang mapambio* [improvised call-and-response choruses] *he said that usually only vijana* [youths] *and "born agains" sing mapambio. This seems like a generalization, but it is probably true. A*

little discouraging was his read on the state of kwaya music in Tanzania. When I asked him about music in his village in Mbeya he told me that the kwaya only sang traditional Nyakyusa melodies with traditional harmonies, ngoma [drums], *and other Tanzanian instruments.* I HAD TO FIGHT THE URGE TO TELL HIM HOW WONDERFUL THAT MUST BE! *He went on to say that his kwaya at home could not sing Bach and Handel the way his kwaya in Dar Es Salaam could.* AT SOME LEVEL THE RELATIVIST IN ME TOOK A BREAK AND ALLOWED ME TO FEEL A LOSS FOR HIS CULTURE. I ALSO FELT ANGER AT TRADITIONAL EXPRESSIVE CULTURE BEING RELEGATED TO A SECONDCLASS STATUS, WHILE THE MUSIC OF THE WESTERN PENETRATION, COLONIZATION, AND CONVERSION PROCESSES RAISED TO NEAR WORSHIP STATUS.

Writing this fieldnote and rereading it now allows me to re-experience this moment, triggering many of the same conflicted emotions I felt when first talking with this young man: joy, anger, sadness, and frustration. This particular fieldnote served a therapeutic purpose at the time of writing, but now also reminds me of the need to embrace conflict in my interpretation of music in kwaya communities. The fieldnote in this case took me beyond the text, back to the field experience at the same time as it propelled me forward to interpretation.

Conclusion: Beyond "Keeping Good Field Notes"

No longer a marginal, or occulted, dimension, writing has emerged as central to what anthropologists do both *in the field* and thereafter. The fact that it has not until recently been portrayed or seriously discussed reflects the persistence of an ideology claiming transparency of representation and immediacy of experience. Writing reduced to method: *keeping good field notes*, making accurate maps, "writing up" results.

James Clifford 1986:2, emphasis added

The continuing presence of epistemological questions lurk close to the surface of my fieldnotes—"What do I know?" and "How can I know what I know?" As I continue to (re-)read the experiences entered as fieldnotes in my journals, notebooks, and diaries and listen to the many voices they contain, I am encouraged to renegotiate ideas, restructure hypotheses, question conclusions, and re-evaluate particular stances I have adopted. In this chapter I attempt to locate fieldnotes within an interactive system in which the production of fieldnotes continually affects and reaffects experience and interpretation, both in and out of the field. Fieldnotes play a major role in the overall performance of field research, and as such they are inter-dependent. The isolation of fieldnotes from the process that produces them denies change over time as well as the ability of experience to be continually re-evaluated. Writing in the field can move beyond what James Clifford suggests would be "keeping good field notes." Only when we release field notes from an objectification that reduces them to "heavy glop" and inexactness can we

begin to include our reactions, responses, and ongoing interaction with notes in our interpretive processes.

In this chapter, I also challenged the construction of cultural translation while still "in the field" by sharing several experiences that led me in my own field research to reject a linear approach to ethnography—Field Research–Fieldnote–Ethnography. Destabilization of this model allowed me to understand experience, understanding, and representation as interdependent. By focusing specifically on fieldnotes I actually focused on myself and on the epistemological processes of just how I came to know what I know. In a review essay of Roger Sanjek's *Fieldnotes*, anthropologist James Fernandez identifies the motivations of many fieldworkers searching for new field research/representation models to be a response to current "problems of reliability and credibility in ethnography" (1993:181). Although writing in the field may very well be just another way of "texting" one's experiences, it is, as I found out, a unique way to approach issues of reliability and readability.

Notes

1. Significant contributions to literature on relationships between ethnography and field research include: Clifford and Marcus 1986; Van Maanen 1988; Sanjek 1990; M. Jackson 1989; Atkinson 1990; and Emerson, Fretz, and Shaw 1995.

2. Earlier drafts of this chapter were read by Mona Christenson Barz, Timothy Cooley, Carolyn Schiller Johnson, Michelle Kisliuk, and Jeff Todd Titon. I am extremely grateful for their comments and suggestions.

3. I am indebted to Ellen Koskoff's presentation of ethnographic material of Lubavitcher women's song for the inspiration of presenting multiple styles/voices within an essay (1993).

4. See, for example, Bruno Nettl: "[T]here is no question that much of what I say is an interpretation of what they said, what I have read, and what I observed" (1989:x) and "In the most technical sense, ethnography describes culture synchronically" (1989:x, 8). Such a view creates a separation, divorcing experience from representation—in this case, the "ethnography" not the ethnographer does the description.

5. See, for example, A. Seeger 1987b for an example of the interactive field research-fieldnote-ethnography model. See also Feld 1990.

6. Mdegella passed away since the writing of this essay. That he has had a profound effect on my field research and that of many other ethnomusicologists working in East Africa is an understatement. I have documented the compositions and contributions of this great composer and choir director elsewhere (Barz 2000; 2001; 2003; 2004a; and 2004b).

7. I use liminality in this sense to refer to the separation of the field research from a "home" community while living in a "field" community. See Victor Turner for a discussion of liminality as the separation of individual from community (1989[1969]:102–8).

The Challenges of Human Relations in Ethnographic Enquiry

Examples from Arctic and Subarctic Fieldwork

Once a journey is designed, equipped, and put in process, a new factor enters and takes over. A trip, a safari, an exploration, is an entity, different from all other journeys. It has personality, temperament, individuality, uniqueness. A journey is a person in itself; no two are alike. And all plans, safeguards, policing and coercion are fruitless. We find after years of struggle that we do not take a trip; a trip takes us.

John Steinbeck, *Travels with Charley in Search of America*

John Steinbeck's wonderfully witty and judicious remark could easily apply to most of my field experiences and probably to those of many other researchers. Although it is seldom stated clearly in our writings, fieldworkers in any of the social sciences frequently need to alter their research plans at the last minute. These changes are felt to be beyond their command. Despite belief in the value of our scientific goals, despite painstaking preparations and appropriate behavior, the fact that both researcher and research objects are human beings cannot be dismissed. When human beings of different cultural backgrounds are brought together, their interaction proves difficult to predict.

This chapter explores some of the human dimensions involved in my own ethnomusicological fieldwork in Arctic and Subarctic contexts, and their direct bearing on my understanding of Inuit, Yupik, and Dene cultures. Within the field of ethnomusicology, this dimension of our work is infrequently discussed in detail, let alone analyzed. Perhaps this is so because it awakens raw emotions one hesitates to reveal publicly and perhaps it is also a consequence of our traditionally positivistic attitude toward scientific objectivity that forbids the emotional realm to enter the rational realm.

However, since Malinowski's time, when informants were referred to as the natives or the savages (see, for instance, his *Diary in the Strict Sense of the Term* 1989[1967]), the trend has been reversed. Most anthropologists now openly recognize and value the human reality and the vulnerability to emotions, enthusiasm, hang-ups, likes, and dislikes of their informants as well as their own, and their obvious impact in the field situation on interpersonal relationships. Most anthropologists also admit to the fact that entering a different cultural setting not only informs us about the "Other" but also in very significant ways enlightens us about ourselves. As Cesara states:

> [A]n anthropologist needs all aspects of his personality, not merely his drive to know; and he needs them under his command to produce good work. He is simultaneously an apprentice researcher and an apprentice human being until, that is, he has become a master of himself. (1982:100)

Although this is true of anyone, looking at oneself from outside one's habitual setting offers a privileged vantage point. In other words, the fieldwork experience and its introspective correlate accelerate personal growth. An anthropologist's work and writings should then reflect both his and his informant's humanity—an echo of anthropology's true mission as the study of humans.

Some anthropologists are particularly successful in this, and I am thinking here of Laura Bohannan [Eleonore Smith Bowen] (1964), Jean Briggs (1970), Manda Cesara (1982), Paul Rabinow (1983), Rosalie Wax (1971), and several others grouped under Peggy Golde's editorship (1986[1970]). Not only do these authors write about personal feelings and emotional responses, they are also able to weave their own and their hosts' personalities and expectations into their perception of the other culture. At the risk of presenting a candid portrait of myself, I firmly believe it is useful to investigate this further, and I shall try to describe and analyze some of my own explorations of other cultures. However, I wish to avoid what John Van Maanen's flippant irony designates as "confessional tales" because I do not wish to be drawn into a simple description of a "fieldwork odyssey" (1988:75). Furthermore, I have no doubt that this discussion is relevant because it helps demystify our sources of knowledge. This raises crucial epistemological questions about knowledge and knowledge acquisition through fieldwork—the same kinds we might ask of historical documents. The difference is that in the process of fieldwork we are face-to-face with the authors of our information.

Over the last twenty-odd years, since 1974, my experience as a field researcher in Inuit, Yupik, and Dene cultures parallels my growth as an ethnomusicologist and as a human being. My discussion here reflects the different stages I went through. For instance, moving from a research assistantship, to the writing of a doctoral dissertation, to autonomous research, represents academic growth. Over the same period, my resourcefulness as a fieldworker was tested by the immense

cultural differences I found between each of the northern settings I had the opportunity to explore. In whichever northern context I worked, I perhaps grew accustomed to travel in small planes and by snowmobile, to eating unusual foods, or to sleeping in summer daylight and working in winter darkness. I have also learned, however, that the material aspects of cultures are less difficult to adapt to than different peoples' outlooks on life. The latter is what affected me most as a person. Thus, my growing experience was not merely a matter of developing adaptive mechanisms or of refining techniques for collecting knowledge—in or out of the field. More important, it meant a growing awareness of the variety and richness of human behaviors, including my own.

Prologue: A Bit of Human Geography

With a few small exceptions, my fieldwork experiences have all been northern, that is, in the northern part of the American continent, with the Inuit (in southern Baffin Land and northern Québec), the Yupik (an Eskimoan group in southwest Alaska), and the Dene Indians (Canada's Northwest Territories). Some features typical of northern settings have had a direct impact on my stays and are therefore worthy of mention here.

First is the matter of isolation. My fieldwork is in small and distant communities, rarely connected by roads. Plane travel is available but rather expensive. If two communities are sufficiently close, that is, less than a hundred miles or so from each other, locals travel from one to the other by snowmobile in winter or by boat in summer. In some areas, winter roads on frozen rivers and frozen muskeg allow huge trucks to deliver construction materials and the store's dry goods for the year. During the three or four months that these roads are open there is a constant flow between communities and especially on weekends to and from communities where liquor is sold. In the dead of winter when roads (rivers and muskeg) are securely frozen, or during the short summer months when traveling by boat is easy, a community virtually empties on weekends, when people either go camping or visiting and drinking. Traveling is a vital component of northern peoples' traditional lifestyles and does not constitute a problem for them as it did for me in that setting.

As a fieldworker, weekends really mean very little to me—they might be used for work because days in the field are numbered. I am also accustomed to moving around at will. In the northern context, when no work can be accomplished and I am unable to travel, I feel like a prisoner. As a fieldworker, those weekends when the towns empty out are especially frustrating. Without my own truck, boat, or snowmobile it is difficult to go beyond the village limits. It is also difficult to catch a ride with another person because traveling conveyances quickly fill up with family and friends.

Besides, where would I go? Drinking trips frankly do not interest me, and the strangeness of the space makes it risky for me just to take a solitary walk. In some

arctic communities I was warned about the presence in the area of polar bears, and people cautioned me against going out alone or without a gun. I mention this not to impress anyone with the exoticness of my experience. I simply want to emphasize the feeling of imprisonment and dependency throughout any such trip. Although there are some occasional snowmobile rides and picnics or outings with friends, weekends entail long hours of inactivity, hours spent fretting and wondering if something could be accomplished by being a little more imaginative.

Another interesting facet of the northern communities I visited is that, unlike most North American Native communities, these are not reservations. Despite the important social problems plaguing many of these small communities, people still proudly identify themselves as users and caretakers of their land. And beyond frequent expressions of resentment toward non-Native people, I never perceive that they—Inuit or Dene—feel dwarfed by a dominant culture. This means that in those areas, the non-Native is the outsider, a stranger in their land, on their home ground, trapped in a world where time and space take on unfamiliar meanings. A trapped stranger is what I become.

Another inescapable reality of any kind of northern project is the question of permits. One must request the Band Council's authorization to conduct research in a given community—in writing beforehand, and in person during a Council meeting as soon as one arrives. In some communities this is a mere formality; in others it requires veritable negotiations. These communities have been so often and so extensively "studied" by a variety of specialists that they are understandably fed up with the summer invasion of student-apprentice researchers, and wish to exercise some control over them. However, providing that a community is assured of reaping benefits from one's work, permission is usually granted, though this still does not guarantee people's participation. With each individual solicited another negotiation takes place, not only about the money offered for interviews but also about the ultimate goals of the work. We must constantly justify why we have come and explain our interest in these matters. This last concern will be discussed at length later.

The time of year is a also a factor to be considered. I personally prefer winter, partly to avoid the summer invasion of researchers, but especially to witness the predominant climatic reality of their lives. Summer trips, although physically easier, only yield an extremely reduced view of the northern life-style. Furthermore, the short summer inspires leisure—more than usual—sometimes making it a lot harder to get anyone to work for me on a regular basis. Just as it is in my milieu, gentler weather encourages family camping and picnicking and just taking it easy in general. On nice days, people just disappear. Although many drum dances and intercommunity festivals take place in summer, they last late into the sunlit nights with consequent disruptions of everyone's sleeping patterns, my own included. Thus, in northern regions I feel that the colder months teach me more about individual people's lives than the warm ones.

The Age of Innocence

When I look back on my first trip to the Arctic in 1974, I realize, somewhat uncomfortably, how little I knew about what I was getting into. At the time, I was a musicology student and working as a research assistant in a group intent on developing a rigorous analytical method that we hoped could be applied to any musical object, whatever the tradition or the source. To a degree this method would, from a "sound" point of view, neutralize any difference between, for instance, Debussy piano music and Inuit music. It was thus decided that three of the group's assistants would study Inuit throat singing, and a few months were spent discussing possible approaches to this material. However, we soon had to admit that our musicological training was of little use for a cross-cultural study. Consequently, we drastically changed our approach and enrolled in an Anthropology program specializing in Inuit culture and language. However, our first trip north took place before enrollment, armed with only a few days of special tutoring which consisted mainly in preparing an interview questionnaire and in learning how to handle tape recorders and microphones.

I believe it is useful to recall these details because they partly shaped the assumptions and expectations, both conscious and unconscious, we carried with us into the field. Looking back on and analyzing the roots of our development, even though in retrospect they appear naive and fragile, can be put to good use as long as the conclusions reached become a lesson learned.

We were literally sent on an errand. That is, each of us was sent to an Inuit community and expected to collect recordings of Inuit throat singing and some information about this unusual vocal form. One assumption was that this mission could be carried out as easily as a trip to the library with the additional merits of some physical discomforts. Consequently it was thought that a few weeks would yield enough information to allow the analytical process to begin. Another assumption was that we should focus mainly on what we were sent out to collect separately from anything else the culture had to offer. In other words, we thought we would deal with a well-defined object of study. Furthermore, we assumed that our object was Inuit "music," since the rare recorded samples of throat singing we could find were obviously "organized sound." We were still very much the products of musicological studies, and we had little awareness of the sociological implications of any expressive behavior such as singing and dancing. Neither did we have any idea what a psychological and intellectual impact these trips would have on our work and on ourselves.

Thus, each of us set off for a different community with a sense of responding to a challenge, both intellectual and physical—and, in my case, of responding to an attraction I had always felt for northern regions. But I remember our fears, not so much of the unknown but rather of falling short of expectations. We assumed that the difficulties encountered could result only from our own shortcomings because

the informant was expected to be flawless. Within our assumptions the informant barely existed anyway—we were going to collect, and only incidentally to meet people.

I also carried other assumptions stemming from my own personality and from inexperience in a northern fieldwork context. For one, I believed it would be easy to convince people of the value of this project if I presented it with enough enthusiasm and adequate justification. Then, I assumed that I would be responsible for my work in the field and that six weeks would be plenty of time if all was well organized. I also took for granted that asking questions would generate responses. Finally, I had planned to address elders and other knowledgeable people, imagining that my interpreters would merely translate. Little did I know!

This first fieldwork experience taught me several lessons. Among other things, I came to understand that no expressive behavior exists in isolation from its cultural context and therefore that the shortest route toward real understanding is still the long way around—that I needed to see the forest as well as the trees. After a while, I also came to realize that my absorption with analytical concerns did not in the least interest the people I was visiting. More important, I awakened to the fact that "organized sound" did not automatically mean "music" and that the throat singing of the Inuit should rather be called "throat sound playing." Thus, it became irrelevant to examine these games in the light of Inuit singing traditions; rather, it was their playing traditions that shed light on many aspects of their performance.

I learned that I could not instruct people to work when and how I thought they should, even when I was paying them. In other words, even for money, my work was not a priority in their lives. On both my first and second trips to an Inuit community, I had to wait for two to three weeks before any concrete work could be accomplished. At the time, I was still unaware of the learning process that was taking place in spite of my apparent unemployment. I therefore had to adjust to other ways of gathering information and learning.

Finally, I began to understand that human relationships rather than methodology determined the quantity and quality of the information gathered. Social relationships within an Inuit community, which included me only during research periods, relationships between informants and interpreters, between myself, informants and interpreters, and between myself and the community at large—a below-the-surface human network of friendships, enmities, or rivalries over which I had little control—influenced my results in important ways. Subsequently, the human challenges this kind of research presented became one of the reasons why it has continued to attract me. I still prefer people to libraries!

The Nonmodel Approach

In subsequent years, the pattern of field trips was influenced by my first experiences. Familiarity with many ethnomusicological and anthropological paradigms

concerning both methodology and ideology never erased, in my mind, the importance of human interactions and the development of relationships as the real sources of learning in the field. Thus, I very consciously resist the temptation of going into the field with a set theoretical model, although I never forget who I am and where I come from (both personally and academically), and the implicit analytic assumptions I necessarily uphold. Instead, having broadly outlined my interests, I feel that a more general preoccupation with ethnographic enquiry and an attitude of receptivity to whatever people want to teach me is more revealing than a very focused approach. This is not to be confused with lack of preparation. On the contrary, preliminary readings and reflections are essential, for although they frame my inevitable assumptions, they also pave the way toward the openness I wish to attain. Neither does my choice imply criticism of other fieldworkers' models. My point here and in the rest of this chapter is to demonstrate how major an influence human interactions are—interactions that can hardly fit into theoretical models. There are, of course, drawbacks as well as advantages to the nonmodel method—try, for instance, to write a grant application from a nonmodel approach, or to write up results without a theoretical framework to shape the writing!

By being as receptive as possible, I leave it to informants to choose the manner in which they wish to instruct me and to decide in which directions they will channel me. With some, this gets me nowhere at all, perhaps because it goes against the usual stereotype of sure-minded white people, but others acknowledge that I progressed since they last saw me and judge that I am ready to reach a little deeper into their culture.

I became especially aware of this while discussing with the director of the Dene Cultural Institute, a woman of considerable Western education, the types of narratives that were told to me on my first, second, and third visits to Denendeh. To her the progression was clear. Not only did the elders assess that my mind was ready for more information, but they also believed that I was handling the information respectfully—an attitude expected of all those learning, whatever their age. They were feeding my soul as well as my tape recorder.

Often, when hoping to record songs, I have been surprised (and, I confess, a little annoyed) at the number of stories the Dene always come back to. Had I been reluctant to listen to these, I would really have missed a lot. Not only would I have missed the narratives' content which is otherwise unobtainable, but I probably would have underestimated the importance of storytelling for the transmission of ethical values. Aside from teaching traditional tales, which recount mythological beliefs as well as historical events and heroes, these stories teach young Dene "to listen," an essential condition for the survival of body and soul. A youngster who does not "listen" does not learn survival skills, whether physical or social, and will probably "not live long"; therefore, elders might not even take the trouble to "talk" to this youngster. Hence, I was also being taught to *listen*, a quality I could never achieve by *asking* too many biased questions. Learning to listen is also learning to

expect and to trust that truth, in due time, will reveal itself. Moreover, some Dene stories are of particular interest for ethnomusicologists because they teach about certain types of normally secret songs. In the context of stories, these songs find a secular outlet, which enables the listener to hear them and to absorb their textual and musical stylistic characteristics.

Another example drawn from my first visit to Denendeh illustrates how an "historical reconstruction" bias might have obscured the meaning of an important shift in the singing, drumming, and dancing tradition of the Dene. Right from the start, I was told repeatedly that "the drum started not long ago." I was puzzled, having assumed like many of us that the drum had been one of the most important and tenacious cultural items of America's First Nations over the centuries. On the other hand, a statement about their gambling game going back a "long, long time ago," a game in which drumming is essential, made the first statement appear like a contradiction. Only by listening to several apparently unrelated stories, by paying close attention to their wording, and by making observations in church, at drum dances, and in private homes, did I eventually understand that "not long ago" referred to the advent of Dene prophets. This occurred toward the end of the nineteenth century, simultaneously with the resolution of some intertribal conflicts. Thus, it was the drum's present usage and its relationships with a new spiritual mode that "started not long ago" (see Beaudry 1992). To have drawn attention to this historical contradiction or "error" by rushing into a series of questions with a need to "order" chronological events might have signaled that I doubted the storytellers or that I was still incapable of relating what they were telling me to real life.

Perhaps this nonmodel approach that I came to believe in is influenced to a great extent by the particular cultures I encountered. In other regions or in other cultures I may not have so strongly felt the thrust of their teachings. However, this approach, at first instinctive, not even well articulated, and reflecting my personal aversion to excessive planning, has proved valuable after all in northern settings. Many unexpected matters would have remained hidden had I not allowed this nonmodel approach to emerge. For instance, only time spent living with an Inuit family—when I became the butt of much teasing and laughter—gradually revealed (to me) the multifaceted importance of laughing and teasing in this culture and consequently helped me understand this most important dimension of the throat games (see Beaudry 1988). I really doubt this could have been uncovered by asking people why they laughed all the time or, worse still, by limiting my understanding to the "sound-producing" qualities of the individuals involved in the game.

Time Frame

Field trips were squeezed between academic terms and within the financial means at my disposal; thus, the length of the different trips ranged from six weeks to four months, with an average of two months per trip. This is different from the

long-term, total immersion approach required for most degrees in anthropology or ethnomusicology; unfortunately, it could not be organized any other way. Nevertheless, some advantages derive from my fieldwork time frame.

One advantage is that I get relief from a situation with strong emotional overtones—a break that allows me to revert to my normal self while considering the field period with some perspective. I was then able to weave both in-situation and out-of-situation perspectives into a more objective whole that I transformed quite rapidly into a learning experience. Going away after collecting a certain quantity of information facilitates ordering that information, making it presentable and workable, and seeing its strengths and weaknesses. It is a time for redefining issues. A subsequent field trip is then necessarily nourished both from the previous one and from the period of absorption following it.

As to the rough material collected—recordings of interviews and songs—I have always found it extremely difficult to synthesize and interpret what I was learning while I was in the field situation. I spent some apparently empty days restlessly fretting over wasted time because my days in the community were limited, rather than in intense intellectual reflection. Away from the field, on the other hand, there is time for making sense of an extremely varied collection of material. Directions appear that seem to have some emic significance, logical threads are revealed, connections are made possible between observations which at first seemed totally disconnected. It is a time for close examination of the informants' very words in search of clues to the meanings of things.

Practical and Methodological Issues

Working through Interpreters

One of the drawbacks of the back-and-forth method of field travel is the increased difficulty of learning the language properly. In all three settings I developed some language skills but not enough proficiency to handle in-depth conversations and interviews. Therefore, I necessarily worked with interpreters. Some people I interviewed could talk to me in English; nevertheless, they felt more at ease in their own language because in this manner they felt they were addressing the people of their community rather than me.

Over the years I worked with a great many interpreters, and it is difficult to summarize this particular working condition simply. On the whole, it is my relationship with them that caused me the strongest conflicting emotions— frustration, anguish, and discomfort as well as joy, warmth, and thankfulness—in good part because these were the people that I spent the most time with. As I said earlier, I first assumed that interpreters would simply translate for me and that I would remain in control of my mission. However, in all three cultures explored, several things became apparent right from the start.

First, I now understand how in northern communities I come to depend on interpreters, not only for translation, but also for introductions to the right people and for advice about matters of etiquette, language, events, interesting subjects, plane schedules, and prices. The list is endless when it comes to things that have to be learned quickly. Moreover, interpreters are in touch with daily events and with the undercurrents of social life in the community. They know about the problems, the illnesses, the moods of the people I want to see. They are also in touch with the gossip and the rumors, some of which are directed at me. The success of my field trips is in their hands. Interpreters are even more than assistants; they assume the role of elder sibling as they help me to socialize and enculturate properly, although this happens within the relatively short span of each trip.

Second, the many hours I spend with interpreter-assistants leads to the development of varying degrees of friendship. Friendship ensures pleasurable work periods, but it necessarily entails reciprocal responsibilities. For example, being the one-from-the-south who comes to find something that the one-from-the-north can provide, I am expected to simply explain what I want and he or she sets out to get it for me. Conversely, I must be understanding, that is, not critical, when he or she gets up late, when a child is sick, when a sister needs help, or when he or she really doesn't feel like working. As a result, most times, the assistants' schedules define the work schedule. Furthermore, implicit equality among friends allows the assistants to feel free to substitute themselves for me. For instance, when we go on visiting rounds inviting people for interviews and singing sessions, sometimes little, if any of the transaction between interpreter and informant is translated to me, and informants address their questions to the interpreter rather than to me. My role is often reduced to standing there, smiling, trusting that all is well, and thanking people without really knowing what is going on. When I insist on being included in conversations, some assistants make real efforts for a few hours but soon revert to their more natural manner.

Third, I developed a preference for working with middle-aged people. Younger people might speak English more proficiently, but many were educated away from their families and communities, and are thus somewhat estranged from their traditional culture. The English language skills of the middle-aged group might be weaker, but these people have a much better sense of their own language and culture, and of the older people's idiom, having been raised mainly "on the land" or "in the bush." However, the middle-aged people often have many children and responsibilities, and are involved in many adult activities. They have obligations and pressures of their own, and many of them refuse to work for me full time. Ideally, I must make arrangements with several people in order to fill my workdays. Matching their schedules with those of the informants sometimes constitutes quite a juggling act because they change all the time.

Fourth, as I said earlier, it is mainly on the assistants' terms that work gets accomplished, and this leads to many frustrating situations. Usually, the first week

goes relatively well. We adjust schedules, spend time discussing the implications of the project, visit people who seem the most relevant and the most amenable to interviews, and generally learn to work with each other. Then the novelty wears off, daily life reclaims them, and my work and money take second rank. Children become a priority; visits to the health clinic become a priority; sleeping off all night card games becomes a priority; cutting wood becomes a priority; doing laundry before the water truck comes becomes a priority—all activities which for some reason can never be planned beforehand.

I often do not understand why an assistant does not show up for work, and I am always tempted to go look for him or her. I came to understand that insistence on my part is not appropriate. Instead, understanding their priorities demonstrates my respect for them. Secretly, though, I cannot help feeling that my own priorities are often not respected—a cause for private anger. To make this sound a little less paranoid, I must explain that northern Native cultures value autonomy and in-dependence. When an assistant or an informant changes his or her mind about working with me, most of them assume that this is not a problem and that I will find something else to do. Assistants show their respect by recognizing (or as-suming) my ability to function alone as well as with them.

When I realize that I have been "stood up," I then have the problem of finding something else to do. Just walking around, trying to stumble onto something interesting is a limited method when it is forty degrees below zero ($-40°$ F) outside and windy! Ironically enough, they know about white people's need of structured time, and often, if I happen to be a bit late they do not allow for that. They might just take off, telling me later that I wasn't there! They aren't offended. They just go and do something else. This is all extremely frustrating for me because many hours are wasted just waiting for the person I am dependent on. It is all the more frustrating because being an adult I don't envision myself as dependent. It is also frustrating because my own culture values time "used" versus time "wasted," and frustrating in the particular context of short trips because in the researcher's world, funding agencies need tangible proof of our efforts—that is, recordings, photo-graphs, and so on.

Still, visiting and recording are, in my experience, what the assistants like best. Other tasks such as retranslating a recent interview are often felt to be somewhat boring, and the success of these tasks depends completely on the relationship that develops between us, and between us and the informants.

Participant Observation

At this point, a brief discussion of some methods of information acquisition is in order because this is closely linked with the development of my relationships with the people in the community and with assistants in particular.

Early on, I was fascinated by the participant-observation approach. Never-theless, once in the field situation I could not easily distinguish between par-

ticipation and observation. Living in a community requires a twenty-four-hour-a-day involvement, which constitutes, in my opinion, a constant observation method. On the other hand, I attend many events that most of the community's non-Native people never bother to attend. Does that qualify as participation? It seems that the expression is at best superfluous. Perhaps it is more interesting to try to understand how people assess my presence and my activities in a community.

When I attend religious services (masses, funerals, weddings), feasts, or sports games, I might think I am "just watching," but the northern cultures I have worked in value someone's presence as much for pleasure as for the social gesture it implies. The Inuit especially assess an individual's mental health and well-being by the amount of interaction he or she engages in. Loners are suspect because they implicitly refuse interaction. However, watching a game does not mean one has to play. Individuals do as they please. Just being there is a demonstration of willingness to engage in social interaction that comprises both those doing and those watching. Just "watching" or "observing" in an anthropological sense is meaningless because in itself it is asocial and therefore threatening.

In Denendeh, when I attend Catholic masses people believe I do so out of conviction, although my purpose, which I can never really share with anyone lest they feel watched, is only that of observing. When attending feasts, I listen to speeches (which I don't understand) with all the others, wait with the others the prescribed amount of time, and then eat just as hungrily as the others. People don't talk to me very much, but I am there, my presence acknowledged by much hand shaking. In their terms, I am participating.

On some occasions such as group dances ("Eskimo dances," fiddle dances, or drum dances), again my status is mixed. My fancy recording equipment is more noticeable than all the other cassette recorders around, and this somewhat isolates me from the others. Even when I sometimes get up and dance with the others, a form of participation expected of me, the recording equipment makes people feel and sometimes resent that I am observing.

Recently, I started taking notes at drum dances mainly for the purpose of complementing my recordings. Every time I do so, it elicits strong reactions and causes malaise around me—"What's that?" "What's that for?" "Can I see what you're writing?" These questions come at me from children, adults, elders—from everybody. Everyone remains polite except for drunks, who, less inhibited, often get really angry at me. On one occasion, my note taking was even mentioned at a Band Council meeting. Although many understand that non-Native people's proverbial lack of memory forces us to write things down, this makes some feel more of an object than does recording or photographing. This is a touchy issue: by "just recording," I behave as a proper human being, a friendly and pleasant one at that, whereas with a notepad I become an observation tool, and in their assessment, not friendly and not pleasant.

Interviewing

Interaction with individuals constitutes my principal method of acquiring information. I spend a lot of time and money interviewing a lot of people, but it's well worth it. It is my shortcut into the transmission network. I put myself in a position where I am told things because I do not spend enough time in the community to learn it all by observation alone. Interviews usually take place in the presence of an interpreter-assistant and are conducted with one person at a time. Everything is recorded. Most of the interviewing is done inside people's homes where they seem to be more comfortable, even with all the disruptions this entails. Some recording sessions attract other members of the household and occasional visitors. On other occasions people take us to a quieter room. Nevertheless, because isolation is abnormal, children and other family members do not hesitate to come in with requests or simply to listen.

The format of the recorded interviews has changed over the years, ranging from my first year's prepared-questionnaire approach (which I quickly rejected), to loosely prepared questions, to semistructured conversations with attempts to initiate singing, to life-story telling, and to storytelling in general. As time goes by, my own development as a fieldworker allows me to relax when something is discussed that seems a bit remote from my interest in songs and musical event descriptions. But I must confess that deciding on the thrust of the conversation is often taken out of my hands. A discussion between my assistant and the person being recorded usually takes place at the beginning of an encounter. Often, little of this is translated to me, and I have learned over the years that I have to stand ready for anything. Sometimes, I am told to ask questions if I want, but often the answers are not translated, or are translated incompletely. At other times I can easily spend an hour recording someone uninterrupted by translation, trusting that all will be translated at a later time.

Asking Questions

Asking questions is problematic in northern cultures. First of all, if the assistants are considered young it is contrary to northern Native etiquette for them to ask questions. A young person should wait until he or she is told something, as it is up to the elders to decide when a young person is ready to hear things. However, when the assistant is old enough to discuss certain matters with elders, personal rivalries or gender differences prevent him or her from answering the assistant. With one male assistant an elderly woman protested that certain matters were too intimate to talk about and just giggled when I was with him. This same woman responded differently when I was helped by a woman. Most of the time it is difficult to analyze an elder's avoidance of one of my queries and to decide whether reluctance is caused by my outsider status or by the presence of the assistant.

Second, for the Inuit especially, asking questions is a mark of mental incapacity. In a culture that values learning by observation and imitation, only the village idiot goes around asking questions. As an outsider I am forgiven for asking questions, but the assistant remains reluctant to proceed like this, so ingrained is the habit of waiting for information to reveal itself.

Third, when the assistant does ask questions, another cultural injunction must be broken for my sake. Informants' answers tend to be lengthy, and in order to translate properly the assistant would have to interrupt the conversation—an impertinent behavior by any northern standard. Assistants are thus caught between their desire to show respect and my need to know what is going on. One assistant, even after weeks of working together, had never really come to terms with this problem. Time and again he entered into an untranslated conversation with an informant, then turned to me saying, "Do you want to ask another question?"

On my first trips, it was hard for me to let go of the question-and-answer format. Euro-Americans value the act of asking questions as a mark of intelligence and healthy inquisitiveness, and I feel the obligation of participating actively in the learning process. Besides, if I do not get an immediate translation I start to feel lost and not in control of the situation, fearing that informants might go off on tangents of their own. I trust certain people, but with others I find it difficult to hide my dissatisfaction, and in this way I make everybody uncomfortable. Though interference on my part often helps to straighten matters out, it sometimes destroys people's concentration.

It is significant that both assistants and informants dislike the question-and-answer format I occasionally adopt. There are several reasons for this. Many find this procedure so boring that they lose interest in the subject and provide only short, uninteresting answers. In keeping in close touch (through translation) with what is said, I lose out on the quality of the content. Furthermore, these are people that see each other nearly every day. Suddenly, through working with me, they are prevented from addressing each other naturally in an attempt to respect the rhythm of my conversation. Finally, middle-aged assistants are justified in feeling that they could give the answers themselves or that they could think of better ways of getting the information. Depending on their personalities and on the relationship that has developed between us, they may or may not take the initiative to ask questions on their own. When they do, the interview often switches to a conversation between assistant and informant.

Retranslating

Ideally, all my interviews must be retranslated. Even when things are translated during the recording, a lot is still missing. Assistants themselves say so when we leave someone's house. The near impossibility of interrupting an elder before reaching the end of a thought makes it very difficult for a translator to remember

all the details of the longer stories. As was discussed earlier, the short question-and-answer format is severely limited in usefulness, and most of my recordings consist of fairly long statements that need to be retranslated after the interview.

When retranslating, the assistants often supplement the informant's answers with their own understanding and explanations. Retranslation sessions, as time- and money-consuming as they might be, represent the most precious moments of all my field experiences—times when I have learned the most. In addition, unless there are personality clashes, it is on these occasions that intimacy develops between an assistant and myself. We work hard—the assistant translating while I write down everything—but we also yawn, complain about boring bits, or laugh at humorous stories. Woven through hard work, stories about my assistants' lives and about mine are exchanged. After a while I even get the latest gossip. On a recent trip, my assistant, a woman of my age, started crying while listening to a song on the tape she was translating. Her grandmother had been singing this song at the time of her father's recent death, precisely at the moment he passed away. It is not the song itself that evoked such emotion but the occasion it recalled. For both of us there was room for emotion while working together.

During these sessions I relax. I am myself in a one-on-one relationship with someone with whom I can laugh, talk, and work. Beyond the pleasantness, I learn about what makes people laugh, complain, and cry. I learn through gossipy conversation who is friends with whom, who is lazy, who has been drinking, who is stingy (and therefore what stingy means), and who is generous (and therefore what generosity means). This is the next best thing to spending a lot of time in a community.

Ethical Issues

"What Do You Want?"

In the fieldwork setting professional motivations are often confused with personal ones. Over and over, acquaintances, friends, informants, interpreters, and assistants want to know why I do all this. As one Dene said to a white friend of mine during a late drum dance, "She looks so tired. Why doesn't she just go home?" In some cases, as when I lived with an Inuit family, the people can see that I am submitting myself to somewhat uncomfortable living conditions. Then, though I express wanting to know about the culture, I soon demonstrate that I am more interested in certain things. This is hardly congruent with their idea of learning the culture. What could I really want?

Northern people are now acquainted with masses of university students who come in search of dissertation material, and they wonder why they are targeted. For many elderly people and for most of the middle-aged generation, obtaining a university degree still represents something of a mystery. It means education, a lot

of education, but beyond that the notion is hazy and seen only from the perspective of eventually getting a job. Knowledge for the sake of knowledge, and not for the sake of ensuring livelihood, is not a well-understood motivation. Within this perspective they feel as if they are being used like objects—observed, analyzed, written about, and left.

Working toward a degree nevertheless remains a fairly concrete motivation. It then appears even more surprising that when this degree business is over we start other similar projects, this time with no obvious reason. As researchers we are something of a puzzle, obtaining and spending a lot of grant money to accomplish something totally unrelated to our own community and lives. Thus, a suspicion arises that we must be doing it for money or for personal advancement. For a long while, we do not escape identification with some of the non-Native people who come to the North precisely for the purpose of making money. Will we be different? Will we interview them, pay them modest fees claiming that we have a small grant, and then return south where we will reap all kinds of benefits from their generosity?

This is certainly the sorest point in my relationships with the northern communities. It crops up regularly in all my trips and generates the most resentment and hostile remarks toward my work. It seems that I will never find the right way to explain the difference between exploration and exploitation. It is also embarrassing to tell them that however important and beautiful their traditions are to them, "down south" only a limited public will be really interested enough to buy the book I say I will write. If nobody is interested, then why am I doing it?

When they wonder about the possible benefits to the community—or to them as individuals—of this work they are even more surprised and suspicious when I invoke the argument of preservation. They may agree and feel that this is truly useful for their cultural survival, but what then does the researcher get out of it? Is it possible that, like missionaries, we do this strictly for altruistic reasons? Not likely!

Obviously, the problem is one of classification. Where do we fit? How do we tie into their lives? Neither nurse, nor teacher, nor Sister, nor social worker, nor government representative—what are we doing there? In one small Inuit community I stayed with a family into which I was adopted. However comforting it is to think that I was adopted because I was nice, in retrospect, I humbly understand that it was easier for that family to create a slot for me within its immediate social network if I was assigned a status as a member of the family. However, this status did not define me outside my adoptive family because I didn't stay long enough for that to happen. For the rest of the village I retained my outsider status—an outsider whose motivations were far from clear (as I will demonstrate).

The relationships we develop in the field are conditioned by the status we are finally given: the single woman visitor or student who must be helped; the researcher (whatever that means) with money enough for such an expensive trip, but

not quite enough to achieve the prestige of a rich person; the priest's and Sister's friend; the recording person who does not represent any radio or television network; the "asker-of-questions" (a nickname the Inuit gave me); or the nice person. Strangely enough, I never achieved the status of employer, although I handled a lot of money and paid everyone that worked for me. Was it because I am a woman or because I remained such a dependent employer (as I explained earlier), or is my failure to be regarded as an employer a compliment? After all, in a colonial context an employer is rarely the best of friends. Maybe, then, they understand that, like them, I am first and foremost a human being.

From a human point of view, the transient nature of my involvement with northern communities stands in the way of the development of deeper friendships. Whether I remain in a community for six weeks or six months, everyone is conscious of the fact that one day I will leave. This aspect of faraway fieldwork has also affected me, and I, too, sometimes hold back, protecting myself as they might from the discomfort of separation. On the other hand, to some people outsiders such as fieldworkers represent a breath of fresh air, new ideas and new persons to whom secrets can be told without fear that they will eventually be known by all. It is precisely because we go away that we hardly represent a menace to privacy.

Although the professional benefits I will reap from these trips are apparent (to me), the communities' own interests are not so obvious. For one, there is a profound and unfortunately legitimate fear that once again, the researcher will take all this "cultural" material home, leaving them nothing, a fear that their trust, interest, and attention has been wasted, and a fear that they have been double-crossed. This has happened so often in the past that it is a wonder that they trust researchers at all, a witness to their enduring faith in human trustworthiness.

Who Do You Think You Are?

In spite of overall friendly relations, the suspicions and fears my hard-to-explain presence evokes lie close to the surface. In nearly every village I have visited I was taken for a spy of one form or another. There was one occasion when this accusation had particularly harmful consequences on my work. It happened in 1980 at Easter time in a very small community of Arctic Québec. One afternoon, the community hall opened in preparation for festivities—games and dances—of the week-long Easter festival. For once I had the unusual and unexpected opportunity to prepare my own recording equipment in advance. This hall also housed the local radio station, and I socialized for a moment with the people there.

When I left the hall, a friend of mine, rather drunk and funny, followed me out. In his drunken state, he made lewd remarks and offers that I just walked away from without paying much attention. A few hours later, as people started gathering in the hall and I showed up with my recording paraphernalia, the Band Council leader came to me, unsmiling, and said, "No recording tonight!" I asked why with a

smile but still got the same stony-faced reply, "No recording tonight!" From that moment I started noticing that nobody was talking to me, or even looking at me. This was very unusual because Inuit people are always outwardly friendly and smiling.

I put the recording equipment away but spent the entire evening there, determined not to look upset or guilty about anything. Inside I was bewildered and feeling sorry for myself, wondering what had gone wrong. My assistant came into the hall later in the evening, and she spoke to me naturally enough, but it was obvious that she was also very disturbed about something. She never said a word, did not play games, and never danced, although she stayed close to me all evening. I didn't have a clue what was wrong with her.

After twenty-four hours of this I discovered through the nurse's boyfriend that my drunken friend had announced on the radio that I had come to study their community and culture so that the government would learn how to trick them better. I was astounded at their reasoning because on that trip I had mostly been researching traditional games, in my view hardly a politically threatening subject. This occurred during the James Bay Agreement negotiation. This was one of the three Inuit villages against signing, and understandably they felt extremely vulnerable and small.

As for my assistant, I later learned that my drunken buddy had gone home after his announcement and beaten his wife—my assistant's sister—severely. My assistant had witnessed it all, incapable of doing anything. It is no wonder that she was too upset to help me, even though she knew what had been said on the radio was false. At least I had a firm friendship with her and I could express my fear of alienating my adopted family. I needed her urgently because talking myself out of this was beyond my Inuttitut skills. When she explained to them what happened they just laughed and said they never worried about the words of a drunkard. Although my family still trusted me, the rest of the community was another matter. We tried to resume work after the Easter festivities were over, but to no avail. People avoided me. One man even left his house in a hurry when I entered—a very meaningful avoidance tactic for an Inuit. Working with people had come to a standstill.

It took a few days for my friend to come out of his drunken state, remembering nothing. I described to him what he had done and what happened as a result, not asking him to do anything. That very same day, he went back to the radio station of his own free will and made a public apology, saying that when he was drunk he did not know what he was saying. He urged people to welcome me again into their homes. I was truly grateful for his gesture, but the harm could not be undone. My remaining fieldwork time was short. About half of the people became friendly again, and had I stayed long enough, things would probably have straightened out, but the work pattern was broken.

Beyond the distress such events caused me, there are things to be learned from the experience. For one, suspicions and fears about my motivations obviously lie close to the surface, ready to erupt instantly—and the community stands ready with self-protective measures. Also close to the surface were my own feelings of dismay and fear because I did not know what was going on, sadness at suddenly losing my friends, and anger, which I could not vent openly. There was not much I could do. I was getting a good taste of the Inuit's most powerful sanction: the deprivation of social interaction, which strips one of any social status whatsoever. Although frustrating professionally, the situation was even more upsetting personally. Although it was my work that had become suspect, it was my integrity that was being punished.

The fact that this happened was also a signal that I was perhaps getting too close, that I might be threatening to the Inuit in some ill-defined way. As one evolves from friendly guest to someone-who-knows-things, one's identity must be readjusted accordingly. This story was proof that I was seen differently than I had been before (this was my second trip there). Throughout that final period, I found it amazing that some people still believed in me. It was their own decision to believe the rumors or not. There was plenty of room for individual choice and action in this allegedly homogenous society.

On Whose Authority?

In none of the communities I visited was my "authority" taken for granted. On the contrary, this authority was constantly challenged, forcing me to question the frames I used for understanding. This is happening everywhere in the North. Julie Cruikshank, who has spent many years in the Yukon, speaks of a "recent explosion of critical local interest in ethnographic research in the North" (1988:27), which demonstrates that not only is one's approach to research scrutinized but that one's publications are also read by members of local communities. We have become accountable to our hosts.

In my experience, contestation took several forms, ranging from subtly voiced doubts about my capacity for ever understanding their culture, to resistance to the decisions regarding the content of interviews, and even to attempts at influencing my choices of kinds of material to look for and people to work with.

An example of this last kind of pressure happened during my first trip to Denendeh in 1987. For the first couple of weeks, meeting people and interviewing went fine. My assistant, a man a bit younger than myself and with whom I became quite friendly, was also the son and brother of two of my informants. After a while though, things came to a standstill, and a whole week went by without anything happening. My assistant had been on a drinking binge, and I had not yet found somebody else to work with. One day, still slightly drunk, he phoned me and told me that the men would not talk to me anymore because I paid the women as much as I paid them. They thought I should pay women less money than the men (!)

because what the men were talking to me about was more important. I could hardly believe what I was hearing, and I became so angry that I could not continue the conversation. Fortunately, I was alone in my house at the time and I didn't have to keep my face from showing anger. I felt trapped ethically. I have always tried to be respectful of the beliefs and practices of the people I am working with, but this was going too far because it meant moving beyond my own principles.

I decided to downplay this incident and just ignore it. After all, my assistant had been drunk when he said this, and I took the chance that he would not remember what he told me. Luckily, the incident did not go any further, and I never had to take a stand one way or another. I certainly did not change my wage practice! However, once my anger had abated I was left with several questions and one partial answer. How, for instance, did my gender affect my relationships? Did the men's exclusivity in handling the drum and the drum songs prevent a woman from asking about them? What does one do when one is profoundly provoked, especially when working in cultures that value emotional restraint? How does one deal with requests that demand a transformation of one's own ethical values? How much of oneself must one hide in order to reach one's objectives?

If throughout this chapter I have neutralized my feminine identity somewhat, this was only meant to focus better on my human identity as opposed to my professional one, but is gender a matter that can ever be left aside? Was it only a matter of gender that was at stake in the scenario I outlined earlier? Were these men trying to tell me something about the importance of the drumming and prophet song tradition? It seems, as I understood much later, that they were pointing at a hierarchy in song and story statuses, something they felt I needed to pay more attention to. My attention to women's songs and stories made them wonder if I could understand anything at all and if I was worth "talking" to. Fortunately, my work was eventually resumed with other assistants and informants, and with those who originally contested me. It was brought home to me rather forcefully, though, that they wanted to assume control over what I was to learn and when. It was their decision that I should not leave their community with a distorted (in their view) picture of their traditions. I had no control over this whatsoever and never really understood what had gone on until several trips later. For a long time I was blinded by gender anger.

"Who Do You Think We Are?"

The development and fostering of human relationships, however close or "objective," raise another dilemma for fieldworkers: what is the status (for us) of those we relate to? It is easy enough to make a case for our own ambiguous status in the field, but only recently have anthropologists begun to question the dangers of mixing business with pleasure. I have no solution to offer, only the expression of my own disquiet. Indeed, it would be professionally simpler to retain a rather neutral attitude toward the people I work with, but because of my interest in them

as human beings I am constantly shifting between recognizing them as either friends or informants and between my own roles as friend and observer. R. Laing identifies a distinction between "friends" and "organisms," that is, between people we see either as interacting with us or people as part of an organic process or system (quoted in Jay 1974[1969]:368). We need the friendships—they ensure the depth and truth of our understanding, but we also need to be able to detach ourselves from these same friends for the sake of observation.

This is particularly difficult when assistants become my friends. As explained earlier, they are the ones with whom I become the most intimate. But, what of the knowledge I gain from their intimate lives, their past and present sufferings, their opinions about other people in the community, and their emotional states? Such is the ransom for enjoying other people as human beings, as friends: discretion, respect for personal lives, and restraint in the information chosen for publication.

Furthermore, assistants often consider themselves to be the true specialists of their culture and sometimes question my decisions or my choice of directions. More and more they resent the fact that publications have not, in the anthropological tradition of the past, adequately represented the collaborative input of all participants. This represents (to them) a breach of professional etiquette and a breach of friendship. It is no wonder northern communities are ambivalent about our presence as researchers.

Again I turn to Julie Cruikshank's analysis of the evolution of anthropological research in the North:

> Increasingly, aboriginal people have their own ideas about the kind of relationship they want to establish with an anthropologist. Their expectations include considerably more sustained participation from the ethnographer than was the norm in the past. While this is certainly a contentious issue, it has to be addressed by every ethnographer working in the North.
>
> The model being negotiated in some northern communities is one based on collaboration between participants rather than research "by" the anthropologist "on" the community (1988:30).

Because of our university-oriented goals and the grant policies we work under, collaboration in the true sense of the word is sometimes difficult to set in motion. Although we take for granted that we are "in control" of our research goals, the physical and cultural distance we feel after leaving a community makes it difficult to implement fully collaborative measures. I hesitate to comment further on this aspect of my research because I have not yet resolved my dilemma, but I have been taught to recognize and value the intelligence and freshness of approach of the many highly dynamic individuals I have worked with.

This is an issue more directly concerned with the representation of a tradition and with the potential uses of analytical results, and perhaps this discussion belongs elsewhere. Let us not forget, however, that this has been fermenting for a long

time in northern Native communities, and that it has underscored all my relationships in the field, whether or not it was consciously felt by me or by the host communities.

Conclusion

In spite of all the unanswered questions, I can only reiterate what I have already stated. Human relationships not only influence the quality of my work but are what makes fieldwork a meaningful experience. Allowing friendships to develop or simply enjoying people as they are is not as simple as it sounds. Friendship and camaraderie are tainted with the pragmatic uses that could be made of them. This is a moral issue, one that can only be answered by individual experience. Because we must remain in control of our feelings and emotions at all times in the name of our research objectives, we might feel that we are prevented from fully being who we are. Beyond the frustration of keeping oneself in check, fieldwork remains a challenging experience because it teaches us that there are many different ways for human beings to be themselves.

Returning to the Ethnomusicological Past

Young Jewish guys from sixteen to twenty study the *Talmud* in a scene of total chaos, their monotonous sing-song penetrated from time to time by the grating shrill of the button-box accordion played by the innkeeper across the street.

Joseph Roth, "Die Juden von Deutsch-Kreuz und Schweh-Khilles"

Once there was a Jewish woman,
And she had a daughter
Who had prepared herself for death.

"Die schöne Jüdin," variant sung by the Deutschkreuzer Frauen, oral tradition

Traversing the Boundaries between Past and Present

Since the first edition of *Shadows in the Field* I have returned again and again to Burgenland, plying the boundary regions of the ethnomusicological past. I return to Burgenland somehow believing that I might hear more Jewish music in this Austrian province that forms the border between Central and Eastern Europe, but once epitomized the multicultural mix of *Mitteleuropa*. Over the course of the past decade, Burgenland has not lost its reputation as a noisy place and a musical landscape with the most unlikely of sonic mixes. That noise of pastness—the *Talmud* students after World War I, recovering the voices of great religious thinkers who went before, their reverie against the accordion player's irreverence— is no less vexing for me than for those ethnographers who went before. The silence of presentness—the Jewish girl ready to dance her death, playing out relentlessly against the Nietzschean tragedy of history—insists on ending so that it may begin again. Jewish past and present are locked in the love-death of a Jewish ballad that has outlived its very subject.

More and more, I am joined by others who return with me to Burgenland's ethnomusicological past. Not least among those engaging with Burgenland's Jewish music are Burgenland musicians today, the provincial composer's union, for example, that joined with the Austrian Jewish Museum in the provincial capital, Eisenstadt, for a conference on the "Music of Jews in Burgenland" in 2002 (Winkler 2006). The memorywork of Austrian and Israeli scholars, most of them young and compelled by the silence of the past, has appeared in print, envoicing the Jews who no longer live in Burgenland (for oral history see Lang, Tobler, and Tschögl 2004; for oral tradition see Dreo and Gmasz 1997). I, too, have striven to share the past with the young whose perspective comes almost entirely from their present, for example, when I taught a seminar on Jewish music in Austria in 1999 at the University of Vienna, and took my University of Chicago students from their study-abroad program in Vienna on a field trip to Burgenland. All of those whom I join in the field, I increasingly realize, hear the Jewish past differently. All make sense from its noise in ways that reveal as much about their present as the Jewish past. Had they not entered the field, had I not returned to the field, we could hardly have known this.

The modes of my ethnographic engagement with Burgenland's Jewish past have also proliferated since first writing about it for *Shadows in the Field.* Not surprisingly, I have written a number of different pieces, each different in style and form from all the others. In my writing, I have obsessively tried to provide something for as many readerships as I am capable—linguistically, rhetorically, and methodologically—of reaching. I write for readers in folk music and Jewish studies (e.g., Bohlman 2005; Bohlman 2006). I deliberately search for a voice that draws ethnomusicologists and historical musicologists together (e.g., Bohlman 2008, especially chapter 1). I performatively transform the music itself, to the extent that I have wrested it from the past, to sound a present that is meaningful (e.g., Bohlman and Holzapfel 2001; Bohlman 2002). In the boundary regions between past and present, I find myself less and less alone. There is more music, and there are more voices. The silence and the noise intensify. Their meaning arises because they draw me time after time into the field. Only in the return to the ethnomusicological past do I find comfort in the search for the meanings of both past and present in the Jewish music of Burgenland.

Jewish Burgenland between Past and Present

The Jewish communities of the *shevah kehillot*, the Seven Holy Cities, of Burgenland constituted, until the 1930s, a boundary region between Central Europe and Eastern Europe. The villages and small cities of the *shevah kehillot* and the areas of previously intensive Jewish settlement in this border region attract relatively little attention today. Most of them are unknown outside of Austria, although a few claim a bit of fame from famous musicians who once lived there: Franz Joseph

Haydn or the family of Fred Astaire in Eisenstadt, Karl Goldmark in Deutschkreuz, and Joseph Joachim in Kittsee. Some physical evidence of the Jewish past from the Seven Holy Cities survives: the synagogue in Kobersdorf; cemeteries, or at least the stones of cemeteries, rescued from the ravages of neglect; and streets and walls intended to bound the Jewish quarters of a town. Human evidence did not survive to the present quite so well; there are reputed to be three Jewish families still living in the Seven Holy Cities today, but no one knows who they are, and they do not identify themselves (Gold 1970).[1]

The most ethnically diverse province in Austria, with modern ethnicity consciously historicizing the diverse musical life of the past,[2] Burgenland witnesses virtually no Jewish music today.[3] Jewish musical life, for all intents and purposes, does not exist in Burgenland at the beginning of the twenty-first century. Nonetheless, it was the Jewish musical life of Burgenland that led me to this borderland between Central and Eastern Europe, this field located between the past and the present. The Seven Holy Cities were in many ways emblematic of Jewish musical life in Europe: a mixture of traditional and modern repertories; complex and contested practices; and a music history shaped by movement both toward and away from the conscious expression of Jewishness. This chapter examines why I choose fieldwork to draw me closer to the Jewish musical life of a past that no longer existed in the present.

The emergence of fieldwork as a research method in the social sciences has resulted to a large degree from its capacity to bring the scholar into contact with the present. The fieldworker not only makes observations in the present, but the present provides diverse frameworks for the several narratives reported by the fieldworker, through fieldnotes, accounts of participant-observation, or full-blown ethnographies. As a lived experience, fieldwork's encounter with the present is an uneasy paradox. On one hand, fieldwork takes place as an excursion into the culture of the Other. In contrast, however, fieldwork must account for everyday practices. The paradoxes proliferate as we attempt to connect the different elements in these statements. "Everyday" and "Other" seem counterpoised at opposite extremes; "culture" and "practice" exhibit no less disjuncture. Temporal considerations sharpen the paradox. Whereas the everyday and its practices would seem to unfold within the present, the culture of the Other requires a systematization, even ossification, of moments gone by. The present, therefore, is ongoing, but once inscribed in ethnography, it is marked by the syntax of pastness. The past, in contrast, is frozen in a timelessness, from which it must be wrenched to be synthesized into the presentness of history. The disjuncture between past and present makes it increasingly difficult for fieldwork to examine either, but necessary to examine both.[4]

I take the paradox and disjuncture of fieldwork as givens in this chapter. I do not try to resolve them; rather, I try to identify an ethnographic and historical space that they open. It is a discursive space of boundaries, not boundaries between

cultures, instead a space within which cultures locate themselves. These boundary spaces undo many of the categories that ethnomusicologists and those engaged in fieldwork have long taken for granted. Culture within these spaces no longer forms into systems, but rather becomes fluid, ephemeral, and contested. History can no longer be recuperated into teleological narratives that "once happened" and now can be told again and again in their inscribed versions. History, too, forms in a temporal space, contested because fragments of the past remain in the everyday of the present.

For the ethnomusicological fieldworker the boundaries between the past and present become themselves the "field," a space allowing one to experience and represent musical practices that are not simply inscriptions of the historical past or aural events of the immediate present. Although I examine one specific case of such a space, that of the Jewish musical past of Burgenland, I argue in this chapter that it is not unique, but rather representative of a wide range of ethnomusicological pasts. That there is no Jewish music to hear in Burgenland at the beginning of the twenty-first century results from the devastation of the Holocaust, which in turn specifies the historical conditions of the Jewish musical past in this boundary region between Eastern and Central Europe. Still, it would not be correct to assume that there was a singular Jewish musical past in Burgenland and that this past, as a whole, simply ended with the Holocaust. The Jewish presence in Burgenland was historically in constant transition. Jews adapted to changing legal restrictions and responded with other culture brokers to fulfill the political agendas of both Habsburg and Esterházy rulers in the area.[5] It was the malleability of Burgenland's Jewish communities that suited them to the contested nature of the historical and geographical spaces in which they lived. Fieldwork in the ethnomusicological past attempts to reckon with that malleability, not to bound "Jewish culture" or to determine bounded repertories of "Jewish music."

The ethnomusicological past always possesses complex meanings and requires different forms of ethnographic representation. At one level, the events of history do calibrate the ethnomusicological past, as, for example, the events of the conflict between empires—Habsburg and Ottoman—provided the initial reasons for transplanting diverse settlements of ethnic and religious Others to Burgenland, and then the events of the Holocaust provided the grounds for eliminating the Jewish and Roma presence from Burgenland. At another level, the ethnomusicological past, like the present studied by ethnomusicologists, comprises ritual practices, which use musical performance to reproduce selfness or to confirm the meanings of community and polity. History is constructed through the actions of musical and ritual specialists. At still another level, the ethnomusicological past exists as a web of seamless everyday musical practices, each one producing myriad moments of history. The musical practices at these different levels may or may not be connected, but within the ethnomusicological past they form through *bricolage* into complex musical meanings. They interact with each other because, as

processes formed out of performance, they occur within the boundary spaces between past and present.

The ethnomusicological past is not one past, but many. In this chapter I reflect on how ethnomusicologists might explore those many pasts. Throughout the chapter I employ several excursuses drawn from my own fieldwork, and I interleave these with more theoretical sections, in which I think through the ways in which different ethnomusicological pasts might be constructed. Together, these excursuses and methodological fragments do not constitute a method. I do not, myself, believe that they could, and perhaps this is an important caveat to keep in mind: Fieldwork, although it requires us to draw extensively from theory, is not a theoretical end in itself. It requires that we be prepared at all times for the unexpected and for the fluidity of experience. Fieldwork is at its theoretical best when it has the potential to respond to this fluidity and the experiences at the boundaries between cultures. I should like to argue in this chapter that ethnomusicological fieldwork may also be at its best when it brings us closer to the fluidity and experiences on the boundaries between the past and present.

Music in Burgenland's Jewish Pasts

Excursus: Remembering the Other. "We always got along well with the Jews." "They were our neighbors, and we never had any problems." "When they made music, we were there; when we had a dance, they were there." "They were taken away so quickly, we had no idea." Burgenlanders today have, by and large, not forgotten their former Jewish neighbors and the Jewish culture of Burgenland. Their memory of Burgenland's Jewish past takes place through remembering and recollection. It is a memory, like many memories, that consists of fragments, pieced together to help the fieldworker complete his or her narrative (cf. Kempowski 1979).

One of the most disturbing aspects of my initial interviews in Burgenland was that I encountered consultants whose memory of the Jewish past was exclusively positive. Although I always attempted to retain my objectivity and not to intrude in an interview with questions that might unnerve, I found it difficult to believe what I was hearing. I responded with mistrust, with a feeling that I should later need to reinterpret the tales of a slightly tarnished, but golden past. I had no idea how literally I might report this mistrust in a future ethnography. Even now, I am not sure how an ethnographer interprets information from the field that deliberately avoids the truth; were the ethnographer in the position of knowing the truth better than others, fieldwork would either be unnecessary or would turn into a form of indictment.

The Burgenlanders were not telling me lies; they were not taking advantage of the trust we had established through my residence in the area; nor were their tales about a good-neighborliness distorted by half-truths. Their memory of the Jewish

past was positive; it was also confused by the disjuncture and destruction that they had reformulated to fit the memory they had constructed. Their memory of the Jewish past had always formed from a pastiche of understanding and misunderstanding. The collective memory I began piecing together also contained considerable gaps, those boundaries with the past that made it difficult for me to distinguish understanding from misunderstanding. This, too, is a quality of the fragmentary nature of memory.

Boundaries were very significant in the Burgenlanders' accounts of the Jewish past. I came to realize this when I gradually began to gather accounts of Jewish burial practices and the rituals accompanying these. Older Burgenlanders still remembered the public aspects of Jewish funerals, processions they had witnessed and laments they had heard. Death and the ritual practices that mark it have not disappeared from the remembering of the Jewish past; the narrativizing of death, in fact, serves as a discursive connective between past and present, for Burgenland Jews used literature in various forms to remember the dead whose lives formed Burgenland's past (see, e.g., Wachstein 1926; Reiss 1995). Death figures into the remembering of the past in various ways. At the deepest level, there is the recognition that death in the Holocaust eventually greeted most of the Jews who were transported from Burgenland. At a surface level, there was the death of a single individual, a small girl in Burgenland, which terminated the blatant attempts to erase Jewish culture in Burgenland during the 1940s.[6] The death of the Jewish past also presses on the memory on an everyday level, through the insistent presence of Jewish cemeteries, which are everywhere to be found in Burgenland. Non-Jews did not pass beyond the boundaries of cemeteries; they did not take part in or observe the ritual that occurred when the community turned to its own religious practices. Just as death had arrested the attention of the Burgenlanders, it allowed them to recall music making and the musical practices that they imagined to take place beyond the boundaries:[7] the singing and prayer of the cortège; the singing emanating from beyond the cemetery's walls; the community's care for the bereaved family in the week and year after the death.

The fragments of Burgenland's remembered Jewish past are not all the same, and it was this realization that led to a further recognition that the tales I was hearing were not dishonest, nor were they attempts to cover up the atrocities of the past. Instead, the fragments of the Jewish past arose from different types of remembering. One set of memories resulted from the encounters between Jews and non-Jews, from a true sense of neighborliness that was necessitated by the large population of Jews in a multicultural society. Another set of memories resulted from the awareness of otherness, the inability to weave difference into the fabric of a single memory. These two sets of memories, I came to understand, were at odds with each other; in effect, they drove each other to even greater extremes with the passage of time. One set of memories increasingly focused on a shared past; the other set became ever more confused by the otherness of a culture whose

fragments were never understandable. The differences and otherness of the Jewish past entered the narratives of the ethnographic present; though they remained untold, these narratives constantly shaped how both present and past were re-membered.[8]

Crossing the Border between Present and Past. Shifting boundaries have ceaselessly mapped out Burgenland's historical past. The province's cultural geography is less a product of what was or what is, than a constant process of realigning borderlines to separate one political entity from another. The peoples of Burgenland have themselves seldom been that political entity, but rather have physically constituted the boundaries that serve as the shifting cartographic traces of the past (see the essays in Baumgartner, Müllner, and Münz 1989). It was, moreover, an aggressive settlement policy that first peopled Burgenland with difference and otherness. In the seventeenth century, when the Ottoman Empire's threat to Central Europe seemed greater than ever,[9] the Habsburg Monarchy mustered diverse settlement groups from the empire and placed them on the open, fertile plains of Burgenland. The defensive role played by the province is evident even in its name, literally "the land of the fortresses." In particular, Saxons from northeastern Germany and Croats were given land to attract them to Burgenland.

The opening up of the border region similarly attracted Jews and Roma, but for somewhat different reasons. Jews settled in large numbers at the end of the seventeenth century largely because they had been driven out of the Hungarian provincial capital, Ödenburg (today, Sopron), in the wake of the failure of the second Turkish siege of Vienna in 1683 (Ernst 1987:233–37). Roma found Burgenland to be an opportunity to map their own culture of diversity and mobility onto the region's historical diversity, discovering a remarkable fit.[10] With the gradual and final exit of the Ottomans from Europe in the eighteenth century, Burgenland had become a collective of otherness. That otherness would shape its history until the present (see *Burgenland* 1993).

The political and national boundaries of Burgenland's history necessarily affected the boundaries of the many local landscapes in the past, not least among them those of the Jewish community. Once Jews had settled in significant numbers in this boundary region, there remained the problem of how and where to locate their communities. Initially, that is after the expulsion from Ödenburg/Sopron, the Hungarian aristocratic families, Esterházy and Batthyány, provided economic and political protection for the Jewish settlements, and for this reason most of these settlements are directly adjacent to the palace or fortress of the various Esterházy or Batthyány family members, most of which were located in small or medium-sized towns. Accordingly, the Jewish quarter lay directly across from the center of power and culture, locating Jewish cultural and religious institutions as close to that center as possible.[11] Although the local boundaries produced by these historical interrelations deliberately demarcated the landscape, the boundaries existed only to be traversed; their separation of Jewish and non-Jewish communities was fig-

urative, but as such they made the complex multiculturalism of that landscape normative.

The shifting boundaries of Burgenland's historical landscape are not unique in Europe, and for Jewish regions of Europe they were relatively characteristic (Applebaum 1994; Wischenbart 1992). Indeed, "Jewish Europe" has never been characterized by fixed boundaries. The location of culture of Jewish communities in Europe has always occupied a region beyond the boundaries (Gruber 1992), where the lives of individuals and communities are less the product of regionalism or nationalism, but rather of in-betweenness (Bhabha 1994:1–9). We know the names of the regions beyond the boundaries, whose populations have historically been multicultural, but it is practically impossible to locate these regions on the maps of modernity. Burgenland, Galicia, Pannonia, Alsace, the Bukovina (see Applebaum 1994). These are just a few of the regions that are beyond the nation and outside of nationalistic histories (see, e.g., Deutsch and Pietsch [1990] and Noll [1991], for studies of the ways music has articulated such histories). Otherness has different meanings within such regions, for it is not an otherness stamped on minorities by nationalist-driven racism. Instead, it is a mutual otherness, an otherness produced by getting along with others rather than by stereotyping and excluding them. It was the form of otherness expressed by Burgenlanders in the 1990s as they described their Jewish neighbors more than fifty years ago.

Burgenland represents the many different ethnographic conditions that connect the present to the past. The musical practices and repertories of the province are no less a conjunction of diverse ethnicities and genres (Dreo, Burian, and Gmasz 1988; *Burgenland* 1993). As a site of Jewish history Burgenland is a place to investigate Jewish music in rural Europe, a region to compare with Galicia or Alsace (Baselgia 1993; Bohlman 1993; Dohrn 1991; Stauben 1986[1860]). Burgenland is multiethnic and multicultural, hence giving a specific context to Jewish music in both the present and the past. It is a border region, defined not so much by a defined identity as by the processes of change that mean that identity must always be negotiated (see Baumgartner, Müllner, and Münz [1989]; for a depiction of a musician engaged in the negotiation of Burgenlander identity see Reiterits 1988). There was never a single Jewish identity in Burgenland; Jewish culture could never be neatly circumscribed. The issue then is not "finding Jewish identity" but finding the conditions that negated Jewish identity (Baumgartner 1988; Gold 1970; Klampfer 1966; Spitzer 1995). These do not lie in the simple assumption that modern residents interpret the Jewish past as a history belonging to another culture.

The tales from the field, instead, narrate an entangled past, and the voices of the present reflect their own entanglement in that past (Van Maanen 1988). As an ethnomusicologist, then, I am motivated not by some presumed power to disentangle the present from the past. To imagine Burgenland's past as a world split between Jews and others might render the region comparable to what we call ethnic cultures throughout the world, but it would violate the historical dynamic

that results from the constantly shifting boundaries between Burgenland's past and present.

The Past's Fieldworks

Excursus: Fieldwork and My Past. Since dissertation research in Israel, begun in 1980, I have devoted much of my research to the study of Jewish music. Although the various projects I have undertaken examine different aspects of Jewish music, the music of European Jews, particularly Ashkenazic Jews in Central and East-Central Europe,[12] has provided the primary repertories at which I have looked. My methodologies have largely been historical, with my point of departure being the experience in the field. To study the musical life of the German-Jewish community of Israel, I engaged in fieldwork in Israel and Germany in the 1980s in order to understand processes of immigration in the 1930s (see Bohlman 1989a). To understand how new forms of Jewish broadsides mediated and represented the transformation of European Jewish communities at the turn of the last century, I explored the urban spaces that facilitated this transformation as the turn of the next century approached (Bohlman 1989b). I have placed myself in the spaces where Jewish communities had been, where Jewish music had been heard. What I encounter through fieldwork, however, consists at best of the traces of what was, or might have been.

My engagement with the past has persistently been personal, and yet it has been personal in ways difficult for many of my consultants to comprehend. My engagement with the ethnomusicological past of Central Europe and Germany has not been an engagement with *my* past, nor with a past my family or ancestors would claim. I am not Jewish, and I have no reason to believe that my ancestors from rural Pomerania, Ireland, or Wisconsin might have been Jewish. I am also neither German nor German American. I speak Hebrew and German, though both as languages I learned later in life and use with no special connections to ethnic or religious identity. The question then arises, is this relevant? Anthropologists and ethnomusicologists have long entered the field to study not their past but someone else's. The question about my own past is, in fact, very relevant because it is a question inevitably asked of me while I am in the field. Those with whom I consult in the field want to know who I am, where I come from, and how my identity relates to theirs. For various reasons, my answers to these questions are important to them, and I do not hesitate to explain to them that I am not Jewish, and it is not my ethnomusicological past I am trying to discover.

By responding to questions about my own relation to the past I am studying, I specify and alter the context for the fieldwork itself. At the most basic level, a new context develops, depending on whether the questioner is Jewish or non-Jewish. My response, then, is not simply a declaration of objectivity. Rarely does this contextualization of the fieldwork stop with the some sort of aphorism about

ethnographers being engaged with someone else's culture anyway. Why, then, should I be interested in this particular past, which is not my past? Many motivations lie behind this line of questioning. For some Jewish questioners, the motivation is a sort of amazement at encountering an anomaly, a non-Jew studying Jewish culture. For some non-Jewish questioners, the motivation is to determine just how far they might go before I begin to lay the burden of European anti-Semitism on them. Am I going to blame them for what I might discover about the past?

The conversation about the nature of my past continues, gradually passing through a transformation itself: It establishes a new discourse between myself, my consultants, and those not present to enter into our conversation. For my consultants, my responses become important because they suggest that I do not come to the field with a set of claims I want to prove or for which I am hoping to unearth new evidence. We establish the past as an ethnographic domain that I can only understand through the newly contextualized discourse of the present. Within this discourse, it may well be that I become the exotic Other, for I come entirely from the outside to learn from those connected to their own pasts in one way or another.

I do not deny that my response about my own identity and my relation to the past I am investigating changes the ethnographic present; it alters the path along which the fieldwork will go. I also do not deny that my particular identity—a non-Jew from North America studying the Jewish past in another part of the world—may unleash some confusion that is never completely resolved. All these issues of identity, nevertheless, foreground the problem of studying the past; indeed, they focus fieldwork on how the identity of the past itself is thrown into contrast by how the discourse of the ethnographic present unravels the identities of both fieldworker and consultants. It becomes increasingly important to know why we want to understand this ethnomusicological past, and knowing why may, in the best of circumstances, draw us slightly closer to the past lived by others we can no longer know.

Ethnomusicology's Pasts

The various ways of remembering the past produce many different histories. The plural in the preceding section heading represents this, if even also its ambiguity. In entering the past from the present, ethnomusicology must reckon with a wide range of differences, but it must also welcome that range to some degree. Fieldwork, particularly if we do not force it to become a set of methods, opens up modalities of interpretation that allow ethnomusicology to recognize these differences. In the present section I embark on discursive excursions into some of the modalities that might effectively contribute to ethnomusicology's engagement with the present and past. I mean this range to be inclusive, not exclusive, and I thereby make no claim to pinning down methods or privileging one modality over another. Ethnomusicological fieldwork is personal—it must be, or it would be

pointless—and the following modalities for turning fieldwork toward the past are also largely personal. They, too, must be.

These different modalities further allow us to recognize that each individual narrates the past not only as she remembers it through her own experiences, but in very selective ways. In the field, the past is conveyed to us through personal narratives and decisions about what to remember. This is evident in the ethnographic historiography that currently narrates the past of the Eastern Europe that was, in the West, synonymous with the world behind the Iron Curtain, the world of communism (Applebaum 1994; Ignatieff 1994). The resurgent Eastern European nationalisms since the transition from socialism, for example, selectively reflect the different types of past (for a history of the ways in which Western Europe imagined Eastern Europe, see Wolff [1994]). For nations secure in their national image emphasis falls on the struggle against communism as the underlying historical motivation for the past half-century. In the case of Poland this emphasis requires filtering out any other histories that might undermine the nationalistic image linking past and present, notably but not solely the nation's historical complicity with anti-Semitism (Hoffman 1993:34–35). The Balkan states, whose nationalism increasingly fends off national names and labels, have seemingly bottomless wells from which to draw different and competing histories. With all these nationalisms there exists the problem that "unwanted pasts" may infect the narrative of a "wanted present" (see Ignatieff 1994). In the fieldwork on which the excursions in this essay are based, the Holocaust stands—or rather its shadow lurks—as the archetypal unwanted past.

Ethnomusicological fieldwork, because of its concern with the narrative and performative agency of music, provides diverse ways of encountering these many pasts. In the following modalities I sketch these diverse ways, though without claiming to represent "the past." Different modalities render the voices of the past audible in different ways. Although each contains potential problems of audibility and therefore can only incompletely represent the past, they become ways of formulating fieldwork questions and of encountering many different pasts.

The Past as Other. The easiest premise for encountering the past is to say it's different from the present. Those who lived in the past are the Others. It's an easy premise, but it's a dangerous premise, and for that reason I begin with a modality of approaching the past's otherness. For ethnomusicology othering the past sometimes has the additional seductive quality of suggesting that one is addressing difference. The past contains the culture of the Other, and ethnomusicologists can therefore go about the business of contrasting it with the present. If the musical culture of the past is that of otherness, it becomes superfluous, even self-defeating, to connect it with the present and self. The music of the past was contained by the past; it stopped sounding, and to recuperate it for the present is only to museumize it and to pretend we can hear it. These are acts of exoticizing, of course, and to some extent such acts have increasingly been the subject of criticism within and without the social sciences

(see, e.g., Fabian 1983; Fischer 1993). I wish to argue here that fieldwork in the ethnomusicological past must not be immune from such criticism.

The otherness of the ethnomusicological past may allow us at times to slip dangerously close to fetishization. It might seem convenient to enforce the space between present and past by observing that, of course, the musical practices of the past were different. But what if such a claim were itself foreign to those in the past? What if, as in the case of many Jewish liturgical practices, the sameness of the past was precisely a music that ascribed identity? The spaces between present and past are far more variegated than the discursive spaces constructed through theory. It is not otherness, pure and simple, that lurks on the other side of the space. Through fieldwork, however, it becomes possible to encounter this space and decide just how, if at all, it is negotiable, and to judge just how the other side relates to selfness.

The Past as "Self." The otherness of the past as an experience in which one did not participate is often inseparable from the selfness of the past as an experience to which one draws closer through its narration (cf. Ricoeur 1992:140–68). The past's selfness is a constructed experience, and yet the location of the "self" in the past is one of the most powerful motivations for doing fieldwork in the past. In analytical philosophy the hermeneutics of the self is not primarily concerned with discovering oneself, but rather with the conditions of sameness and, by extension, of identity. Were we to turn to this hermeneutics of self in the fieldwork experience, we should enter on a different process of constructing identity, both the identity of the past itself and the identity of those who inhabited the past.

The use of fieldwork to interrogate the past as self is problematic on several levels. For many who search in the past for filiopietistic reasons, in other words to glorify their forebears, the selfness of the past is ipso facto a means of glorifying the present. Indeed, one is made blind to the otherness of self. Only the visibly present characteristics of the past are thrown into relief. Much ethnic folk-music research suffers from this dilemma of selfness. The constructs of the present (e.g., hyphenated folk musics such as Italian-American, African-American, or Irish-American) should have existed in the past because they do, presumably, in the present. A more hermeneutic consideration of self, however, would insist on problematizing the identity of self. Whose self is it that the fieldworker seeks to discover? His or her own, or someone else's? Must we assume that the identity of the past bears a relation to the identity of the present? Whose past does the self narrate when telling tales from the past?

The selfness and the otherness of the past are not unrelated, and it may well be their relatedness that makes it possible for fieldwork to examine identity. The question posed by hyphenated ethnic musics might therefore become not what the history of Italian-American folk song is, but to what extent have certain folk musics been identifiable as Italian-American as opposed to Catholic-American, Calabrian-American, or Neapolitan popular song. The otherness and sameness of these identities coexist, and the past takes shape from the tension implicit in this

coexistence. Fieldwork in the ethnomusicological past ideally reads beyond the simple presence of selfness and otherness to perceive how music brings competing identities into the tension of history.

The Past as Musical Object. Throughout the history of folk-song scholarship, it is the musical object that has most completely represented the past. A folk song comes into existence in the past, assuming authority over the past through claims made for its age and timelessness. For this reason, the folk song serves political ends, not least among them the incontrovertible evidence that the national existed even before the nation, indeed *was* the nation waiting for the moment when it would be allowed to be born. Music's timelessness is returned to time through oral tradition, which, reproduced through tune families or patterns of variance, connects the past to the present. It is the musical object, nonetheless, that enters the present from the past. Recuperating that object through fieldwork makes it possible to obtain a piece of the past.

For much folk-song scholarship, it is the objectification of music that ultimately valorizes and essentializes the past. This valorization is nowhere more evident than in the study of narrative genres. The German ballad embodies *Geschichte* and *Geschichte*, that is, "story" and "history." One notion of *Geschichte* is the past; the other connects it to the present. The objective nature of music surviving from the past, however, also erases parts of the past. If the folk song survives until the present, it has also overcome the power the past may have exerted on it. The past of the song, therefore, turns into a vast temporal space of otherness, where change has not happened, or at least has not impinged on the identity of the musical object. Still, the musical object does encode a version of the past. Experiencing it in the present provides a mode of decoding, if indeed one mode among many.

The Past as Everyday and Mentalité. Ethnomusicological fieldwork may seem ahistorical when it aims to capture the synchronic musical events of the everyday. The fieldworker going to a music culture other than her own describes a setting that simply is. It exists outside of history, and its musical events enter the ethnography as if the past and future of their existence have no particular bearing on the present. Synchronic interpretations contrast with diachronic, in which change either does or does not take place, but nonetheless possesses a dynamic of its own, distinct from that of the everyday (cf. de Certeau 1984).

The study of music in the everyday, however, also provides a historical framework for ethnomusicology, for it situates music in contexts that are independent of the extraordinary events of history, allowing for a different form of narrative imagination, the study of a *mentalité*. As the French historiographic school known as the *annalistes* discovered, the records allowing for a reconstruction of the everyday in the past assume very different forms, but they are nonetheless abundant. Observations concerning ritual performance or musical practices transmitted by oral tradition allow us gradually to understand who made

music and where it was made. What distinguishes the everyday is how specific events stand out against the fabric of community life, thereby creating a subaltern historical dynamic.

The everyday musical life of rural European Jewish communities emerges from the records in its synagogues and in the discussions about the nature of ritual. A rather more frightening everyday has been recorded in the anti-Semitic reports of ritual murder (see Hsia 1988) or the several dance forms that incorporate the typically Jewish, usually known simply as *Judentänze.* Such records reveal that the everyday required contestation and resistance for survival. From an ethnographic standpoint, however, such struggle in the everyday remains as prevalent at the end of the twentieth century as it was at earlier moments in the history of Jewish Europe. Observations the ethnomusicologist might make today concerning the struggle to maintain ritual observances in Hungary, notoriously anti-Semitic throughout much of the Cold War era, evoke the everyday contexts in which ritual observance in Burgenland might have taken place in the eighteenth century at the periphery of the court in Eisenstadt or after the shift of international borders in 1921. The musical practices of ritual provided the glue of an everyday whose history resulted from responding to the threat against it.

The Past as Oral Tradition.   The fabric that makes the everyday legible is oral tradition. It is a quality of the culture of the past and the present, hence it serves in the methodologies of folklore and ethnomusicology by connecting past and present. Through the experience of oral tradition in the field, the ethnographer tries to create a text for reading culture by moving backward through time. By interpreting the music of the present as linked to something previous through processes of either stability or change, it becomes possible to read backward through the past to a moment, perhaps, when only oral tradition existed. In this way, oral tradition may even render the past timeless.

As a theoretical framework oral tradition may connect the present to the past in different ways. Frequently, in the popular imagination, it is oral tradition that allows a community or culture to believe that some core of musical practices from the past—some essence of the past—remains intact in the present. Nineteenth- and early twentieth-century European concepts of Jewish music, for example, held that the liturgical practices of the contemporary synagogue, if truly Jewish, had been transmitted orally from the time before the destruction of the Temple in Jerusalem. So persuasive was this belief that it provided a means of explaining away the surface traits different regional styles shared with the musics of neighboring or surrounding cultures. The surface, inscribed by modern notational practices, was not what counted, but rather the unbroken transmission from cantor to cantor, from daily service to daily service (see Idelsohn 1932:vi).

In the second half of the twentieth century, ethnographic theory tended instead to privilege the processes of change that necessarily result from oral tradition. It follows that musical texts of the present are not those of the past, but

metonymically they represent the past. Certain types of change, therefore, are predictable, say the substitution of references in folk-song texts that are more meaningful to contemporary singers; other kinds of change predictably do not happen, say when stereotypes of the characters in narrative folk song remain intact even when the surrounding field of meanings changes. The construction of otherness in European genres such as the *Schnaderhüpfl* or in dance genres given names such as *Judentanz* ("Jewish dance") lead scholars to believe these are unchanging symbols of social criticism. Such constructions of otherness are often not meaningful in modern practice—images of Scottishness are not frequently reported by those dancing the *Schottisch*—and that is precisely the evidence that makes oral tradition a seductive means of approaching the past, for it functions without conscious maintenance of the past. Its power to connect present to past lies in its invisibility.

Past as Archaeology and Epistemic Knowledge. It is in the historiographic concepts of Michel Foucault that we most completely encounter the notion of understanding the past by encountering the ways in which its inhabitants constructed their self-knowledge (Foucault 1972). This epistemic knowledge is suggestive for the ethnomusicological fieldworker, not least because it resides in one of the premises with which fieldworkers struggle with objectivity in the present. The episteme in the archaeology of the past, however, produces a history through "the conditions of possibility" (Foucault 1970:xxii), and therefore we encounter it in the "space of knowledge" representing a music culture or community. Musical practices not only take place in this space of knowledge, but their performative nature means that they transform that space into a field for expressing identity.

The archaeological approach to the ethnomusicological past is potentially very productive. Musical practices are frequently central to identity and the knowledge of self, especially when groups must together make the decisions necessary for coherent performance. Epistemic knowledge does not reside at the surface of musical style; rather, it inheres in the spaces of a shared knowledge that makes performance possible. The shared knowledge of identity occupies many different spaces: the sacred space of religious ritual, the rules guiding performance in oral tradition or the knowledge of body that informs how individuals use dance to express their social connectedness. The knowledge of self is necessary for the continuation of musical practices, and hence it becomes a type of historical knowledge that connects past to present. Through fieldwork, it follows, one can attempt to locate musical knowledge that conveys a sense of self in its relation to the past. Methodologically, we have moved from perceiving musical objects in the past to understanding how music has historically served as a means of knowing the past.

Past as Contested Space. It is the performative nature of music that necessitates the interpretation of music within the spaces of past and present. Entering the field immediately brings the ethnomusicologist into the space where music has

been performed, and the nature of that space reflects the influences that performance exerts on the space that has once contained it. The interaction between performance and space is political in the sense that the conditions of performance have resulted from various hierarchies, various pressures on the public space of music to contain and define its public, its audience. The space of the musical past is not infrequently a contested space, and the impact of that contestation on the present may provide palpable evidence for the fieldworker to interpret the past.

Music conveys the contested nature of public space in various ways. The multilingual nature of a border region, for example, penetrates the texts of the song repertories that fill that region. This is evident in the historical traditions of Slovakia, particularly along the trade routes that follow the Carpathian Mountains north toward Poland, Ukraine, and Galicia (Wischenbart 1992). The songscape of this range is marked by the mixture of German, Slovak, Czech, Hungarian, and Yiddish texts, vying to penetrate the spaces each ethnic or religious group has sought out for itself. The contested musical spaces of culture may result from the denial of other spaces, in other words the performance of a space formed through music's presence in ritual. Pilgrimage practices, currently revived in the late twentieth century as an alternative cultural space, allow pilgrims in the Carpathians to perform a temporally ephemeral world whose boundaries remain only in the traces of musical texts. The past as a web of contested spaces offers a radically different history and historiography (see, e.g., Taussig 1987), and it is the historiographic potential that is most suggestive for the fieldworker studying the past.

By recognizing the contested nature of the past's spaces, the ethnomusicologist shifts her focus from musical product to process. The interpretation of public spaces in which women dance in South India, for example, has enabled ethnomusicologists to rethink the nature of Karnatak music history. The shift of emphasis by ethnomusicologists studying the Holocaust from repertories to the spaces in which Jews, Roma, and homosexuals attempted to survive, have revealed remarkably intensive music cultures, where musical performance became the very struggle to survive (see, especially, Flam 1992). To know the past through musical performance requires more than decoding whatever texts might survive. Before it is possible to hear the voices of those long silenced, it is necessary to embark on the journey of fieldwork to locate the spaces given meaning by those voices.

Past as Narrative Space. The contested spaces of the past are rarely absent from the narrative spaces inscribed by those whose writings connect past to present. In his focus on the conditions of imperialism and its modern decay, Edward Said trenchantly argued that the stories told in the past cannot escape the political connections to the world around them (1993:62–80, and passim). As a literary theorist Said interpreted narrative as literature, and he particularly concerned himself with the English novel, the genre that ascended in importance with the British Empire and narrated its spread, destruction, and dilemma of decay in modernity. I should like to suggest in this section that other forms of empire and

colonialism produce other narrative genres whose narrative spaces may open up the interpretation of the ethnomusicological past. In particular, I believe that the narrative capacity of music shapes quite different genres in which the spaces of the past are palpable.

The fieldwork I do in Burgenland has benefited from an awareness of quite different narrative genres chronicling the complex history of European Jewry. There is even a musical novel, whose title character is a young Jewish pianist in Eisenstadt, Franz Werfel's *Cella oder die Überwinder* (1982[1955]). I have never been able to escape the impulse to read this novel as a parable for the Jewish musical past. Though raised in the musical city of Haydn, Cella's family struggled to provide the talented young pianist sufficient opportunity to nurture her talent. As the *Anschluß* with Nazi Germany approached in the late 1930s, it became increasingly apparent that Cella would have to leave Austria, as would her narrator father, who largely took charge of the girl's music education. Werfel failed to finish the novel before his death, although its fragments narrate several attempts to leave Austria after being driven out of Burgenland by fascist Austrian functionaries. The final chapters follow the father to the Swiss border, which he was able to cross, but beyond which he narrated no more. The narrative spaces are so troubling precisely because the destruction of Burgenland and the silencing of its music are so clearly represented by the novel.

Werfel was not the only Jewish novelist to narrativize the spaces of Jewish Europe, spaces with only fluid boundaries; nor was he the only Jewish novelist to weave music as a narrative device into his novels. These techniques permeate the novels of Joseph Roth, to take one notable example, whose characters in novels such as *Die Flucht ohne Ende* (1978[1927]) and *Das falsche Gewicht* (1977[1937]) move across the Jewish landscapes of Galicia and other regions that no longer exist. Marking these landscapes are the narratives of musicians and musical performances, taverns where Jews and Roma sing together, small cities in Germany where Jewish conductors worship the Romantic ideals of Wagner, and Burgenland's Seven Holy Cities (Roth 2001).

Musical genres, too, evoke narrative spaces, which become means of hearing the voices of the ethnomusicological past. In Central Europe the ballad is the most obvious example of the creation of musical spaces that narrativize the past. The ballad "Die schöne Jüdin" ("The Beautiful Jewish Girl," DVldr. 158) is one such narrative of a young woman forced to cross beyond the boundaries of the Jewish community, to confront the other because "she was ready to dance" ("zum Tanz war sie bereit"; for versions of "Die schöne Jüdin" see Dittmar 1992). The metaphors of space and community are unequivocal in this ballad; the resolution of the encounter with Christian Europe, nonetheless, remains equivocal in its options: suicide, conversion, departure from Jewish tradition. The narrative spaces of ballads, novels, or other genres are not real in the literal sense, but it is because they evoke images of the social spaces of the Jewish past that they open up potential

connections to that past. Embedded in their narratives is a knowledge of self and community, a knowledge embodied by those who inhabited the spaces of the past. Today, sung by non-Jews, "Die schöne Jüdin," survives in the narrative spaces of Burgenland (see the second epigraph of this chapter).

The Past as Performance. Music lends meaning to the spaces of the past through performance, a physical meaning expressed by the physicality of performance. In the various approaches to the ethnomusicological past that I have sketched in this section, performance has been a fundamental, though not always emphasized, component of fieldwork itself. In this sense, I mean "fieldwork itself" as a means of encountering the past that is different from history as a practice of writing and writing about the past. Fieldwork is physical encounter; its historical capacity goes beyond the text to perceive meaning in the body.

To investigate the past as performance the ethnographer recognizes a mutually dependent relationship between the spaces in which musical performance took place and the physical, bodily acts that transformed those spaces. In the examples from my own fieldwork that punctuate this chapter the connection between space and body has been essential. My fieldwork in Burgenland has largely explored the spaces of music making: synagogues formerly filled with song and prayer, communities with complex mixtures of public spaces, shifting boundaries in border regions. No one can relocate the individuals who once performed within these spaces, but the memories of the past and the imagination of the everyday, informed by the musical texts and objects that were performed in the spaces, gradually bring the past into focus.

The interpretation of the past through performance relies on the premise that musicians perform in order actively to transform their bodies and the spaces they occupy. Historical ethnographers, particularly, have theorized that this active concern with the narrative potential of the body ultimately makes it an agent for the performance of history (Comaroff 1987; Comaroff and Comaroff 1992:215–33). When praying and singing in the synagogue, for example, worshipers clothe themselves differently according to the gender roles, hierarchical roles within ritual, and performative roles within the articulation of sacred texts and the remembrance of history. The interdependence of space, music, and ritual on the body is profound.

Musical performance in public spaces, too, acts aggressively to transform those spaces so as to act on history and consciousness.[13] Public performance of music, whether that of eighteenth-century ballad hawkers or twenty-first-century rappers, is one of the most powerful means of arresting attention. By necessity, the fieldworker enters into these spaces in the present in order to have her attention arrested by the transformations that performers work on the spaces as historically shifting fields. Each performance, each moment shaped by musical practice, draws the fieldworker into a complex of meanings embodied by the physical spaces linking present to past.

Entering the Field of the Past

Berger's world no longer existed. The farther we went, the
more we searched in vain for this vanished world.

Gerhard Roth, *Eine Reise in das Innere von Wien* (1991:57)

Excursus: Locating the Past, Finding Its Spaces. The synagogue of Kobersdorf,
one of the smallest of the Seven Holy Cities of Burgenland, stood empty in 1990
when I first entered it. Directly across the open space of the wide street sur-
rounding the small, but newly refurbished castle/fortress of the former Esterházy
residents of Kobersdorf, the synagogue was plainly visible to the tourists who came
to Kobersdorf during the summer to see the outdoor theater productions in the
courtyard of the castle. The summer productions were a new idea, welcomed and
eagerly supported by local boosterism, to bring added revenue and a bit of rec-
ognition to this small town of several thousand, only a dozen kilometers from the
newly permeable Hungarian border. Most of those attending the summer pro-
ductions did not know that an abandoned synagogue stood in Kobersdorf. Al-
though it fronted the broad street, the façade of the synagogue had received no care
or repair since it was spared from destruction by local superstition (see note 6).
There was no entrance from the street, traditionally no entrance because it was at
this end that the *bima*, the pulpit and location of the Torah scrolls, stood; facing
these, worshipers were turned toward the east, Jerusalem, but more immediately
toward the Esterházy castle and the Hungarian border.

With Manfred Fuchs, the mayor of Kobersdorf, but by profession a music
teacher and choral director, I fought my way through the tangle of weeds and
brush that guarded the synagogue's front door at the back of the building. Fuchs
had brought the keys, but, to his surprise, there was no need to use them; the door
was unlocked. Inside, the sanctuary was silent; not even roosting pigeons were
about to disturb the silence. I had entered a space outside of time, at least outside of
the fifty-two years since the building had last been occupied. The synagogue's
silence remembered those fifty-two years, a period during which, I learned later, no
one knew what to do with the synagogue.

Questions of ownership had not been answered; the Jewish *Kultusgemeinde* in
Vienna, the official administrative body for the Austrian Jewish community,
simply did not know what to do with the building, or with the other spaces of
Kobersdorf's Jewish community left silent by the Holocaust. Manfred Fuchs had
developed a concept for the synagogue, which would turn it into a concert space,
for use, especially, in the performance of Jewish music. We were witnessing at that
moment just how good the acoustics were, though the space was entirely devoid of
furniture and human occupants, who might otherwise inflect and distort the
acoustically live surfaces of plaster and wood. As a concert hall, and not least in

conjunction with the summer theater festival, the synagogue would serve as a means of remembering the past. The interest was there, and probably also the local investment from the mineral-water bottling company just outside of town, but too many questions lingered. The town had been told that the space was still sacred, and to use it as a concert hall would mean desacralizing the space. Only the Austrian Jewish community could do this, but communications from Vienna indicated that it was not exactly clear how one did such things. At the very least, a rabbinical official from Israel would have to come to Kobersdorf, and it was not entirely clear whether the synagogue should be stripped of its sacredness anyway. The synagogue stood, its space silent, not yielding its memories of the sounds that once filled it. It remained a space inhabited by the past.

The Jewish musical past in Kobersdorf resided in many of the town's spaces, and it was my growing sense of these spaces, which I explored through summer fieldwork in 1990 and 1991, that gradually allowed me to perceive something of the Jewish music that once filled them. The Jewish spaces of Kobersdorf formed during the eighteenth century and assumed more specific shape during the nineteenth century, when the town's Jewish population grew to almost 50 percent of the total. Kobersdorf was also one of the Burgenland villages that had a large population of Protestants, about half of all Christians living there, and the village was divided lengthwise on the two sides of the main street into Protestant and Catholic sectors. The Hungarian castle was the major political and cultural space of the town. There was one other political parsing of the space, namely the border between Hungary and Austria (Hungarian and Austrian parts of the Habsburg Empire), just outside of town until 1921, when the village and its environs were absorbed into Austria.

The diverse cultural, political, and religious spaces created remarkably complex musical spaces. That religious institutions embodied spaces in which different sacred musical practices were maintained goes without saying. What must be said, however, is that these spaces crossed and intersected with one another. Those with whom I spoke in the early 1990s remembered musical practices that took place in the village spaces and, particularly, in the movement between the spaces. The *Schulklopfer* (lit.: "school ringer," with "school" used in Yiddish as "synagogue" and in German as school) walked the streets of Kobersdorf, calling Jewish residents to prayer and mustering the children for religious instruction (see also Lang, Tobler, and Tschögl 2004:31–32).

Secular musical practices were also mobile and dependent on the fluid nature of Kobersdorf's spaces. On the same street that separated the synagogue from the castle and only a few buildings south of the synagogue was a building standing at the intersection of the road to the former border crossing and the main street flanked by Protestant and Catholic sectors. The building, in many ways, epitomized the spatial confluence of Burgenland's history. The building had been the Jewish tavern, a secular space necessary for the maintenance of Jewish identity in a

multicultural society. As a Jewish tavern, it provided kosher meals, overnight accommodations for travelers moving between the different parts of the empire, and a space for public music making.

Contemporary residents remembered all these functions, particularly because they had participated in them. It was in the tavern that they participated in the everyday Jewish musical life of Kobersdorf. They participated not as Jews, but as Kobersdorfers and Burgenlanders. The musical life embodied by this space, moreover, was not just Jewish, but Kobersdorfer and Burgenlander. Different dance bands played there, and indeed active exchange of musical repertory and style was facilitated by the public nature of the tavern's intersection with the village's fluid spaces. The celebrations following Jewish weddings, obviously, took place in this space, but Jewish musicians did not play at these; Roma bands from the area did. There was no irony in this, for Jewish bands played at Roma weddings. Jewish musicians also played at Christian celebrations and with Christian and Roma musicians. The most important and publicly visible ensemble in the village, moreover, was the Kobersdorfer Salonorchester, an ensemble that toured widely throughout western Hungary and eastern Austria. The Salonorchester was the pride of Kobersdorf, and its members were mostly Jewish, all Jewish in the memories of contemporary residents. As a musical ensemble the Salonorchester extended the public spaces of Kobersdorf to the many places it played. The mix of musicians and the complex cultural conditions of music making in the village were reconfigured at each performance.

The Jewish tavern, however, was silent in the early 1990s. It had been a private home for many years. Roma musicians were infrequent performers in the village. Local wind ensembles and choral groups performed in public, but their repertories were regional and national. The spaces in which Jews had made music, however, were still there, and it was these spaces that gradually provided me with a means of making the past legible and audible. The cemetery, virtually untouched, remained a space to enter; its entrance house, with materials used in caring for the bodies of the deceased left where they were, provided me with a space through which I could symbolically enter the past. The silence I first heard in the synagogue was everywhere in the village, but everywhere it was different. I entered the spaces of the Jewish past, and gradually the ways in which music filled them became evident to me. When they joined me in these spaces of the Jewish past, the Kobersdorfers remembered them again, and they returned to them—to dances, to weddings, to concerts of the Salonorchester—at least in their imaginations. Gradually, the boundaries between the spaces of the present and the past blurred, and fieldwork was bringing me into contact with the ethnomusicological past in vivid, unexpected ways. Occupying the spaces that had once been filled with Jewish music, the modern residents of Kobersdorf and I were drawn closer to a Jewish musical past that had been silent for half a century.

The New Europe and the People without Ethnomusicological Pasts

Even this idyllic scene, though, isn't innocent of history.

Eva Hoffman, *Exit into History*

This chapter is not a call to ethnomusicologists to become more historical. Since the advent of the New Musicology in the early 1990s, much has been made about rapprochement between ethnomusicology and historical musicology, with ethnomusicologists urged to arm themselves with historical tools. Even as the New Musicology waned in influence, ethnomusicologists did heed the call to historiography, and in so doing they have immensely enriched our field. The subject of this chapter, now as it was in 1997, remains fieldwork, and it is in the failure of historians to turn to fieldwork that we witness one of the reasons the rapprochement has faltered. Historical musicology, with shockingly few exceptions, still does not employ fieldwork, and its view of the past often reflects this all too painfully. It reflects this because of the human neglect its methods, now in the twenty-first century, uncritically accept. To state it bluntly by returning to the examples most common to this chapter, Jews and Jewish musicians have still failed to find a significant presence in the music history constructed by historical musicologists for Europe, which is their fundamental domain. Jewish music remains imprisoned by the historiography of pastness (with, however, several important recent exceptions, including John and Zimmermann 2004 and Frühauf 2005). The situation is not promising for the other peoples whose lives constitute the music history of Burgenland. As I write about my return to the Burgenland past in 2006, a year dedicated to endlessly celebrating the 250th anniversary of Mozart's birth, the Burgenland Roma, who have produced some of the most important musicians living in Europe today, remain largely silent in music history. The boundary regions of Europe, Old and New, lie outside the historical geography of Western music history, as if their everyday musical practices did not happen.

I want to conclude this chapter by focusing on Europe, in fact on the New Europe, which is, I want to suggest, an extraordinarily important domain for carrying out fieldwork in the past. Europe poses a special problem because of its historicization of itself, its obsession with a certain type of historical understanding of its identity. The modern trope of this identity is the nation, and that trope influences many modern forms of musical identity: folk song, national songs religious sectarianism, to name a few obvious cases. Much of the concern for identity in the New Europe derives from a long tradition of privileging sameness and scorning otherness. In particular, it is the tradition of privileging sameness that has made ethnomusicological fieldwork extremely difficult. Much folk-song fieldwork, for example, has been devoted to shoring up old boundaries (e.g., in

northern Italian Südtirol, where German, not Italian, songs are customarily collected).

Fieldwork has traditionally not addressed the fluid nature of boundaries, nor has it looked beyond cultural boundaries to the music cultures of those who are not contained by them. Europe, as a historical and cultural domain of musical practices, has been a place where fieldwork has been difficult because of the assumption of sameness, the assumption that there has been *a* history of Western art music, if not of Western music in general. The extensive sameness that we encounter in this overwhelming historiographic concern with self has encumbered the use of fieldwork to study Europe.

Modern musical scholars, however, remain encumbered at their own risk. The rise of the New Europe—the reconfiguration of culture and historical memory following the fall of communist state systems in Eastern Europe—has again foregrounded the tendency of nationalisms and ethnic groups to engage in selective processes of memory (Applebaum 1994; Ignatieff 1994). The past, at the present moment, is an increasingly competitive arena, in which one group's memory must validate its claims on land and history through destruction or erasure of another group's memory. New musical repertories and new musical practices have emerged, which nevertheless achieve validity through their putative connections to the past. Music again becomes a performative medium for making the contested spaces of past and present public. Musicians perform the histories of Europe on street corners throughout Europe, intensifying the narrative space that the past will occupy (Bohlman 1994).

The setting for fieldwork is there, on the street, in the public sphere that mixes old and new musical voices. There are additional European voices that will not be heard in the New Europe, especially the historical victims. The silence of Jewish communities in Burgenland is but one example. Roma, Sintis, and other groups of Gypsies are another case of racism that refuses to become invisible. More recently, Turkish residents of Germany have become another, though the historical connections to Europe's buttressing of itself against Muslims has not entirely been forgotten in the long history of Europe. Outside the course of Western art music's past, there exist and have long existed forms of identity embodied by the spaces of the cabaret or the social spheres in which women, gays and lesbians, or workers have made the music that voiced their own histories. History's victims have come to represent new boundaries of Europe (e.g., the Saami in the north) and within Europe (e.g., the reemergence of pilgrimage routes, performed by the estimated one hundred million Europeans who make pilgrimages each year). The spaces of the imperial past have neither disappeared nor become less serious in their contestation of Europe's present, witness Sarajevo's framing of the twentieth century (cf. Marcus 1993).

The historical problems of the European past, strikingly, have again emerged in the problematic struggles of the New Europe. The disjuncture between past and

present—the disjuncture that fieldwork necessarily confronts—has become more precarious and more compelling as a social space the ethnomusicologist must investigate. This disjuncture may efface and blur the boundaries between past and present, intensifying the immediacy of engaging in fieldwork in the ethnomusicological past and present. As Eva Hoffman observed in the epigraph that opens this concluding section, no place, however idyllic, is innocent of history. It is that absence of innocence that urges the ethnographer to look for new forms of history and to struggle to understand the reasons for the disjuncture between past and present. These are the troubling realities that characterize fieldwork. These are the conditions that connect musical practices in the present to those in the past, conditions ethnomusicology can address because it must.

Notes

1. During my fieldwork in Burgenland I have benefited enormously from the counsel from and experiences I shared with Gabriele Burian, Walter Burian, Manfred Fuchs, Roland Mahr, Franziska Pietsch, and Rudolf Pietsch. I should like to express my gratitude for that counsel and experience, as well as to the National Endowment for the Humanities (Summer Stipends) and the Alexander von Humboldt Foundation for their financial support of fieldwork in Burgenland during 1990 and 1991.

2. Burgenland still has a higher percentage of Hungarians, Croats, Roma, and Protestants than any other province in Austria. Croatian, for example, is an official language in many grade schools in middle and southern Burgenland, and Hungarian provides a special track for some high schools (*Gymnasien*) that emphasize modern languages. The historical presence of Roma is still evident in the many settlements that surround villages in Burgenland, whereas the presence of Protestants is announced by the extensive use of stone in church architecture, for example in the building of steeples. Ethnic diversity in Burgenland is official and fully present in the public sphere (Baumgartner, Müllner, and Münz 1989). For recorded examples of the diverse musical traditions of Burgenland see the first volume of *Tondokumente zur Volksmusik in Österreich* (*Burgenland* 1993).

3. The ballad "Die schöne Jüdin" ("The Beautiful Jewish Girl") remains in oral tradition. A recording of a version from Deutschkreuz from which the second epigraph for this article is transcribed is included on *Burgenland* (1993), which, though sung by non-Jews, survives in one of the Seven Holy Cities.

4. Johannes Fabian interpellates the many ways in which anthropology must remove cultures from time in order to construct the images of the Other that appear in ethnographies. By removing the Other from temporal frameworks that the anthropologist would represent as his or her own, it follows, the Other becomes understandable, framed as an object that can be pondered and studied; see Fabian 1983. Isolating images of the Other through ethnomusicological fieldwork remains the primary focus of the essays in *Deutsche Gesellschaft für Musik des Orients* 1981.

5. Burgenland was a region in which the Austrian and Hungarian parts of the Habsburg Monarchy, in effect, overlapped. Ruled primarily by the Hungarian throne, that is by the Esterházy family, Burgenland's courts served as a political and cultural transition to the Austrian seat of the empire in Vienna. We witness this clearly in the biography of

Joseph Haydn, who spent most of his life as a court composer in the service of the Esterházy family in Burgenland.

6. The story of this death is recalled as follows. In order to demonstrate the power of the National Socialist regime in Vienna, a decision was made in the early 1940s to demolish all the synagogues of the Seven Holy Cities in a single day, one after the other, with crowds of residents gathered to witness the disappearance of the last visible traces of Jewish culture. School children were marched to the sites of demolition, and when the sixth of the seven synagogues was blown up, a brick or stone struck a girl in the head, killing her immediately. The Burgenlanders saw this as a sign that the acts of demolition were wrong, if not the implementation of evil, and the seventh synagogue, which still stands in Kobersdorf, was spared.

7. Jewish cemeteries were never inside the walls of a European city, and therefore quite literally occupied a space beyond the boundaries. In one of the Seven Holy Cities, Kobersdorf, the Jewish cemetery was even on the other side of the international boundary with Hungary, and local residents remember how many mourners would stop at that border rather than accompanying the corpse beyond the Hungarian borderpost.

8. John Van Maanen (1988) compares the different forms the tales encountered and produced by fieldworkers assume, subsuming these under three large categories: realist tales, confessional tales, and impressionist tales. Issues of right and wrong, truth and falsehood, and understanding and misunderstanding shape all three tale types, thereby enriching and complicating modern ethnography.

9. The Ottoman occupation of Buda began in 1541, ending only in 1686, and during this period Burgenland, as the western part of the Pannonian basin of Hungary, became the final bulwark against an invasion of Vienna.

10. The earliest Roma settlements of Austria, those in Burgenland, first appeared in the sixteenth century and included Roma largely from the areas of present-day Hungary and Croatia. The history of Roma settlement in the area has historically been of such significance that Roma who descend from the early settlements are simply called *burgenländische Roma*, which distinguishes them from all other Roma, Sinti, and Lovara groups in Austria.

11. In Eisenstadt, summer home of the Esterházy court, as well as to the musical career of Franz Joseph Haydn, this is strikingly evident, for the Jewish community is much closer to the palace than the Christian sections of this small city, now the provincial capital of Burgenland (Klampfer 1966).

12. The vernacular of Jewish communities in these areas has been German, rather than Yiddish, since roughly the beginning of the nineteenth century.

13. Klaus Theweleit takes the body's presence as a historically situated vessel for performance as a metaphor for radically disjunct cross references that at once intensify historical consciousness and confuse it by transforming it into a field of restless signification. Musical performance transforms Theweleit's notion of the body into an unceasing proliferation of historical meaning (1988).

Theories Forged in the Crucible of Action

The Joys, Dangers, and Potentials of Advocacy and Fieldwork

Along with the fishhooks, fish line, ceramic beads, writing implements, "field" banjo and guitar, recording tapes, batteries, and memories of Claude Lévi-Strauss's *Tristes Tropiques* that my wife and I took with us to study the cosmology, social organization, and music of the Suyá Indians in Mato Grosso, Brazil, thirty-five years ago, I carried a conviction that knowledge should be used for practical purposes, as well as to produce more knowledge. This position was reinforced by my fieldwork experiences. I have spent large parts of my professional career doing a combination of what might be called "applied" and "theoretical" ethnomusicology. I believe the two strengthen one another. Combining them can also profoundly affect the nature of our field research and experience, sometimes for the better and sometimes for the worse.

I believe that research on the lives and music of people everywhere in the world can be important in itself, moving us toward a better understanding of both music and humanity. I disagree with the utilitarian extreme that *all* research must be of immediate use to a community and with the opposite extreme that human societies are objects we may observe without becoming involved in their aspirations. I think our theoretical work can be improved by applying our theories to practical issues and that our practical work can benefit from some of our theoretical activities. Much of this chapter describes the basis for these positions, rooted in my field research with a single group spanning more than thirty-five years.[1]

I am, of course, far from being the first ethnomusicologist to try to combine research and action in ethnomusicology. Daniel Sheehy, for example, has presented a nice history of applied ethnomusicology in an issue of the journal *Ethnomusicology* dedicated to the subject (Sheehy 1992), and is himself a leading example of an ethnomusicologist in the public sector. Applying musical knowledge

to influence people runs in my family also. My grandfather Charles Seeger worked as a musicologist outside of university departments for most of his life and my uncles Mike and Pete Seeger have used their performances to influence the musical (Mike) and political (Pete) values of their audiences. In 1939 Charles Seeger speculated on the ability of musicologists to guide large-scale government music projects, as well as their interest in doing so, and concluded "the answer must be negative. We have been too busy recovering our past to discover our present" (C. Seeger 1944:14). He did, however, think the involvement of scholars of music in what he called "applied musicology" was important for the field: "Without an applied musicology, the pure study [of music] must of necessity know less well where it stands, where it is going, where its weak links are, what motivation lies behind it, and what ends it serves" (C. Seeger 1944:17).

A strong movement toward increased public (or "applied") ethnomusicology[2] grew in the 1980s and continues in the early twenty-first century. The large Applied Ethnomusicology Section of the Society for Ethnomusicology (see SEM 2006) builds on the experiences of Jeff Titon (1992), Martha Ellen Davis (1992), and the influential work of Bess Lomax Hawes (1992).

Different disciplines place different weight on the combination of research and the immediately practical use of knowledge. Graduates of law and medical schools are supposed to apply their ideas every day. Anthropology in both the United States and the Great Britain has a long (if somewhat conflicted) history of involvement in applied work (see Pink 2006). The emphasis on public activities may also vary to some extent by geographic region and intellectual tradition. In Latin America, for example, scholars have long participated in political and social processes. They have pioneered many kinds of collaborative work (for example, Araújo 2006 on dialogic fieldwork). The application of knowledge may vary by circumstance, as well. During the terrible events that accompanied the dissolution of the former Yugoslavia, many ethnomusicologists became advocates for tolerance and justice over war and persecution (see, for example, Pettan 1998 for ethnomusicology; for anthropology see Schwandner-Sievers 2006). In the United States, by contrast, most university-based scholars have either chosen not to engage much in public ethnomusicology or have been relegated to the periphery through the marginalization of universities from the public sphere.[3] Whatever the cause of such disengagement, however, the radical distinction between theory and practice has been increasingly challenged in the social sciences and within ethnomusicology. More and more ethnomusicologists, for example, see their future in becoming public activists and intellectuals rather than in reproducing the somewhat restricted activities of their professors.

The examples that follow present some of the advantages and the difficulties of combining analytic field research with practical advocacy and public ethnomusicology projects. I begin with an example that demonstrates how a project con-

ceived by the members of the community gave me the experience and skills that were essential to me many years later, continue with an example that demonstrates the difficulty of engaging in projects before understanding the community, then describe how offering long-term benefits to communities of our field research requires ethnomusicologists to systematize and preserve their recordings and notes, and finally conclude with some recommendations for scholars on the basis of the experiences described.

Every research site and research project is different, and there are five important things to know about my own. First, my research was funded by the National Institutes of Health and this funding required only a dissertation in social anthropology. Second, I undertook my research in a Brazilian Indian "tribe" (meaning linguistically distinct community) known as the Suyá (they call themselves Kĩsêdjê). Their total population—all of the people in the world who speak their language—was under 100 individuals in 1971, most of whom lived in a single village (they now number over 350 and have three villages). Third, they had made peace with Brazilians only twelve years prior to our arrival, lived in a very isolated location on an Indian reservation called the Parque Indígena do Xingu, were essentially monolingual, and were not part of the market economy (they had no way to obtain or to spend money). Fourth, my wife, Judith Seeger, accompanied me on almost all my research trips over more than three decades and provided essential insights and balance to my presence but was not there to do research. Fifth, the Suyá were ethnographers in their own right. They watched the non-Indians who visited the Xingu very carefully and applied their knowledge in their relations with them. They were deeply interested in the vocal music of both Indian and non-Indian communities and "recorded" it by memorizing it and performing it. They quizzed members of other tribes on their lives and their ceremonies in much the same way a non-indigenous investigator would have done. They also thought their music deserved careful study and respect.

This information is relevant because many other chapters in this book deal with nation-states and individuals in dispersed populations numbering in the millions. Closest to my own research experiences are those described by Beaudry and Kisliuk. Many of the examples I describe are important only in the context of the size, isolation, and lack of experience with Brazilian society characteristic of the Suyá in the 1970s and early 1980s, although I believe the lessons to be learned from them have some general application. Due to their small number and residence in a single village during most of my research, I saw virtually every adult man every day and I could watch the public activities of the entire community from the shade of the men's house in the center of the village plaza where I often wrote in my field journal. These things are impossible in larger communities. Even though many things changed dramatically in the 1990s and twenty-first century, the Suyá remain a relatively small and isolated community in Brazil.

The Enduring Value of a Recording Project

One of the reasons ethnomusicologists talk so much about fieldwork is it is often profoundly exciting. One of the characteristics of field research is that you rarely know what's going to happen next. A new idea, or incredible tedium, may lie just seconds beyond the present. Even after years of research, I can be surprised by something I don't know, or by a turn of events I don't expect.

One evening in 1976 was a good example of such a moment. My wife and I were making our first visit to the Suyá since 1973. The stars of the Southern Hemisphere shone brilliantly overhead as, for at least the 300th time, I sat with the men in the center of the village plaza for the evening gathering. I sat quietly, my presence acknowledged but unimportant for the flow of humorous anecdotes and conversation about hunting, fishing, neighboring Indian groups, and the activities of Brazilians in the region. I sat in the dark listening companionably for three reasons: First, we had no source of light other than eight candles and a flashlight we brought for emergencies and there was nothing else I could do in the dark except sit with the women (not considered very appropriate by either gender) or retire to my hammock (I rather envied those ethnomusicologists who seemed to have boundless energy, furniture, and light enabling them to work at night). Second, I learned a lot listening to men talk among themselves about things that I would never have thought to ask or did not know how to ask—including the terms for musical structures. Third, I must confess I loved the sight of the stars overhead in the crystal clear sky and that usually low-key way of ending tiring days, even though I was uncomfortably perched on a low hard stool and sometimes nodded off to sleep. That particular night my attention was recalled suddenly from the stars to the relative silence around me and I could tell that whatever was going to be said next had been planned in advance.

"Tony, why haven't you made an LP record of our music? Roberto Carlos [a Brazilian musician very popular at the time] makes lots of money from his records. We would like to have lots of money because we would like to have lots of trade goods."

Gathering my wits about me, I responded lamely, "I didn't know you wanted me to." In fact, I didn't even realize they knew what an LP disc was. Although a few Suyá had transistor radios, I had seen only one battery-operated record player in the entire reservation; there were none in the village.

"Yes, we would like one. We would like to have one and get *lots* of money and *lots* of trade goods."

Ethnomusicologists are often asked for such assistance. We typically use expensive audiovisual equipment, frequently come from fairly wealthy social classes, and sometimes come from other countries renowned for their wealth. The people we work with thus often assume that we know all about making recordings, scheduling concert tours, and giving good advice to performers on how to achieve

their musical ambitions. The truth is that most of us know little or nothing about the record business, have never organized a concert tour in our lives, and haven't given much thought to what might be the best career choices for musicians trying to break out of the mold of the traditions we are interested in studying. We lack the specialized knowledge to be very helpful (Seeger 2006). The Suyá request mirrored those of many other musicians in their unrealistic expectations (Roberto Carlos makes money selling records; therefore, we will make money selling records) and also in their earnestness and sincerity. This was clearly something they had talked about and decided upon.

I believe the key to responding to such requests is to turn them into opportunities for research and experience that will benefit all parties. Ethnomusicologists can learn more about the music they study, and the members of the group or community can learn how to manage their own affairs in ways they might not otherwise have done. Sometimes these experiences can be unexpectedly important lessons for life. Here is how I proceeded:

Since the recording was their idea, I decided to involve them in the project as partners. Specifically, I thought they should play a decisive role in determining what appeared on the recording, should have a say in the content of the liner notes that would accompany it, and should receive any financial benefits that might accrue from it. The reasons were obvious: they knew more about their music than I did and they wanted their music accurately presented. I also decided that I, rather than they, should create the track sequence and write most of the liner notes. The reasons for this also seemed obvious: I had been playing and collecting recordings since I was a child; I knew more about Brazilian society (the target audience) than they did; none of the Suyá knew how to write; and I had some objectives of my own that I thought should be communicated through the sounds to address misunderstandings of Brazilians about indigenous music. I also thought the process, as much as the product, could be important for both the Suyá and for me. While such collaboration seems obvious today, it was very unusual for ethnographic recordings at the time. In that era, most ethnomusicologists made recordings while doing their field research and then selected the material, wrote the liner notes, and produced the recordings on their own after they returned home. The performers were not consulted and sometimes did not even receive a copy of the recording.

I began the project a few days later by asking the Suyá individually and collectively what they did *not* want to have on the recording. They thought their enemies would raid them if they heard certain songs. Some invocations caused harm rather than good, so they decided it would be better to include an invocation to cure a toothache than one to make a woman infertile. Their theories about music and its efficacy thus influenced the content of the recording. Once the universe of the possible was defined (the possible being everything on the seventy or so hours of tape I had recorded minus the material they did not think appropriate), I began to work on a selection and a sequence. First, I decided the

recording should include examples of speech, song, myth, and curing invocation because these genres were all interrelated. All Suyá music (called *ngere*) was song/dance, specific combinations of words, melodies, and movements. Words were also delivered in a variety of rhythmic and tonal patterns in everyday speech, oratory, instructions, and other genres. Thus *my* theories about Suyá music and speech (presented in a scholarly format in Seeger 2004:25–51) influenced the contents of the recording as well. Through the sequence I was acting as what Richard Kurin has described as a "culture broker" (Kurin 1997), serving as a mediator between the Suyá and the projected audience based on my imperfect understanding of both and my desire to be faithful to the former and intelligible to the latter.

It is hard to imagine how difficult communication was only twenty-five years ago. It could take months to get a letter to the Suyá; if it arrived, none of them could read it. I could send a tape, but there was no guarantee anyone could play it because their few audio cassette tape recorders broke frequently in the 1970s and batteries were often unobtainable. It took weeks or months of preparation to get to the field and sometimes weeks to leave it, due to the shortage of motorboats traveling on the Xingu River and delays in the once-a-week supply flights of the Brazilian Air Force that were the only way into or out of the region. It was thus a year or two later that I played my proposed sequence for the Suyá. They listened to it and thought it was fine. Together we worked on the song translations and they clarified some of my questions about the different tracks. We were making progress, but how could I get it published?

Like many things, the problem was solved through a friendship made by happenstance. A new colleague asked me if I would like to publish a recording of Suyá music on his friend's independent record label, called "Tacape" (which is the Portuguese word for an indigenous wooden club). We created the pre-master in his apartment and went to a commercial facility in Rio de Janeiro to make the master from which the recording would be pressed.[4]

This left me with the cover photo to select and the liner notes to complete. I had chosen a nice reflexive photograph, showing the Suyá singing one of the songs on the recording while my wife recorded them on our reel-to-reel tape recorder. The photograph indicated that these were Indians singing, of course, but also that they were being recorded by identifiable actors using a specific technology. The producer accepted the photograph but cropped my wife and the tape recorder out of it, saying that my concept would be too confusing. Since my wife had objected to the way she looked in the photograph, it seemed wise not to make a large fuss about the crop. I did, however, feel that commerce had interfered with my theory-informed concept for the cover. The label owner and I also haggled over the title of the album, but the result was greatly improved by the discussion. The LP was called *Indian Music: Suyá Vocal Art* (*Música Indígena: A arte vocal dos Suyá*, A. Seeger e a Comunidade Indígena Suyá 1982). The label owner granted me a fairly

generous word limit for the length of the liner notes and I was able to include a number of photographs of my choice. What I wasn't told was that the typeface would be very small and that the notes would be printed on only one side of a large sheet of paper, to save expense (the font could have been much larger if they had been printed on both sides of the sheet).

The financial arrangement was that I would receive 10 percent of the copies that were pressed, the equivalent of 10 percent artist royalties paid at the time of manufacture. We also did our best to protect the rights of the Suyá. Eventually 100 copies of the LP arrived at my office in Rio de Janeiro and I had to figure out what to do with them. I felt the Suyá deserved all the financial benefit to be derived from their performances. I raided my savings to pay the Suyá their share immediately. When a Suyá leader was in São Paulo for medical treatment, I sent the equivalent of US$1,000.00 to a friend, who took him shopping for trade goods for the entire community.[5] Then I began to recover the advance by selling the LPs one at a time for the equivalent of US$10 each. Trying to do the "right thing" can be costly and time consuming. I held a publication party at the annual meeting of the Brazilian Anthropological Association; I sold them at the Society for Ethnomusicology; and I gave them to colleagues. I lost money, but at least the Suyá had received all the royalties for their recording. I thought this was an essential part of the recording process, since I received a salary for my work. It was with somewhat mixed emotions, however, that I learned the recording had sold out and the record company had manufactured another run and sent me 10 percent of that one, too.

The Suyá were delighted with the cassette copies I sent them, since they had no record player. They continue to enjoy CD-R copies of it today. *A arte vocal dos Suyá* was apparently the first full-length LP recording devoted to the music of a single Indian group published in Brazil. It was followed later by many others. The production established some excellent precedents, both for me and for the Suyá. Ever since then they have expected payment for use of their cultural heritage. They tell people who want to photograph or record them that "their anthropologist" pays them for their published recordings.[6] They insist on receiving some payment for their participation in any kind of commercial venture, whether nonprofit or not. In the context of the generalized exploitation of traditional cultures around the world, their explicit position is an important one.

The project was also fun—it was a joy to work with the Suyá on a project they really cared about that simultaneously developed their skills and mine. It was exciting to meet new challenges and (in most cases) to overcome them. I received very flattering feedback from both the Suyá and many Brazilian anthropologists and the general public on the recording itself. I also received invitations from two other Indian groups to produce LPs of their music, which I was unfortunately unable to do because I had taken a job at Indiana University in 1982. Collaborative projects that benefit the community in which one does research can be rewarding in many ways.

My experience producing *A arte vocal dos Suyá* would not have been particularly significant to anyone besides the Suyá, although I learned a lot doing it, had I not later become director of Smithsonian Folkways Recordings. The surprise request made in 1976 became the basis of my professional activities between 1988 and 2000. My experience with collaborative production, my convictions regarding who should benefit from recordings, and my experiences dealing with the Brazilian record company on cover art and liner notes shaped my approach to reissuing existing Folkways recordings on CD and creating new releases on Smithsonian Folkways Recordings. Responding to the Suyá desire that we release a recording of their music created a model I later used for directing the label and producing hundreds of other recordings. I had produced only one recording when I was hired to be the first curator and director of Folkways records at the Smithsonian Institution—*A arte vocal dos Suyá.* But I had worked out quite a few important issues in the process. Clearly, researchers can learn useful lessons for life working with members of a community to achieve the community's aims.

There is an important movement in anthropology and ethnomusicology toward collaborative models in which research is combined with advocacy (see Lassiter 2005). The El Dorado Task Force of the American Anthropological Association "insist[s] that the anthropology of indigenous peoples and related communities must move toward 'collaborative' models, in which anthropological research is not merely combined with advocacy but inherently advocative in that research is, from its outset, aimed at material, symbolic, and political benefits for the research population, as its members have helped to define these" (Lassiter 2005:ix). While not all researchers would agree that all research must be inherently advocative, many of us recognize the importance of collaboration in the objectives, methods, and end products of research.[7]

Some ethnomusicologists are only able to do their research if they are part of an applied project of some kind. But the easiest kind of project to support is of the kind I engaged in with the Suyá: one that is presented by the members of the community as their own. All researchers probably discover that there are moments in which they feel they must intervene, but such actions should be taken with great care, self-awareness, and posterior analysis.

The Possible Importance of Not Acting as an Agent for Public Projects during Field Research

Engagement in public ethnomusicology projects during fieldwork has its own perils. Sometimes it is important for researchers *not* to act and instead to refrain from interfering in the life of a community. One rainy evening, when my wife and I were swinging slowly in our hammocks in the darkening interior of the large, one-room thatched-roof house in which we lived with some thirty Suyá, we were given another message. One of the adult women who lived in the house came over

and sat down next to our hammocks (a rare occurrence, since the area of a family's hammocks was private space) and quietly addressed us. "We like you." she said. "Do you know why we like you? We like you because you never tell us what to do. You never tell us we have done something wrong."

This was a message as clear as that of the question about why I hadn't produced an LP of their music. It was clearly prearranged, carefully thought out, and in this case delivered by a woman. I was puzzled by it at first. Then I realized that most Brazilians and other non-Indians the Suyá had met wanted them to do something. Reservation administrators wanted them to move their village to be closer to medical aid and simultaneously to liberate their former territories for occupation by ranches (Seeger, forthcoming). Doctors wanted them to take medicines and subject themselves to shots and tests. Other Brazilians wanted to teach them things, and did not hesitate to express their opinions about Suyá activities. They expressed disgust at some of their body ornaments, condemned some of their practices, and waxed enthusiastic over others. NGOs wanted them to meet their project deadlines and to act "responsibly."

Colonized peoples and other so-called subaltern populations live in an environment in which others insist on telling them what to do, rarely take the time to understand what they are already doing, and show little respect for their way of life. It is very easy for researchers to reproduce that pattern even while professing to respect the local traditions. If researchers' purpose is to understand life in its full complexity, it is important to be careful about considering their own ethics before those of the community in which they are living. If one doesn't believe that death is caused by witches, it is very easy to say witches shouldn't be killed. The United States has a death penalty for murder, however. The exact moment at which an embryo becomes a human being is so heatedly debated in U.S. society that it seemed absurd to query the Suyá's own definition of when life begins. I would never have suggested or supported the raids the Suyá launched on the surrounding ranches because I felt they were much too dangerous for them; but their raids eventually resulted in the recovery of their former territory and the expulsion of the ranchers. In placing their own ethical position above that of the people they are working with, researchers not only reproduce the already frequent suppression of the people's self-determination but also may fail to discover certain aspects of the people's lives.

While our presence certainly influenced some aspects of Suyá life, I consciously intervened in two cases where I thought a death should be prevented. The first was when I thought an old man would die without professional medical care but no one was helping him because he was a suspected "witch" (*wayanga*) who caused deaths. The second time was when a Suyá angrily announced he wanted to kill a visiting British anthropologist who had told him to put something in one place rather than another. He announced, "around here, *we* say where things go, not anyone else." In the first case, I put the old man into a canoe and paddled off

alone down the river toward medical aid; in the second case, I suggested that maybe killing would be excessive punishment for what I suggested was ignorance. Although I sometimes replay those moments in my mind even decades later, I have no regrets for having saved the old man's life (for a while at least; several years later he was clubbed to death by members of another tribe who thought he was responsible for deaths in their group) or for possibly saving a colleague's life by suggesting that clubbing was an excessive response for his perceived offense. I was convinced that my role with the Suyá was not to tell them what to do and it was made clear to us that this stance was appreciated.

Working with and around Other Actors and Institutions during Research

Ethnomusicologists' descriptions of fieldwork all too often fail to mention other actors and agencies working in the same community. This may be traced to the "heroic myth" of research that one works alone (or with a small team) within a community, or it may be done to keep the narrative simpler. The implied "us" and "them" in many ethnographic descriptions fails to acknowledge the activities of other agents and agencies working in the field at the same time.[8] The non-Indian agents working in many Brazilian Indian communities, for example, include other researchers, government bureaucrats, Non-Governmental Organizations (NGOs), Christian missionaries, journalists, (in a few cases) tourists, and members of surrounding non-indigenous communities.

The stereotypical description of the different roles is that the researchers come to learn from the community and then return to their homes and institutions never to reappear; the government bureaucrats come to implement government policies and often stay long periods of time; the NGOs want to implement specific projects they believe will benefit the community; the missionaries come to teach what they perceive to be the truth and make converts; journalists come for stories of interest to their readers; tourists come to learn and experience but leave with little knowledge or experience; members of surrounding non-indigenous communities want to live as comfortably as they can in a capitalist economic system by taking the Indians' land and using their labor.

There are complex relationships among these actors as well. Government bureaucrats distrust researchers and journalists because their reports can be critical of their administration and damaging to their careers. Workers with NGOs often mistrust researchers because they only care about their research projects, government agents because they haven't done enough for the communities, and tourists because they aren't serious. On the other hand, NGOs are usually happy with journalists who publicize their work. Missionaries often mistrust researchers because they establish close relationships with the community but may not share the missionaries' religious beliefs. Members of surrounding communities often

desire the land resources granted to the indigenous peoples and envy them for the money and attention they receive from the government, from NGOs, and from journalists. Researchers are often critical of all the other agents and contrast their deep "ethnographic understanding" with the shallow understanding and objectives of the others. At times these attitudes escalate into active hostility. Then the different agents mobilize their support within their communities and within their larger organizations in an attempt to oust one or more of the others.[9]

In fact, many government bureaucrats try hard to benefit the communities under their responsibility, although they often lack the resources and flexibility of the NGOs. Missionaries of different denominations act quite differently. Journalists and tourists can be beneficial to the other actors and to members of the communities. And researchers may combine their research with activities that directly benefit the communities in which they do their field research—often in collaboration with government agencies or NGOs. In some impressive cases, the agents have been able to work together. This requires interpersonal skills and a careful evaluation of the implications of such collaborations.

Today, a number of well-intentioned and very competent Brazilian NGOs operate in the Suyá area.[10] Some of them have been financed by musicians—for example, Sting, whose donations paid for training and implementing the bilingual education that enabled most of the younger Suyá (as well as other Indian communities in the Xingu area) to learn some Portuguese, understand money, and write in their own language. Other programs trained Suyá as medical technicians. A few NGOs have tried to involve the Suyá in community development projects that would generate income and give them a greater degree of economic autonomy. Some of these projects have seemed to me to be fairly ill-advised and others to be potentially useful. The attempts of the NGO project managers to convince the Suyá to participate has given me a new insight into the difficulties of trying to do one's own research and applied work at the same time.

When people have a project they think will benefit the community but which requires its active participation to do so, they often depend on members of the group who support the project. Those who do not support it are easily perceived as adversaries and their concerns and demands dismissed as excessive. It is very easy for unwitting project coordinators to exacerbate existing factional politics without meaning to do so. Desire for the success of the project in which one is engaged inevitably shapes a person's view of the community and events in it.

Listening to the swirling discussion in the men's gathering at night I learned to see things in a much more complex way. Here are two examples of their objections:

"The NGO wants to give us a truck, but they want to give us a *used* truck! Why should Indians always get used equipment? Why do the NGO members have new trucks and just give us the old ones? I'm not going to accept it. I'm only going to accept a new truck." I might have intervened here with observations on budget,

funding restrictions, and spending rubrics, but I could also see the logic from the Suyá perspective (seeing things from "the native's point of view" is, after all, what we specialize in). Why should they take an old truck that would require more maintenance and reiterate their status as dependents? Eventually, they got a new truck.

"The NGO wants us to plant fruit trees along the border between our territory and that of the Brazilians, but they won't give us any money or trade goods for doing so. We all work hard to cut and plant our own gardens; how could we live in the forest for weeks in order to cut down the long clearing and plant fruit trees in it without some kind of recompense? Why shouldn't the Brazilians do it for us?" I might have intervened here with observations on the usefulness of a clear indication of the territorial divide between their land and that of the ranches and soybean farms around them, but I had spent a lot of time in the forest and knew just how much work it would be. I also wasn't sure the fruit trees would grow there. The planting was never done, and new GPS equipment renders such a defining barrier less important.

The Brazilian government and some project coordinators have occasionally asked my help to convince the Suyá of the importance of doing one thing or another. Even when I thought it was an excellent idea, I was not very good at it. I am much too humble to think I can predict the outcome of a project. The Suyá have also defined a specific role for me. They don't want me to advocate for one activity or another; they want me to work for them once they have decided what to do.

Of course, researchers have their own agendas and can be just as blinded by their need to complete their research plans as NGO project managers are by their need to successfully complete their projects. As this volume demonstrates, fieldwork is also a value-laden activity. Sometimes the only way to undertake ethnomusicological research in a region is through affiliation with NGOs or community-directed projects. There is nothing inherently wrong with this—indeed, it can be instructive and rewarding. But researchers in this position might want to separate their research from their project-related activities. There are several dangers to good research from combining a specific project with research. One of these, which I encountered in a Ph.D. dissertation of a student of mine in the 1970s, is that because she supported a certain kind of political action within the community, the student omitted some research data. Her analysis of the political processes was thus incomplete and inadequate. If advocacy requires the premeditated presentation of incomplete data and partial analysis without a discussion of that omission, it should not be presented as research. This is as true of ethnomusicological research as it is of research for pharmaceutical companies that does not acknowledge possible conflicts of interest. Another danger is that ethnomusicologists involved in public ethnomusicology projects will feel uncomfortable reporting their failures (on the research side, it is easy to change the focus of one's research to another topic and thus failure is very rare indeed). Yet admitting the failure of a project

might endanger their future employment and their relations with the members of the community. How are we ever going to learn about applying our skills in ethnomusicology without understanding failures as well as successes?

The Unexpected Long-Term Benefits of Field Research on Space and Time

One stereotype of researchers I have encountered in Brazil is that our research is narrowly focused on issues of little significance to the current lives of the communities we study. It is important to reject the simple dichotomy between "pure" (useless) and "applied" (useful) work. They can be closely intertwined. What begins as research without a practical objective for the community (as distinct from the practical objective of completing a dissertation) may produce results that become important in practical ways years later. On the other hand, what begins as a practical project may, if it is documented and reflected upon, inform theoretical understanding in the future.

Sometimes the results of field research have a profound impact on the lives of the communities or individuals we study for reasons unimaginable at the time we undertake the research. Even when our academic theories have long since been discredited, the data themselves may be used in new ways. For example, some North American Indian communities carefully consult the publications of the Bureau of Indian Affairs for their detailed descriptions of material culture (Jabbour 1983:2), and some Australian indigenous societies have been able to use audio recordings of their songs to support their claims to their ancestral lands (Koch 1995). The recovery of early twentieth-century recordings of German researchers in what is now Papua New Guinea stimulated cultural revivals and self-respect in locations where they were made (Niles 2004). It is true that not all research publications have proven useful to the communities researched, but detailed descriptive data and recordings have been appreciated by many communities. A good example of this kind of usefulness was my ability to use data I had collected for purely academic motives in the 1970s to prepare a document that demonstrated the Suyá's right to their former territories twenty years later.

When I traveled with the Suyá by canoe in the early 1970s, they insisted that I write down the information they told me about the places we paddled by or visited on long hunting and fishing trips. Almost every curve of the river had a name commemorating some past event. Former village sites, located on large bends in the river and surrounded by fruit trees planted decades before, recalled other experiences. Their ancestors, buried in the floors of their houses at death, were remembered even when nothing remained of the houses themselves. Canoe travel is slow, especially paddling upriver hugging the riverbank, and the trips gave me ample time to learn the history recorded in the landscape (Seeger 1977). Taking notes on what they told me also provided a brief respite from the arduous paddling.

Upon our return, the men would often quiz me on the evening after the trip: "Why was the lake where we stopped and cooked fish called 'place where the moon died?'" "What was the name of the place our ancestors obtained clay for making pots?" Because it was so obviously important to them, I made careful notes.

Among the places we visited were former village sites, located two days' paddle outside their reservation. I wrote down the names of the plants found there that did not grow where they currently lived. Standing in the overgrown plazas, I noted the names of people who died and had been buried in different house sites. This information, only a little of which I had used in my dissertation and other publications, was extremely useful in defending the Suyá leaders against charges of criminal trespass after they had occupied the ranches in their former territory and expelled all but a few Brazilians, four of whom they took hostage for a few days. The Suyá verbal claim to the land did not carry much weight in court. Written documentation by a professional researcher was deemed admissible, especially when corroborated by Brazilian specialists appointed to investigate the claims. Since they had never stopped using the land, and since they had a constitutional right to their ancestral territory (what could be more ancestral than ancestral graveyards?), the Suyá were not only absolved of criminal trespass but the land was returned to them by the Brazilian government.

My wife and I visited the Suyá again in 2003, thirty-two years after our first encounter, to celebrate their construction of a new main village on their recovered territory. This village stands on the site of an earlier village, where some of the elders were living in 1959 when the Suyá made peaceful contact with Brazilians. A few kilometers away stood the house, corrals, and airstrip of a ranch that had deforested most of area and left it largely unsuited to Suyá horticulture; a few cattle ran wild in the untended pastures. We found the Suyá triumphantly occupying the largest village they have made in a century. They were healthier, more numerous, and more optimistic about the future than they had been at any time since our first visit in 1971. They were performing one of my favorite short ceremonies, the Bee Ceremony, whose songs I find particularly melodic and moving.

New Uses of Old Recordings and Notes—If They Survive to Be Useful

Not all was perfect, however. Some of the middle generation—men and women who had been born after the Suyá made peace with the Brazilians and were now parents of growing children—expressed concern that they were forgetting things their parents had known. They wanted to start a kind of culture center in their new village and asked for our assistance. In 2005, the Indiana University Archives of Traditional Music produced digital transfers of all my audiotapes, students of mine translated the documentation,[11] and we shipped them down to the Suyá. They

were received with great excitement by all, and copies are currently circulating among the four Suyá villages.

I have also helped to provide the Suyá a new anthropologist with whom they enjoy working and whom they trust to support their projects as I have done. In September 2004, I brought five Suyá and a young Brazilian recent Ph.D., Marcela Coelho de Souza, to perform at the opening of the new National Museum of the American Indian in Washington D.C. I had contracted Marcela to assist with translation and escorting the Suyá around Washington. I learned a lot about the challenges of arranging tours that month, another frequent activity for ethnomusicologists, and definitely needed her help. Marcela proved to be a very able assistant, and the Suyá men on the trip—including the three chiefs—decided they liked and trusted her. She became the first anthropologist the Suyá have allowed to visit them since my wife and I started working with them in 1971. I am sending her copies of my field notes, and she has made copies of the recordings I have sent. I hope, through this, to continue a tradition of research and activism long after I am unable to continue to do so.

This kind of return will only be possible if ethnomusicologists and other researchers are proactive with respect to their recordings and unpublished writings. Audiovisual recordings must be archived, preserved, and migrated to new media formats or they will not be available in the future; our unpublished works must be saved for appropriate access. From the start I justified my recording of Suyá traditions as providing them for their descendents. I told them I would store my recordings in a safe place for their grandchildren to use (although I did not really expect them to travel to the Archives of Traditional Music in Bloomington, Indiana, to do so). The Suyá neither needed nor could use my recordings until recently, but they were safely stored for them in Indiana for when they wanted them. Now, with solar power, rechargeable batteries, and computers, the materials are welcome and easy to access. Rapid social and cultural change and new technology speeded up the process so that my recordings are playable in the village by their children.

Preservation of audio and video recordings should be of major concern to ethnomusicologists, and ensuring their survival and future usability is an important kind of public ethnomusicology. Even when community members make their own recordings, there is little likelihood these will be playable fifteen, thirty, or fifty years from now. Not only do the media (tapes, discs, hard drives) disintegrate, the machines that play them become obsolete. Even when commercial recordings have appeared of the community's music, there is no guarantee that companies will take care of the masters (the history of audiovisual preservation by the major commercial labels has not been a good one even in the United States). In addition to archiving our own recordings, it might be a good idea to preserve some of the community's recordings of themselves. If ethnomusicologists don't think about

the future use of the recordings they make and collect, only the most commercially successful recordings will survive—and they represent only a tiny fraction of any community's musical traditions and performers. This is as true of the Suyá as of any genre of popular music in the world today.

Conclusions

This chapter has followed the sequence of public ethnomusicology projects in which I have participated as a research scholar—from a co-produced LP record in the 1970s through a successful land claim in the 1990s to the support of a Suyá culture center in the twenty-first century. Here are some general reflections:

1. The dichotomy of "theoretical" and "public" or "applied" ethnomusicology is false. The most abstract research can have practical benefits and the most practical projects can stimulate abstract thinking.

2. The most rewarding public projects for ethnomusicologists will often come from the desires of the community members themselves. The process of doing a collaborative project is instructive and the results often exciting. Even when the community wants assistance in something that has little to do with music, the collaboration can be valuable in itself.

3. Working for an outside NGO or government entity can provide essential legitimacy, important insights, and necessary financial support, but it is important to separate the objectives of the research project from the objectives of the action project in some way. This can be done by working in an HIV/AIDS education program, for example, while studying musical theatre. If the object of study is the NGO project itself, then it is essential to consider possible conflicts of interest in advance (Van Buren 2006).

4. When working on a public project, it is very important to try to understand factional politics and the relationships among the different agents and agencies in the community in which one plans to introduce it. It is probably also better to understate expected results than to overstate them, to be very clear about the various steps and the difficulties that might occur at each of them, and to keep a good record of the process.

5. Public ethnomusicology projects should be treated as a kind of field research. In spite of the inevitable fatigue and lack of time, ethnomusicologists doing public work should keep field journals of the project and document it as carefully as if it were their dissertation research. Writing, for many of us, is at once a form of reflection and a snapshot of our understanding at a given moment (see Barz, this volume). Such snapshots are needed if the process is to be analyzed.

6. The benefits to both researchers and the members of the communities they study can be enduring. Practical benefits to the profession and to the long-term success of the community are also possible. There are joys and potentials in collaboration, even as there are dangers to be avoided.

It is a long time since I have taken fishhooks and fish line to the Suyá (they prefer to buy their own now), but after thirty-five years of intermittent research and collaboration with them I am still convinced of the importance of using the results of our research in places far beyond university walls for the benefit of the communities whose music we study. I also believe that in so doing we will improve the field of ethnomusicology itself and increase its impact on the future of both music and community life.

Notes

1. My thanks to my wife, Judith Seeger, for her contributions to this chapter and for her careful proofreading.

My convictions about the integration of thought and action were part of my up-bringing and later of my academic training. They were also shaped by many experiences not part of my fieldwork. On the other hand, field research did provide some very important formative experiences. My dissertation research was done between 1971 and 1973, about 15 months of which my wife and I lived in the Suyá village. I was employed in Brazil as an associate professor of anthropology at the Museu Nacional in Rio de Janeiro from 1975 to 1982; during these years my wife and I visited the Suyá every year or two on shorter trips (a total of about nine months). Responding to requests from the Suyá, we have returned five times between 1994 and 2007 for visits of between two and six weeks.

2. Terminology is a problem in this area. Many university-based ethnomusicologists reject the term "applied ethnomusicology" because feel they "apply" their ethnomusicology in very practical ways every day in their teaching and writing. "Public Sector" ethnomusicology is limited to government-supported work. Following sociology and some other fields, I use the term "public ethnomusicology" to refer to ethnomusicological activities undertaken primarily outside of universities and directed toward the public. But, bowing to convention, I also use the term "applied ethnomusicology" in this chapter.

3. Some of the dichotomy between "theory" and "practice" may be due to a centuries-old distinction between the liberal arts and the practical arts. The liberal arts are meant for reflection, not necessarily for action, while the practical arts are more like crafts. This is perhaps why the "professional schools" where doctors, lawyers, dentists, nurses, and business leaders are trained are more active in the public arena. Scholars in these fields are expected and encouraged to participate in local, regional, and national debates, as well as in practical action. On the other hand, there are both internal and external forces on public scholars who seek to address issues beyond the academy.

4. My thanks to José Maria Neves and Conrado Silva. Tacape has since gone out of business and the rights to the recording returned to the Suyá. I have not released it on CD because it is now very short (45 minutes) and adding an additional 30 minutes would be a major project. The recording is currently out of print, though many of the genres featured there also appear on the audio CD accompanying *Why Suyá Sing* (Seeger 2004) and on the DVD nearly finished that will accompany future printings and translations.

5. At the time, no Suyá possessed, could understand, or could manage money. Their own numbering system was based on a count of "1, 2, 3,4, many," and the few Suyá who

pronounced large numbers in Portuguese usually spoke them without any concept of what they meant. A very high inflation rate and changing currency also made Brazilian money much harder to understand than dollars have been in the U.S. Today they have mastered all these things and have a community non-profit association (Associação Indígena Kĩsêdjê) with a bank account to which I can send funds directly.

6. The equity of the relationship can be debated endlessly, but I have made an effort to ensure the Suyá benefit from my work with them. I have consistently sent them 100 percent of the income from my recordings of their performances, 50 percent of the income from my print publications, and roughly 50 percent of the lecture fees I have received when they have been the subject of the talk. I have also raised grant funds for their projects.

7. Brazilian anthropologists Patricia Faulhaber and Louis Forline describe an extremely interesting collaboration between researchers and the Ticuna on a CD-ROM. The Ticuna are a large indigenous group living in Brazil, Colombia, and Peru. They write: the Ticuna "understand electronic communication as a form of ritual commodification, one that creates conditions for journeys into the cosmos and for interaction between humans and other beings residing in other worlds" (Faulhaber and Forline 2005). Through indigenous organizations, 1,200 copies of the CD-ROM were distributed to the community, the primary audience for the project, where they have contributed to a widespread cultural revival (Faulhaber 2003).

8. The best place to learn about the agents and agencies with which researchers have interacted during their field research is usually in the "acknowledgments" section of their ethnographies (this is true of my own books as well). I recommend the careful study of these acknowledgments, which often thank individuals and agencies unmentioned elsewhere.

9. During the 1970s, the most frequent accusations of government agencies against researchers and members of NGOs who were working on community projects were misuse of funds (for men) and having sexual relations with Indians (for women). Either accusation was enough to ensure their removal from the indigenous area and the termination of their projects. I recommend proactive caution with respect to both financial accounting and personal relationships for researcher/activists. At the Smithsonian Institution we were instructed to consider how something would look on the front page of the *Washington Post* before undertaking it. The important issue may well be how things appear in the court of public opinion, not how we, or our discipline, consider them.

10. The Suyá have also dealt with government administrators, doctors, researchers, and a few journalists since 1959. So far they have not allowed missionaries to visit their villages.

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