

Long-Term Field Research in Ethnomusicology in the 21st-Century

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Pesquisa de campo
de longa duração na
Etnomusicologia no século 21

Resumo: Este artigo faz uma revisão da história da pesquisa de campo de longa duração na Etnomusicologia (e em alguma medida na Antropologia social) e descreve algumas mudanças resultantes das transformações tecnológicas e éticas no século 21. Com base na experiência do autor entre os índios Suyá/Kisêdjê desde 1971, discute algumas das vantagens e desvantagens dessa modalidade de pesquisa de campo. Entre as vantagens estão o enriquecimento das perspectivas dos pesquisadores como resultado de um maior aprofundamento em campo, enriquecimento oriundo das mudanças tanto na comunidade quanto do pesquisador ao longo do tempo, possibilidades de colaboração e ajuda quanto aos anseios da comunidade, satisfação e realização pessoal, e, especialmente, o aumento do respeito pelas iniciativas dos sujeitos da pesquisa. Entre as desvantagens incluem-se uma possível limitação no desenvolvimento intelectual, uma redução na intensidade da pesquisa, confiança demais ou (alternativamente) muito estresse, bem como conflitos potenciais entre os achados dos pesquisadores e a auto-imagem e a auto-compreensão da comunidade. A melhora na forma de comunicação e o aumento do reconhecimento de que os indivíduos e as comunidades pesquisadas têm seus próprios objetivos, provavelmente contribuirá para um aumento da opção por pesquisas de longa duração. Este fato deve, no entanto, ser também tema de reflexão.

Palavras-chave: Trabalho de campo, Pesquisa, Etnomusicologia, Suyá/Kisêdjê

Abstract: This paper reviews the history of long-term field research in ethnomusicology (and to a certain extent social anthropology) and describes some of the changes resulting from transformations in technology and ethics in the 21st century. Based on the author's experience with the Suyá/ Kisêdjê Indians since 1971, it discusses some of the advantages and disadvantages of field research that extends for long periods of time. Among the advantages are enrichment of researchers' perspectives as a result of greater time depth, enrichment stemming from changes in both the community and the researcher over time, possibilities for collaboration and assistance with community objectives, personal enjoyment and fulfillment, and especially increased respect for the intentions of the subjects of the research. Disadvantages include a possible failure to grow intellectually, a loss of the intensity in the research, too much comfort or (alternatively) too much stress, and potential conflicts between the researchers' findings and the community's self-image and understanding. Improved communication and recognition that the individuals and communities researched have their own objectives will probably contribute to an increased frequency of long-term research. This fact must, however, also be a subject of conscious reflection.

Keywords: Fieldwork, Research, Ethnomusicology, Suyá/ Kisêdjê

Modes of Field Research

In this first decade of the 21st century we can look back on the field research undertaken by both anthropologists and ethnomusicologists in the 20th century and see some profound changes. The weakening of some colonial hierarchies and the strengthening of post-colonial intellectual perspectives, dramatic changes in communications technologies, and changes in the objectives of our fields of knowledge are just a few of these transformations. Other, more local influences on research such as available funding and professional ethics have also had an impact.

At the end of the 19th and during the early 20th century, field research was hardly considered necessary for advancing understanding of musical phenomena. Comparative Musicology was founded upon the analysis of written transcriptions of performers who came to Europe or of audio recordings made by travelers and local scholars that were sent to centers for research and analysis in Europe. This was similar to the way that Sir James Frazer, Edward Tyler, and Lewis Henry Morgan (all considered anthropologists), developed their studies of human institutions and culture. When early comparative musicologists made their own recordings, they often did so in their homes or laboratories, rather than in the normal performance context of the genres they recorded. They often relied on field recordings made by explorers, travelers, and missionaries. In the United States, researchers were more apt to make their own recordings in the field, influenced in part by Franz Boas, who recorded Kwakiutl songs as early as 1897, following the example of anthropologist Jesse Walter Fewkes, who made what is believed to be the first ethnographic recording in 1890.

As George Stocking has described in his studies of the history anthropology (STOCKING 1983), first-hand interactions we would call field research date from the beginning of the discipline, but “field research” was turned into an explicit methodology in the writings of Bronislaw Malinowski. Malinowski argued that researchers should learn the local language, live in the communities they are researching, and obtain an in-depth grasp of daily life by staying in the field for extended periods of time. When he was able to return to Europe, he left the Trobriand Islands and (as far as I know) did not return. In the ensuing years he wrote a number of books based on the research he undertook years before (most famously Malinowski 1932).

The Malinowski model, adopted by much of anthropology (and ethnomusicology) in the ensuing decades, typically involved researchers going to the “field”—however defined, but usually distant from the researcher’s residence – for at least one year (and preferably two or more) of intensive research undertaken in the local language(s). After the completion of their field stays, the researchers typically left and did not return. The researchers published their books and articles in a language inaccessible to the community in which the research was done and little effort was made to return knowledge to the committee or involve the community further in the scholarly products based on their teaching. This was typical of the research done in the Xingu when I was a graduate student, but it was also true of researchers who studied non-indigenous communities in the Northeast of Brazil and elsewhere. This changed with the recognition of the significance of “applied” projects.

“Applied” or “public” anthropology, where anthropologists work to assist communities with socio-economic changes of some kind, has a fairly long history in U.S. anthropology⁴. It also has a long history in ethnomusicology (Sheehy 1992), although its practitioners may not have called it by that name. Increasingly, individuals and communities with which researchers work now require that the researcher become involved in some project of interest to the community. At the same time, many researchers feel morally obligated to assist those they consider in need. While I do not think all research need be of immediate practical use (see Seeger 2008), my use of information to benefit the community has proven to be important to them and very satisfying to me as a researcher. I believe theories are both tested and developed from their application in specific contexts. There is a long tradition of applied anthropology in Brazil, and one motivation for long-term research/activism projects is the desire to see a project through to its conclusion. In the United States, however, many scholars do not become involved in applied projects and many do not maintain contact with the individuals and groups with whom they did their research.

There are both understandable reasons and those that can be criticized for the radical separation of field research and subsequent scholarly work. The “understandable ones”

include the difficulties of transportation and communication, lack of financial support, family obligations, health, and problems of language and literacy. Add to these a researcher's change of occupation, the conclusion of a particular topic with a given group, interest in comparative research, or interest in researching other topics, and it is quite understandable why many researchers leave and never return. We can, however, criticize those who departed making promises that were not kept, or implying future assistance that was not given. Some other ethical issues surround the publication of research data, among them the recognition of the research community's contributions, ongoing community access to photographs and recordings, and the distribution income from royalties. These concerns were rarely expressed because most 20th century researchers did not continue to communicate with the individuals or communities they researched after their research was concluded.

There were some long-term research projects in the 20th Century, however, some of which are discussed briefly in a report on a 1975 Wenner-Gren Symposium, "The Theoretical and Methodological Implications of Long-Term Field Research in Social Anthropology" (Colson et al 1976). All of those attending the conference had done multi-year or longer research and all thought it was valuable. They observed that some social events are multi-year processes—like the 8-year pig feasting cycle in parts of New Guinea, or the generational cycle. Certain research projects by their very nature require extended fieldwork plans. Although valuable for the research, the theoretical benefits of long-term research were not clear to the participants, and there was a feeling that improved methods were required to manage the large amount of data collected over long periods of time. Another kind of long-term project, a little different from individual research goals, was the creation of "field schools" that served as a home for generations of different researchers who visited them. One of the most famous of these was the Chiapas Project, established by Evon Z. Vogt (Harvard University) at Zinacantan, in Chiapas, Mexico (Vogt 1994). George M. Foster briefly describes this in a review.

"During the first two decades of the project, 142 graduate and undergraduate students had engaged in research in Chiapas.... By 1992 project participants had published 40 books, over 180 articles, and two novels based on their research in Chiapas [since 1957]" (Foster 1998: 229).

In the 21st century, changes in transportation and communications technology have dramatically reduced the isolation of most field research sites. Many researchers now also live quite close to their research sites, which further facilitates continuing communication. Even those who reside at a distance may communicate electronically or by voice. Most Brazilian researchers today can communicate easily with their research communities from

their home or institution. It is possible to undertake long-term research projects in a way that would have been extremely difficult in earlier decades.

When Those We Research Set the Terms - how research can become long-term:

My wife and I made our first visit to the Kisêdjê 38 years ago, in July 1971. Although I had intended to complete my research by the late 1970s and publish everything I had to say about the Kisêdjê by 1984 (10 years after completing my PhD), that is not the way it worked out (although the first edition of my last book on them appeared in 1987). Although I prepared a plan for comparative ethnomusicological research among the Northern Je, I did not visit other indigenous communities partly because the Kisêdjê were active agents in convincing me not to do so and partly because I had moved into other areas of professional research activities, among them audiovisual archiving, record production, and the administration of professional organizations (among them SEM and ICTM). The focus of much of my writing changed from the Kisêdjê to issues of intellectual property, cultural heritage, and related topics. My perspective on those issues was informed by my field research but did not develop solely from it.

It is probably hard for young researchers to imagine how laborious it was to communicate within Brazil even as late as the 1980s. I could write a letter to the Kisêdjê, but no one could read it regardless of what language I might use. I could send an audiocassette to them; but often there was no working cassette player in the village. When there was, there were often no batteries for it. Personal visits were difficult too. It could take months to obtain authorization from FUNAI to visit; for decades access was only possible in planes flown by FAB; there were no telephones in the Xingu and the two-way radios there often did not communicate beyond the Xingu region. The collaborative co-produced LP recording and liner notes, *A Arte Vocal dos Suyá* (Seeger e a Comunidade Suyá 1982), took five years to complete because at least one year would pass between each stage of the consulting process. I did send them copies of my books, however, since I knew that even if no Kisêdjê could read them someone else might read the book aloud to them. If that didn't happen, the main contribution of my publication would be that the men could roll tobacco in the pages and smoke them.

The Kisêdjê consider themselves to be responsible for our long involvement with them. One of them described a conversation with an upper Xingu Indian about their respective

"Whites" (referred to as kupën katxi in the Kisêdjê language and as kara'í more generally in the Upper Xingu). The Xinguano asked how come the Kisêdjê's Whiteman kept returning to visit and help them while his tribe's Whiteman left and never returned. The Kisêdjê man had responded that I kept returning because they treated me so well; if his own tribe had treated their White as well as the Kisêdjê treated me, theirs would not have gone away. Two things are worth noting here. First, both Indians considered researchers to be their "property" in some form. Second, the Kisêdjê thought themselves to be responsible for my continuing visits (rather than the attributing it to my ethics, political beliefs, access to research funds, laziness etc.). They acted on their perception of their agency as well. When I had not visited them for over 10 years, they mailed a small package to me in Washington DC containing nothing but an audio cassette. The purpose of the cassette, they later told me, was to make me sad and homesick for their village and their music. Several months later, they sent me a fax saying they were having trouble with invasions in their land and invited me to visit them consult with them about these problems. When we arrived, they told me that their plan to first make me sad and then invite me down directly had been a great success, as evidenced by our arrival in the village. I think the agency of communities "capturing" or maintaining relationships with researchers needs to be considered analytically. The incorporation of members of other groups through capture, adoption, marriage, or other means is fairly common in South American Indian ethnography. But what about researchers? Are we part of that pattern? And if so, how does that fact influence the way we should expect to do our research?

My perspective on our return to the Kisêdjê is different from theirs. When I left my appointment at the Museu Nacional/UFRJ to take up a teaching position in the United States in late 1982, I thought my 1982 field trip would be my last visit to the Kisêdjê. I felt that I had no new questions to ask them; rather I mostly needed time to devote myself to organizing the large number of research journals and recordings that I had amassed during nearly 24 months in the village⁵. During the following decade we kept in touch in messages sent through researchers going to the Xingu and occasionally I would receive a collect telephone call. I decided to return in 1994 because of the direct appeal of the Kisêdjê for assistance with a concrete problem of land that I felt it would be unethical to refuse. My return was fairly quick and simple. In the intervening years, authorization from FUNAI had become much easier. The Kisêdjê simply requested FUNAI to grant the authorization. Travel was much easier as well, since we could take a bus to a point where they could meet us in their own pickup truck and then carry us on the river in their own motorboat. The trip that could take a month or more in 1970 took only two days in 1994.

I also found that I was granted a different role to play in the community in 1994 and that there were many new issues that I could investigate that benefited from a perspective spanning decades. It was also nice to see so many old acquaintances again, and to have my earlier research prove useful for establishing their right to lands that had been taken from them in the 1950s. I have returned at least 7 times since 1994 for fairly short visits, often with a member of my family.

My family and I have enjoyed our decades of association with the Kisêdjê. We enjoy visiting them and seeing how well they have coped with the pressures on them posed by the advancing frontier and the passage of generations. We admire the achievements of each other's children and wish them well for the future. Unfortunately, we all age together and their helath is often precarious—as ours inevitably will become—requiring expensive treatments with which we are unable to assist, and we are not as able to visit them as we once were. I am particularly happy that they are working with anthropologist Marcela Coelho de Souza, with certain local civilian authorities, the Escola Paulista de Medicina, and with NGOs like the Instituto Socioambiental. The Kisêdjê no longer have or need one Whiteman—they have enlisted the support of many other Whites. In spite of all their new supporters, however, they still appreciate our decades with them. They often remark that when they requested assistance, I have sent what I was able to send or sought grants to assist them with their projects⁶. In fact, their experience with us has shaped their relationship to would-be researchers. They ask them “will you be willing to help us in 30 years, like Tony, or will you go away and never send us anything?” The Kisêdjê and the Seegers, in somewhat different ways, appreciate the years of our acquaintance.

Some Advantages of Long Term Research

There are some definite advantages to engaging in long-term research.

1. Greater time depth enriches researchers' perspective. Most researchers visiting a community for 12 months witness a very short moment in its history. Our visits are like a still photograph of a full length film. In spite of our best efforts, we can capture only a fairly brief moment. Revisiting the same community over a period of decades, or maintaining contact through research assistants or high-speed communications, provides a richer understanding of social and musical processes than a single research trip. It is possible to see changes in the community in terms of leadership, languages, music, and almost anything else during 30 years, which can lead to substantially different insights than those made on the basis of a short visit.

2. Multiple opportunities to discover answers to an old question. Sometimes a researcher can resolve an issue that could not be answered years before due to incomprehension or a language barrier. An example of this was my inability to find out whether the Kisêdjê had a word for, or even recognized, the rising pitch (*afinação ascendente*) in their unison songs described in *Why Suyá Sing* (Seeger 2004:88-103). On the last day of my 2007 visit to the village I found two of my best research assistants (formerly called “informants”) sitting quietly behind their house without much to do and had the opportunity to take up the question again. I discovered that the Kisêdjê do have a word for this phenomenon and that they have an explanation for why it occurs—two things I had been unable to determine twenty-five years previously. Sometimes, if you wait long enough, you figure out how to ask a question in such a way that it gets an answer—though the long delay in discovering it raises new questions.

3. As the individual or community changes over time, new questions emerge to be studied and theorized. In 1982 I could think of no new questions to ask that I did not correctly predict the response to. By 1994, ethnomusicology and anthropology had changed, and so had the Kisêdjê and I in some important ways. This opened an opportunity to consider research topics I had never investigated before. And I could do so without starting an entirely new project, learning a new language, and convincing knowledgeable people to work with me. That part was done already. I could take up new subjects with all the benefit of earlier research and all the sophistication of more contemporary anthropology and ethnomusicology.

4. As the researcher ages and changes his or her perspective also changes. Most of the research among indigenous groups has been done by fairly young researchers—and the focus of our interests is probably partly determined by our age. As we become older, have children, and watch our parents age, our interest in certain subjects may change. I wouldn’t exaggerate the importance of this, but it certainly must occur. Our own status changes: as generations die and are replaced we may cease to be “the young stranger asking those irrelevant questions of old people” to “the only one who was paying attention to the elders while they were alive.”

5. Collaboration and assistance. Long-term research enables researchers to be of assistance to individuals and communities when requested and when possible. This may be done in the form of personal assistance or as an applied ethnomusicology project. An example of this was the use of my field data in the evaluation of the Kisêdjê claim to be lands on the Wawi and Pacas Rivers in the 1990s. Although not collected for land claims work, the information I gathered while doing my research included a great deal of the conclusive evidence required for sustaining their claim to the territory (Seeger 2008).

6. Long term research can be comfortable and enjoyable. Returning to a familiar place to visit old friends, watch their children grow, sing with them, and collaborate on publications and other projects can be very enjoyable. Researchers are human; our search to understand what people are telling us often gives us a large emotional investment in our research; and in most cases no one is more interested in our research than the people we are studying. There are, of course, places where it is not comfortable and enjoyable; where the misery and despair of the populations with whom we have done research is heart-breaking⁷.

7. In a number of cases, anthropologists have turned long-term research projects into training and collaborative ventures. The Harvard University project in Zinacantan, established by Evon Z. Vogt referred to earlier, is an example of this, as is the Summer Field School for ethnomusicology at the Catholic University in Lima Peru that documents calendrical rituals in the Andes. In these cases, many different perspectives contribute to a rich, collaborative understanding of an area, and presumably the individuals and communities in the area find the relationship rewarding.

8. Long Term Research respects the intentions of those researched. This is probably the most important of the benefits of long-term research. Today, it is widely recognized that research is not the solo endeavor described by Malinowski but rather collaborative endeavor with the individual or members of the community being researched. Field research has been reconceptualized in both anthropology and ethnomusicology. One of the most important examples of such a change—and one that is particularly important for Brazilian ethnomusicology—is Samuel Araujo's dialogic and participatory research undertaken with residents of Maré, in Rio de Janeiro (Araujo et al 2006). If members of the communities researched are treated as active participants in the research, then the decision of when to end the research does not lie with just one of the researchers. If we are to be truly dialogic and participatory, then the decision of when the work is over, and who should benefit from it and in what ways lies not with the anthropologist or ethnomusicologist, but with the group of all those involved. This means that an ethnomusicologist's research will be long or short not only because he or she wants it that way, but because those researched insist on it.

Some Disadvantages of Long-Term Research

There are some potential disadvantages to long-term research. Overall they are may not be as important as the advantages, but they are worth considering carefully. Researchers should try to minimize their impact in any particular case. Among them are the following:

1. A failure to grow intellectually. Continuing research with a single individual or community may mean that a researcher fails to grow intellectually through research in other areas and communities or on new topics for which the community is not the best place to do the required research. Every topic presents new challenges. The music of indigenous peoples in Brazil raises some issues not necessarily raised by heavily media-driven popular music genres such as Rock or Reggae. If a researcher stays with one research subject, and one group or community, they may be challenged at first and then later fail to grow intellectually from the stimulus of new research projects. To a certain extent this is reduced by advantages 3, 4, and 8 above. But researchers should ask themselves: “am I still being challenged, or is this getting too comfortable?”

2. A loss of intensity. One of the reasons that field researchers learn so much while they are in the field is that they are forced to do so by the strict limitations of time available to them. Field research is always difficult. It is always tempting to put off some of the really difficult topics until later. People who remain in a given community for a long period of time—for example missionaries and government employees—do not necessarily have a deep understanding of the communities in which they live. A longer time for research (once rapport, language ability, and trust have been established) will not necessarily lead to a more profound understanding. If research is thought of as a multi-year endeavor, what would keep the researcher and community from postponing discussion of the more difficult topics until later—much later?

3. Comfort is not necessarily a good thing for research. It can be difficult to ask questions that might embarrass old friends, and yet in every community there are differences between what is said and what is done. We are all masses of contradictions, and it is sometimes harder to point those out, to say nothing of publish about them, when one is making repeated friendly visits. It can also be difficult to question our own earlier conclusions.

4. Conflict between the researcher’s findings and a community’s self-image or understanding. While it is an ideologically satisfying to argue that all publications should be done collaboratively with community members, and that we should not publish what they do not wish researchers to publish, in some cases this will remove entire subjects from analysis. It is easy to imagine many situations in which it would be difficult to obtain consensus about a publication—political activities, economic irregularities, violence against members of another group etc.

Here is a clear-cut case: During the U.S. wars in Iraq and Afghanistan during the 21st century, both journalists and anthropologists were “embedded” or otherwise included in activities of the United States military. Most readers would not want to need military ap-

proval to publish their research results. Readers of this journal would probably not want to request prior consent from the Brazilian government for their publications.

Yet the control exercised by the U.S. military is similar to the control that may be required by indigenous and other communities with whom ethnomusicologists wish to do research. This could have a profound effect on the discipline. Is this important? It depends on what your research is about, and what your understanding is with those with whom you intend to work. Here is the "Executive Summary" from American Anthropological Association report of the commission on anthropologists working for the U.S. military:

The Commission recognizes both opportunities and risks to those anthropologists choosing to engage with the work of the military, security and intelligence arenas. We do not recommend non-engagement, but instead emphasize differences in kinds of engagement and accompanying ethical considerations. We advise careful analysis of specific roles, activities, and institutional contexts of engagement in order to ascertain ethical consequences. These ethical considerations begin with the admonition to do no harm to those one studies (or with whom one works, in an applied setting) and to be honest and transparent in communicating what one is doing. Given this framework, we offer procedural recommendations to AAA designed to address current and future issues, to foster civil and open discussion of them, and to offer guidance to individual anthropologists who might consider such work (AAA 2007:4).

One of the advantages of being an outsider is that one sees things differently and can also learn from one's research associates (formerly known as informants) and yet not being fully integrated into the community⁸. It is reasonable to be concerned that community-vetted research reports might become the equivalent of press releases that reflect the interests of certain hegemonic powers within the communities being studied. This will not necessarily happen, but some discussion of publications should occur before they are undertaken.

5. Stress. It can be very stressful to receive desperate pleas for assistance from people we have worked with during our field research when we are unable to respond to them. It can be difficult for people we study to understand that much of our apparent wealth stems from our research budgets and university-owned equipment, and not from disposable income we have readily available to assist them—or even to return to visit them. If we do research in several different areas, to what extent can we maintain intense relationships with all the people with whom we have worked in addition to the other demands on our time, our e-mail response, our emotions and our income?

Improving Long-term Research Through Collaboration and Courtesy

In spite of some of the challenges of collaborative research that endures over long periods of time, it is quite likely this will increasingly be required by individuals and communities with whom we work and by our own ethical positions. It is not only easier to send copies of our recordings and photographs to others; it is often required by the terms of our research. It is easier to fulfill our long-term obligations to communities by furnishing copies of our work than it ever was in the past, and more of our communities are able to read what we write and to contribute in writing to them.

There is another, less obvious, area where we need to improve our methods and practices. This is in the way researchers manage their own data. One of the concerns of the 1976 Wenner-Gren report was that more attention should be given by researchers to organizing the large amounts of field data that such research often produces. Researchers like William Crocker, an anthropologist at the Smithsonian Institution, whose material on the Ramkokamekra/Canelas includes thousands of pages of correspondence, notes and other material collected over a period of more than 40 years, face massive organizational problems. The way we organize our data is often hampered by the way we start to organize it on our first field trip⁹.

Long-term studies do not necessarily have to be undertaken by a single researcher—sometimes a series of researchers works in the same community over a long period. Multiple researchers working with a single community or group can also reap the benefits of a long-term perspective without some of the disadvantages. In Brazilian Indian studies, for example, even when a given researcher does not return after his or her intensive research, other researchers usually eventually carry out further studies with the same group. They frequently use many of the same research assistants (“informants”) as their predecessors. There is no systematic attempt to ensure that materials collected by one researcher are available to future researchers or to community members. Most researchers consider their recordings and field notes to be personal property. Relatively few of these find their way into publicly accessible archives. Instead, later researchers and community members are usually only able to consult published materials on the groups they study and the communities are usually unable to consult anything. One of the most frequent complaints of communities around the world is that people who have taken photographs and made recordings do not send them copies.

Making copies of field notes, unpublished manuscripts, photographs, and recordings available to later researchers and to members of the community is an ethical issue as well

as a practical one. Field recordings often document events when the communities themselves did not have the ability to record themselves. Even when they do have recording devices, most communities lack the facilities for archiving and preserving recordings they make. If researchers do not take steps to deposit their field-collected materials in archives, are they acting unprofessionally and unethically? I think so, even though some researchers argue that they are reluctant to deposit their materials because some of those materials were collected with the understanding that they were confidential and other community members (and scholars) would not know about them. This problem is easily resolved by restricting access to some materials while allowing access to the rest, or by removing those materials before depositing them.

We can facilitate the work of subsequent researchers in other ways as well through active assistance. The Kisêdjê have had a new White (anthropologist) since 2004. Marcela Coelho de Souza, now Associate Professor of Anthropology at the University of Brasília, is the first anthropologist they have invited to work with them since our research began in 1971. When she began intensive research with the Kisêdjê, I provided her with copies of my typed index/summary of my field notes, and have offered to make the complete handwritten notes available to her as well. I also sent both her and the Kisêdjê digital copies of all of the audio recordings I made and previously deposited in the Indiana University Archives of Traditional Music. In addition, we traveled together to the Kisêdjê in 2007 and made it clear that we were friends and that I fully supported her work¹⁰. With my notes and recordings, and my endorsement to the Kisêdjê, she is better able to understand why I wrote what I did.

It may well be that the byproducts of our research—recordings, photographs, artifacts, and notes—will be more interesting in 100 years than the theoretical publications we develop from them. How many publications from 1909 are interesting today? Yet the rare recordings that survive from that time have been eagerly received by communities in which they were originally made (Niles and Palie 2003)¹¹.

Not every researcher or every community will be interested in undertaking or hosting a long-term research project. On the other hand, it seems to me entirely justifiable to expect that future researchers and the communities should be able to access the byproducts of the research process, such as the recordings and the (perhaps edited or restricted in access) field notes. By failing to create and support institutions to acquire and preserve research materials, we undermine both the lives of the people we work with and the work of future researchers.

Conclusions

Changes in transportation and communications technology have transformed the way anthropologists and ethnomusicologists do research today in many ways. One result is that is more conceivable and easier to establish long-term relationships that endure in one form or another for years, or even decades. Such ongoing relationships have both positive and negative aspects, some of which I have described. I am sure that with thought and self-awareness of the positive ones could be improved and increased, and the negative ones could be avoided or minimized. This is a good thing, because I think long-term research projects will become more common.

While the motives for long-term research in Anthropology and Ethnomusicology in the past have been varied, it is quite possible that research projects in the future will extend far beyond the initial researcher's participation in them. This will be due to the increasingly dialogic, community-based, nature of many future research projects. Even when a researcher chooses to study other things, members of the community may wish to continue the project alone, or with different researchers, or to involve the original researcher in new ways. Similarly, we may expect the proliferation of multi-sited documentation centers that house audio and video recordings, photographs, and videotapes accumulated during research and make them available both to members of the communities and (selectively) to the general public (the multi-sited resource could also be an online repository accessible from computers in different places).

In addition to the long-term research we ourselves do as scholars, it is of critical importance to recognize the kind of long-term research that is undertaken when members of communities continue documentation projects or when a researcher returns to a place where an earlier researcher has already worked. We need to establish a tradition of preserving recordings, photographs, and field notes of researchers so that future researchers and community members may use them¹². Long-term research is not only an individual's option. For our discipline it is often a requirement—required of us by the people with whom we propose to work—and in the 21st century we need to address how it can be best accomplished.

Notes

¹ I originally wrote a draft of this paper for delivery at the IV Encontro da Associação Brasileira de Etnomusicologia/ABET in Maceió in November 2008. I wrote it because ethnomusicology is literally a very young field in Brazil. There are few veteran researchers and many young scholars who are just beginning their first research projects. These young ethnomusicologists have many questions about methods, ethics, and best practices. Since I am one of the relatively small number of ethnomusicologists working in Brazil who has continued to work with the same indigenous group for decades, I thought it was important to contribute something about long-term research. I am deeply indebted to Dra. Marcela Coelho de Souza for her extremely thoughtful comments and suggestions on the earlier draft of this paper. She will certainly see the influence of her input, and I thank her very much for her thoughtful remarks, but she cannot be held responsible for what I did with them. I am also indebted to my wife, Judith Seeger, who accompanied me on most of my research trips and has made important contributions to everything I write.

² The domination of indigenous peoples presents difficulties to researchers. Early explorers describing indigenous peoples usually applied the name given to a people by their neighbors, rather than the community's designation for themselves. This was the case with the Suyá, who now prefer to be called by their traditional name for themselves: Kisêdjê. This poses bibliographic problems, so I have used both names in places to facilitate bibliographic searches.

³ While this paper focuses on ethnomusicological research, some of its observations apply to anthropology as well. Ethnomusicologists have adopted many research methods from anthropology, especially fieldwork. The history of field research on music parallels that of field research in anthropology more generally. Additionally, in Brazil many ethnomusicologists have been trained in Anthropology.

⁴ It has occasionally been very controversial, as in the involvement of U.S. anthropologists in Viet Nam and Afghanistan and the involvement of British anthropologists in colonial administration. In spite of this sometimes controversial history, both the Society for Ethnomusicology and the International Council for Traditional Music have large and active applied ethnomusicology sections and study groups. Many young scholars seek to combine applied and research activities in their careers.

⁵ The lack of new questions was certainly a lack of imagination on my part, and had nothing to do with the richness of Kisêdjê culture and life. In retrospect, it was probably good that I spent some years thinking and writing without returning to the field.

⁶ For many years, when the Kisêdjê had no bank accounts, I would send funds for community needs when requested. These funds came from all the royalties on their audio publications, half (and now all) the royalties on my books about them, and about half the lecture fees of lectures I gave that focused on what I had learned from them, as well as from our personal savings. Today, when possible, I have this sent directly to the account of the Associação Indígena Kisêdjê (AIK).

⁷ An example of this is a message Samuel Araujo sent to members of ABET in December 2008: "Unfortunately there was one more victim among innocent ones last Thursday, an eight-year-old black and poor child of Maré, amidst the daily armed conflict over the wreckage of greed, cynicism and morbidity. Any way out? (Araujo, e-mail December 6, 2008).

⁸ Why does the hero in U.S. cowboy movies ride into town, quickly get rid of all the bad-guys who have been there a long time, and ride away without marrying the school teacher? Partly because he is an outsider without ties to the community. If he were required to marry the schoolteacher, obtain a mortgage from the evil banker, use water controlled by the scheming cattlemen, and obey the law of the crooked sheriff, he probably would never actually “clean up the town” at all.

⁹ I wish that I had started being more systematic in my organization of my field data. It was only quite late that I realized I needed to find a storage and reference system that would serve for all of it. The importance of organization becomes especially clear when the original collector is unavailable to organize it him or herself. It is better to do it yourself, as soon as possible after each field trip.

¹⁰ One Kísédjê said to us on that trip “it is nice to see you like each other; most Whites say bad things about each other and fight.” It is true that representatives of different agencies, among them researchers, are often in conflict. This does not go unnoticed.

¹¹ I am not sure where I should deposit copies of my field notes, photographs, and recordings in Brazil other than with the Kísédjê and Marcela Coelho de Souza. The established folklore archive in Rio de Janeiro does not accept recordings of indigenous traditions (copies of my wife’s recordings of non-indigenous traditions from Espirito Santo are deposited there). The Museu do Índio (Rio de Janeiro) has, at various times, made efforts to establish an audiovisual archive, but with different degrees of success. Now that the analog originals have been digitized and the annotations translated, it would be fairly easy to make a copy—but where should I deposit them? Where will you deposit yours? One of the difficulties of ensuring the passage of research materials from one generation to another is the lack of suitable repositories for them.

¹² Establishing a tradition of organizing, preserving, and providing controlled access to the highly perishable (or quickly obsolete and unplayable on modern equipment and software) is neither easy nor inexpensive. It would require a combination of researcher and community commitment, the strong support of professional organizations like ABET and ABA, and financial support from public and private sources. For many practical observations on audiovisual archiving in the 21st Century, see Anthony Seeger and Shubha Chaudhuri, 2004.

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