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From ‘our pot’ to ‘my pot’: reflections on ownership and long-term fieldwork with the Suyá/ Kĩsêdjê 1971-2019

Anthony Seeger

This paper addresses issues of anthropological and ethnomusicological field research and changes in Suyá/Kĩsêdjê concepts of ownership over a half century. It begins with a pot and moves to a discussion of a few of the issues involved in selecting a research topic and field site. It continues with a discussion of the author’s fieldwork experiences with the Suyá/Kĩsêdjê Indigenous group in Brazil that the author has been visiting for almost 50 years. The paper discusses some of the advantages, disadvantages, and determinants of such long-term field research. Some things have changed a lot over 50 years and others have not changed as much. An important area of change among the Suyá/Kĩsêdjê has been in the concept of “ownership and control.” Kĩsêdjê ideas have been transformed, especially during the past 20 years, by their increased contact with the market economy and their experience licensing body paint designs to a sandal company. The advantages of long-term field research, of encouraging a younger scholar to continue work with the Kĩsêdjê, and the opportunities to learn from changing concepts of ownership and fieldwork are evidenced throughout the paper and summarized in the short concluding section.

KEYWORDS: intellectual property, ownership, Suyá, Kĩsêdjê, long-term research, fieldwork.

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INTRODUCTION

It is a great honour to be asked to present the inaugural lecture for the department of anthropology that carries the name Aula Ernesto Veiga de Oliveira.¹ My lecture, like his life, deals with ethnology and with law. Ernesto Veiga de Oliveira trained as a lawyer, but later became a tireless researcher who investigated many facets of Portuguese life – the people, their agricultural activities, oral literature, festivals, and material culture, to name just a few of the subjects discussed in his extensive writings.

To honour the many years during which Ernesto Veiga de Oliveira tirelessly travelled around the Portugal asking questions and recording, I will be dedicating some of this lecture to the subject of field research. This is also an appropriate subject for an inaugural lecture in Anthropology and other fields where ethnographic research is an important method, among them ethnomusicology, folklore, and some sociology. The methods, approaches and justification of ethnographic research have changed over the decades, as have the ideas and attitudes toward researchers of the people from whom we learn. I will be taking the long view, nearer to the end of my career than to the beginning of it. From this long-term perspective I will discuss concepts of ownership and control and how they have changed over the years.

This paper begins with a pot, then moves on to a discussion of some of the issues involved in selecting a research topic and field site. It continues with a discussion of the author's fieldwork experiences with an Indigenous group in Brazil that used to be called the Suyá. Today they prefer to be called Kísêdjê, and the author honours their wishes through this paper. I have been visiting them for almost 50 years and will describe some of the advantages, disadvantages, and determinants of such long-term field research. Some things have changed a lot over 50 years and others have not changed as much. An important area of change has been in Kísêdjê concepts of "ownership and control." Kísêdjê ideas have been transformed, especially during the past 20 years, by their increased contact with the market economy and its definitions of ownership as well as their experience licensing body paint designs to a sandal company. The advantages of long-term field research, of recruiting a younger scholar to be the Kísêdjê's next anthropologist, and the opportunities to learn from changing concepts of ownership and fieldwork are evidenced throughout the paper and summarized in the concluding section.

1 This paper was originally presented as "Ideias em mudança: dono, proprietário e trabalho de campo na floresta amazônica, 1971-2015" in Lisbon, Portugal, as the 2019 Aula Ernesto Veiga de Oliveira, organized by the Department of Anthropology (School of Social Sciences and Humanities) at ISCTE – Lisbon University Institute. I am deeply indebted to the audience for the lively debates that followed and have incorporated some of their ideas into this reworked paper. When possible, I have kept to the informal style of the original presentation.

MY POT

When I traded some beads for a small clay pot to eat from in 1971, I announced “This is my pot” (*i-ngoi*). No, I was corrected, that is how a child speaks. Adults say “This is our pot” (*aji-iō-ngoi*). The beads had been mine; the pot was not entirely mine. “Our pot” was in fact part of a complex set of relationships and ideas.



DISSERTATION FIELD RESEARCH

Since this was originally a presentation to begin a semester in the Department of Anthropology, I will start with some basic questions about field research: why, where, and for how long?

Why do we choose one research topic instead of another? Multiple reasons may contribute to the selection of a specific subject for research. These include questions related to theory in the arts and/or social sciences, a passion for a particular place, activity, kind of music or sound, or curiosity about family or cultural roots. Reasons may also include national or foundation funding priorities, advisors' preferences, or the possibility of participating in team research. My undergraduate and graduate training were in the social sciences. I wanted to use them to understand the role of music in society, since members of my family had been persecuted for their musical as well as political activities when I was a child. My own theoretical interests were sociological and anthropological. I was interested in studying the relationships between the sounds of music, cosmology, and social organization for my PhD because I thought sound and music might be an important part of the construction of

cosmology and the enactment of social relationships. I felt that Malinowski's functionalism wasn't satisfactory and wanted to investigate the interaction of these areas in a non-capitalist society. My principal advisors at the University of Chicago were Terence S. Turner and Victor W. Turner (not related). Clifford Geertz was on my committee as well until he left for Princeton while I was in the field. They all wrote about field research and I am very grateful for their contributions to my research and writing.

Where should we do our research? The selection of an appropriate place to research depends on a number of factors including appropriateness for the research topic, personal attitudes and abilities, advisors' suggestions, and funding. Whenever possible, it is probably better to start with your theoretical and personal interests and then pick a research site particularly appropriate for your questions and abilities. Not all field sites work equally well for all topics or for all people.

The researcher's personal interests and limitations are important in deciding what we want to research and where we can go. I, for example, feel claustrophobic in large crowds. And I have sensitive ears that are very painful when I am exposed to loud music. This means that it would have been physically difficult for me to address my sociological questions studying rock concerts in large stadiums, or rave parties and EDM. A remote village in the Brazilian Amazon without electricity was much easier for me to contemplate than crowded spaces with amplified sound. Other personal factors that may shape field research include health, abilities, language fluency, and family considerations. Yet field research is important. Living with people, learning from them in the contexts of their daily lives, and watching them act in addition to talking with them provides a much richer body of information with which to address theoretical issues than interviews or observation alone.

Financial support is another important factor in determining field sites. Research can be very expensive, and it is not possible to get funding for every kind of project. Sometimes we have to fit our projects into existing priorities and categories of funding organizations. It is often possible to pursue the same theoretical questions in several different places, however, and grant writing and reporting are essential skills for researchers everywhere. I was fortunate to have my dissertation research funding provided with my graduate fellowship, giving me considerable freedom of choice for a place to do my doctoral research.

It seemed important for my topic to work with a society with complex social organization, a relatively undisturbed cosmology, and ceremonies that would have musical components. I was eventually able to do fieldwork with a fairly isolated Indigenous society in Brazil whose cosmology had not been altered by missionaries, whose economic and social life had not been affected by selling their labour in a market economy, and who enjoyed singing, and performed

long ceremonies. They were members of the Ge language family. The social organization of Ge-speaking groups was famously complicated (Lévi-Strauss 1963). Brazilian ethnographer Curt Nimuendaju had described Ge rites of passage that lasted for months (Nimuendaju 1946). A small society called the Suyá had not yet been studied in depth and one of my dissertation advisors, Terence S. Turner, had studied another Ge society. In sum, the objectives of my research project, my personal inabilities and abilities, the funding and my advisors were all part of my final choice about where to do my fieldwork. I found research very exciting; I never knew where a question or a day in a canoe might lead and every surprise led to an improved understanding.

When is your field research finished? Is it when your grant runs out? Is it when your advisor says, "Hurry up! you are missing the final deadline!"? Is it when you have fully answered all the questions that you wanted to research? It's probably none of these. Your grant may end before you have finished your research, but you hope you have enough for your thesis.² Your advisor will almost certainly want your thesis before you think you are ready to defend it. You will probably never be able to answer all the questions you were trying to research – some of them will turn out to be impossible to work on. Or you might keep researching with the same community for the rest of your life. Most of you in the audience are at the start of your research careers or working toward your doctorates. It may be hard to imagine that you might continue doing research in the same place for fifty years.

LONG-TERM RESEARCH:

HOW COLLABORATIVE RESEARCH BECOMES LONG-TERM

I arrived in Brazil in 1970 and in the Xingu Indigenous Park (Parque Indígena do Xingu) in Mato Grosso in 1971, accompanied by my wife Judy. Her participation in my research was essential for understanding women's perspectives on almost everything. Our research situation was unusually remote even for the time. Northern Mato Grosso, Brazil, was then very isolated. There was no money and there were no stores. There were lots of trees and no roads. There were no settlers or missionaries. We could only reach it by taking a Brazilian Air Force (FAB) supply plane that once a week delivered supplies to Air Force bases and some indigenous outposts in the interior, including the Xingu Indigenous Park. FAB would transport researchers with permission papers from the

2 I often compare thesis-writing with ice skating on a small lake. You want to stay on the thick ice (things that you really know and support) and avoid skating toward the "thin ice" where you might fall through and your lack of knowledge be revealed. Clifford Geertz might have substituted "thick description" and "thin description" for ice, but I never asked him. No one can know everything about a community, but should try to be strong on the material needed for the dissertation topic.

national research council (CNPq), the agency in charge of Indigenous affairs (Funai), and a sponsoring Brazilian institution (in our case the National Museum in Rio de Janeiro). But FAB allowed us to take only a minimum amount of baggage. We received some logistical support from the directors of the Xingu Indigenous Park, Orlando and Cláudio Villas Bôas. They fed us when we were at their administration posts and helped provide transportation to and from the Kísêdjê village, which was about 100 miles by river from the landing strip. The Kísêdjê agreed to host our research and we settled in with them for 15 months of participatory fieldwork.³ It was more participatory than I had anticipated because the Kísêdjê expected me to contribute to the community with food supply by going hunting or fishing every other day. My wife contributed by working in the gardens and processing manioc in the back of the house. We could not take enough weight on the FAB plane to bring any food – we needed our allowance for enough research supplies and trade goods to last six months. The Kísêdjê helped us cut and plant a garden with manioc, maize, sweet potatoes, bananas, a little sugarcane, and a few other crops. While a few men spoke a little trade Portuguese, we had to learn a language never previously studied in order to talk with them. Progress was agonizingly slow, but I learned many things the Kísêdjê wanted me to know about their lives as the men paddled, hunted, fished, or talked in the nightly gatherings in the village plaza. The women talked with my wife as they worked together in the gardens and in the back of the houses. By 1973 my fellowship was over, and I was pretty sure I had enough information to write a dissertation. But I still felt that I did not understand the musical life of the Kísêdjê as well as I should, so I left music out of my PhD thesis. I wrote on cosmology and social organization only (revised and published as Seeger 1981). After I received my doctorate, I was invited to accept a position in social anthropology at the National Museum in Rio de Janeiro by the anthropologist Roberto DaMatta in 1975. I undertook a series of additional field research trips between 1975 and 1982. It was then that I really began to understand how music was part of social life and cosmological ideas.

When did my research end? It hasn't. I thought my 1982 visit would be my last. I prepared a list of questions to investigate in the field and wrote down the answers I expected to receive to those questions. When I asked them, I did not learn anything beyond what I had expected to be told. I didn't know everything about the Suyá/Kísêdjê – indeed I was ignorant of a lot of things – but I couldn't think of any new questions to ask about music. I thought that this was probably a good indication that I had done all the field research that I could do on my topic. By then we had spent a total of about 22 months in the

3 I have written extensively about this elsewhere (in English, Seeger 1981: 1-17; in Portuguese Seeger 1980: 25-43, and subsequent writings).

field, including two trips with our very young children. My wife, children, and I returned to the United States where I began to write *Why Suyá Sing* (Seeger 2004 [1987]).

My research did not end then, however. The Kĩsêdjê had other ideas. They didn't think I should be done. Their understanding was that they had established an enduring relationship with me. They called me "our Whiteman." They wanted us to come back and had a plan to make us return. They sent me a cassette tape with nothing but music on it that they gave to a passing anthropologist who had been a student of mine. She sent it to me in the United States. Then, a year or so later, they sent me a fax saying that they were having problems with the invasion of their land by settlers and wondered if I could come talk with them before they did something drastic about it. When at length we returned in 1994, they proudly told me that their strategy had succeeded. They had sent us the tape with nothing but music on it to make us miss them. Then they sent the fax to explicitly invite us to return. And we came back! We are still visiting them from time to time twenty-five years later, and sometimes communicate by email or WhatsApp. This is something that is worth remembering as we all do research. We are not the only actors in the research endeavor. The people we learn from also have objectives and agency. This is now a commonplace in the collaborative research that characterizes much of the field research being done today. What happens in our field of research, including its duration, depends not only on us but also on our collaborators in the field.

Insufficient attention has been given to the agency of the communities themselves in soliciting the continuity of anthropological research. The two collections of articles I found on the subject discuss examples of long-term research undertaken by individuals or groups of individuals in different locations around the world. Most of the essays highlight the advantages of this kind of research because it is possible to see long-term trends and to accompany what are often profound changes in communities (Foster *et al.* 1979; Kemper and Royce 2002).⁴ These collections include important discussions of the challenges of passing the responsibility for a project to new generations of researchers and the rewards of the close personal ties that develop between researchers and particular families in local communities over decades.

I have written elsewhere about long-term research in general (Seeger 2008). But in this presentation, I want to stress that there are both advantages and disadvantages to spreading out your research over five decades. Most of the publications have mentioned only the advantages.

4 But I only learned of an excellent article by Manuela Ivone Cunha on the complex implications of follow-up research after a period of years and re-studies in general (Cunha 2014) too late to include it in my reflections here.

From my readings and experience I can think of eight advantages of long-term research: (1) Greater time depth enriches researchers' perspective. (2) Multiple opportunities to discover answers to long-standing questions that weren't possible to learn early in the investigation. (3) As the researcher, academic field, or community change over time, new questions emerge to be studied and theorized. (4) Involvement of the community in our research. Our recordings and field notes – may have more enduring value to the peoples we study than any of our theoretical writings (Seeger 1986: 266). (5) Effective long-term collaboration on community and applied anthropology projects (for example Lee and Biesele 2002). (6) Long term research can be comfortable and enjoyable.⁵ (7) In a few cases, anthropologists have turned long-term research projects into training and collaborative ventures (Harvard Chiapas project: Vogt 2002; the Gwembe Valley project: Scudder and Coleson 2002). (8) Long term research may better respect the intentions of those researched.

Research today is no longer viewed as the solo endeavour imagined by 19th and some 20th century researchers. It is instead recognized to be a collaborative endeavour with members of the community being researched. Field research has been reconceptualized from one of discovery to one of more humble learning and collaboration 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 Araújo's dialogic and participatory research undertaken with residents of Maré, in Rio de Janeiro (Araújo *et al.* 2006). If members of the communities 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 only with the anthropologist or ethnomusicologist, but with all those involved. This means that an ethnomusicologist's research may be long or short not only because they want it that way, but because those researched insist on it.

There are some potential disadvantages to long-term research. They may not be as important as the advantages, but they are worth considering carefully. Researchers should try to minimize the impact of these disadvantages. I can think of 5. I describe them a bit more fully because they are rarely discussed: (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 different topics.

5 There are, of course, field sites where it is not comfortable and enjoyable and where the misery and despair of the populations with whom we have done research is heart-breaking. In those cases, field research may be neither comfortable nor enjoyable and return visits are sometimes impossible because the communities have been killed or dispersed.

Researchers should ask themselves: “am I still being challenged, or is this getting *too* comfortable?”⁶ (2) A loss of intensity. One of the reasons 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. A longer time for research (once rapport, language ability, and trust have been established) will not necessarily lead to a more profound understanding. (3) Comfort is not necessarily a good thing for research. After years of familiarity and sometimes friendship with members of a community it can be difficult to ask questions that might embarrass old friends and acquaintances. It can also be difficult to question one’s own earlier conclusions. (4) An increasing conflict of interest between the researcher’s findings and a community’s self-image or understanding. Scholars need to be concerned about whether they are acting as anthropologists or as publicists for individuals or communities and be specific about it. (5) Stress. In addition to comfort, long-term research may have its own stresses.

In sum, there are both advantages and disadvantages to any kind of research project. In the next part of this paper I examine one of the advantages: observing changes in Kĩsêdjê ideas about ownership, property, and identity. One of the reasons this subject is important is that changes in ideas of ownership can also influence some other important aspects of social life.

KĨSÊDJÊ IDEAS OF OWNERSHIP AND CONTROL

Indigenous communities are not necessarily “traditionalists” and change is not always slow. They may decide to change themselves profoundly and rapidly, as the Kĩsêdjê did after they encountered the societies of the Upper Xingu. They refer to their more distant past as “when we drank only water” and “when we slept on the ground” because they quickly adopted new ways of preparing drinks from manioc and sleeping in hammocks. During the nearly 50 years I have been visiting them I have seen some things change profoundly and some not change much at all. Some of the changes come from outside forces. Their rights to land are threatened; their remaining forests are burning due to unpredictable weather; and their water is polluted from agricultural toxins. Among the things that have changed little are those they can better control themselves: the Mouse Ceremony, naming, village structure, certain principles of kinship and sociability, and a profound desire to maintain a degree of autonomy from Brazilian society.

6 In fairness, most anthropologists who have done long-term research say this has not happened to them. I changed my professional activities and intellectual challenges to include audiovisual archiving, establishing and running Smithsonian Folkways Recordings, and administering professional organizations in addition to making occasional visits to the Kĩsêdjê.

One area in which there have been large changes is Kĩsêdjê ideas about property, ownership, and their control of both tangible and intangible things. Their pre-contact ideas of ownership and control were very different from those of Euro-American-based national legislation covering intellectual property. A key Kĩsêdjê concept is that of “owner/controller”. I noted this in my 1974 dissertation and ethnography.

“The concept of the owner-controller (*kande*) permeates Suyá society, even though there is relatively little property in the material sense of the word. Hunting areas, potential garden sites, and fishing spots are all collectively controlled. But it is a fallacy of ethnocentricity to maintain that ownership and property are unimportant. From the Suyá perspective most things have owner-controllers: villages, ceremonies, songs, houses, gardens, belongings, pets, and so forth. The importance of *kande* is pervasive” (Seeger 1981: 181-182).

There has been some more recent excellent writing about indigenous ideas of ownership in Brazil. I recommend an essential overview essay by Carlos Fausto, “Donos demais: maestria e domínio na Amazônia” (Fausto 2008) and a translated collection of articles, *Ownership and Nurture: Studies in Native Amazonian Property Relations* (Brightman, Fausto and Grotti 2016). This book includes an article by Marcela Coelho de Souza about the Kĩsêdjê, previously published in Portuguese.

In 2004 I encouraged a Brazilian anthropologist to do research among the Kĩsêdjê by recommending her to the community, sending her copies of my typed field notes to read, funding part of her first research trip and going with her to the field.⁷ Marcela Stockler Coelho de Souza, now a professor of anthropology at the University of Brasilia, has been doing research among the Kĩsêdjê since 2004 and publishing works based on her focus on contemporary issues (especially 2012 and 2018). I am using her article “The forgotten pattern and the stolen design: contract, exchange, and creativity among the Kĩsêdjê” (Souza 2016 [2012], see also Souza 2018) and our conversations as the basis for my presentation of the licensing of a design to a sandal company, below.

There are probably thousands of local systems of ideas about rights and obligations regarding knowledge, music, material objects, and other things in

7 The concept of ownership sometimes extends to communities. Anthropologists have been known to refer to their research sites as “my community” or “my people” and to resent others who visit them. It works the other way sometimes. The Kĩsêdjê called me “our anthropologist” and only in 2004 asked me if it would be alright for them to have another anthropologist. I told them they could have as many anthropologists as they wished, and I meant it. When Marcela and I were in the field together one Kĩsêdjê observed. “You like each other. That’s good. White people often fight with each other over us.”

use around the world. One of them, the Euro-American definitions of intellectual property, copyright, and authors rights, has become hegemonic partly through colonialism and more recently by its inclusion in the General Agreement on Tariffs and Trade (GATT). The trade agreement has been signed by the vast majority of countries in the world, including Brazil. Well-intentioned efforts by NGOs and anthropologists to protect Brazilian Indians from the unhampered exploitation of their knowledge and cultural heritage have associated knowledge with individual tribes. Efforts have been made to create special rules governing indigenous rights that respect collective ownership and do away with the time limits included in the Euro-American Intellectual Property model.⁸

Coelho de Souza develops a sophisticated argument about Brazilian Indian regimes of knowledge production.

“If the vocabulary of ‘rights’ must be improved, it is because there are many ways of “owning” an object, and the differences among them go well beyond the right to alienate it or not. To whom it should be alienated, with what purpose, for how long, and in what circumstances are primordial, not secondary, considerations. The form and nature of the transaction defines the relationships between the transactors and objects transacted. The property model is obviously too poor to deal with such a logic” (Souza 2016 [2012]: 175).

WHOSE POT IS THIS?

I now return to my opening example, which exemplifies Coelho de Souza’s observations. When I began my field research in 1971, I was quick to discover the complexities of control over objects to which Coelho de Souza refers. When I traded some beads for a small clay pot to eat food from, I was instructed not to say *i-ngoi* or “my pot” because that is the way a child speaks. Adults, I was instructed, say, “our pot” (*aji-iō-ngoi*). Even though I traded beads for it, I did not possess it entirely. But I was its *kande*. I later translated the complex meanings of the word “*kande*” as “owner/controller” (Seeger 1980: 181) because the idea of “ownership” was less absolute than the one I was familiar with.⁹ I was more of a controller of the use of the pot than its absolute owner. People had to ask my permission if they wanted to use it. But since the cultural norm was to never deny a request, even that right wasn’t absolute. It was acceptable to say

8 There is a large literature on this subject, especially regarding the rights of indigenous peoples (see Seeger 2012). As of this writing, Taiwan has one of the most protective laws for indigenous music. The United Nations Declaration on the Rights of Indigenous Peoples (United Nations 2007) addresses control over cultural heritage at some length.

9 Other anthropologists have translated the term as “master” and *maestria* (Fausto 2008).

“later” and “be careful, don’t break it.” But “our pot” was in fact a somewhat shared possession that extended to the thirty people living in the house with my wife and I. The same was true when I learned a song. I sang by invitation of the Kĩsêdjê most ceremonies. According to the Kĩsêdjê ideas about songs, certain knowledgeable people learned songs from animals or other beings and sang them quietly to members of the Kĩsêdjê community to teach them. But the person who sang a song for the first time out loud was called its *kande*. The “owner/controller of ceremonies” taught me an individual song to sing in the Mouse Ceremony. The song was still, in a sense, an animal’s song; the human who taught it learned it and taught it to me; but I was considered the “kande” of the individual shout song (*akia*) because I first sang it loudly and publicly. When I asked what being its *kande* meant, I was told that if other people sang it, I could say “don’t play around! Sing it seriously!” But nothing more. This is a very different concept from copyright law, or authors rights, where being the “owner”¹⁰ of a song includes an exclusive right to the use of the melody and text and the right to sue others who use even a small part of it without permission, not only during a person’s lifetime, but for 50 or 70 years after they die. The Kĩsêdjê concept of “theft” was similarly different from those I was familiar with. The language for a person taking something without permission (what in English would be called “stealing”) was surrounded by nuance, including that they might eventually return it someday. The word *kande* was used for many, many things. A chief was “the *kande* of the village.” A very successful fisherman might be called the *kandê* of the fish – in the sense that he controlled them. There were many types of *kandê*.

WHOSE DESIGN IS THIS?

An important moment in the transformation of Kĩsêdjê ideas of ownership was their decision to work with a company to produce lightweight rubber sandals decorated with Kĩsêdjê designs that would be part of the sandal collection marketed by Brazilian supermodel Giselle Bündchen. The Kĩsêdjê were approached through a São Paulo-based NGO with whom they had collaborated on other projects, the Instituto Socioambiental (ISA), which provided advice throughout the process. The Kĩsêdjê would allow certain designs to be used and would participate in a promotional video with the model to market the sandals in return for a lump sum payment. About two-thirds of the money would go to ISA for its project to replant the headwaters of the Xingu River to protect the water quality of river and its tributaries. Ms. Bündchen had

10 I put “owner” in brackets because copyright is a “limited-term monopoly” on the use of the creation, not property. Eventually the control expires and the song will enter “public domain”, when anyone use it for any purpose.

supported that project earlier. The remainder of the payment would go to the Kĩsêdjê in exchange for the use of their designs and collaboration.

In many ways this seems like an ideal and deserving project. Efforts to reduce water pollution were very important to everyone in the region and the Kĩsêdjê would benefit from something they would just license, not lose permanently. ISA provided legal and technical assistance. Giselle Bündchen would spend time in the village and be painted with a design for a promotional video.¹¹ But things were not that simple, as Marcela Coelho de Souza describes in her article (Souza 2016 [2012]).

Several complications arose from the agreement, of which I'll just name three. First was the necessity of picking a body paint design to paint on Giselle that no other indigenous group could claim was theirs. The Kĩsêdjê did not think it appropriate to paint her with a men's body paint design. The Kĩsêdjê had to recall or create a long-forgotten (or new) women's body paint design because they had adopted all of their current women's body paint designs from neighbouring indigenous groups in the Upper Xingu over the past two hundred years.¹² Those groups would certainly have demanded part of the licensing payment if they had used them. Second, they had to decide what body ornaments could be used by Giselle. Third, they had to decide what to do with the money they received. While each of these might appear to be simple, each was part of some major changes happening among the Kĩsêdjê and involved lengthy discussions and negotiations.

The need to find a unique body paint design

Knowledge quickly becomes a contentious issue when money is involved and it becomes a commodity, governed by a largely European set of concepts. Kĩsêdjê ideas about their rights changed in this new environment. The Kĩsêdjê description of their history does not start with their creation from nothing, but rather with their existence as a group of people without fire, garden crops, names, ceremonies, and lip discs, among other things. Their stories about the past include how they took fire from the jaguar,¹³ maize from the mouse,¹⁴ learned about gardens from an old woman, and names from people underground. In the same way they did not compose songs of their own but learned them from many outside sources, including enemies, animals, monsters, and anthropologists (Seeger 2004 [1987]: 52-65). Kĩsêdjê history is thus one of repeated additions through appropriation or gifting (Seeger 1993; Souza 2016

11 This video, available on YouTube for several years, is no longer posted.

12 Adding Upper Xingu ceremonies to their activities was another of the important changes they adopted after they encountered those groups in the early 19th century.

13 See Turner and Fajans (2017) for an analysis of the Kayapo version of this story.

14 This story is central to the Mouse Ceremony and appears in the book *Why Suyá Sing* (Seeger 2004 [1987]).

[2012]). But suddenly, in this new intellectual property regime, that was no longer possible. They had to rethink their historical process and consider that by Euro-Brazilian and local standards what they had adopted as theirs was perhaps not theirs to use for money.¹⁵ Finding an “original” body paint design was thus very important. They reconstructed or found one, and painted Giselle Bündchen with it in the promotional video.

Their concern about their right to use ideas traceable to the Upper Xingu extended beyond the sandal license. They decided to stop making and selling certain baskets that used Upper Xingu designs. They stopped carving bird-shaped stools and ceased to paint designs of Upper Xingu origins on artifacts they made for sale in nearby cities as well. They created new kinds of artifacts for sale that were completely distinct from those of any other group. They also asked me to remove a track from a recording that accompanied *Why Suyá Sing* because they had learned it from another tribe recently and did not feel it was theirs to perform on a CD. They acquired permission to perform some of the Upper Xingu ceremonies they have done for generations from an Upper Xingu leader. Thus, the Brazilian concepts of intellectual property manifested in negotiations over licensing the sandal design were instrumental in changes they made in their material culture, musical performance, and understanding of history.

Deciding what ornaments are appropriate for a woman to use in an advertisement

The Kĩsêdjê elders decided that Ms. Bündchen should only be ornamented with women’s ornaments and designs. The design they recalled would serve for her body. But there was a lot of discussion about whether she could wear a feather headdress only worn by men and other details. In the end, after discussion, they recognized the importance of the symbolism of feathers headdresses to whites and allowed Giselle to be photographed in the headdresses (Souza 2016 [2012]: 171-173).

Division of the money received

Until this contract, most of the payments from outside to the Kĩsêdjê I knew about had been paid to the community as a whole. At first this was done through their leaders. Later, payments were made through a community organization, the Indigenous Association of the Kĩsêdjê (Associação Indígena Kĩsêdjê, or AIK). But because of the wording in the contract with the sandal

15 The attempt to associate indigenous knowledge with entire groups rather than individuals marked an important advance in thinking about Indigenous property. But during thousands of years of movement and social contacts, so much knowledge was shared that attributing artifacts and designs to a single tribe in perpetuity can create a new kind of distortion.

company, funds had been promised to “actors” who were to be paid individually. As a result, for the first time I was aware of, Kĩsêdjê participants in the advertising film were paid individually. They received different amounts according to their age and family status (younger unmarried participants were paid less than older married ones) (Souza 2016 [2012]: 179). Thus, half of the money coming to the village went to individuals and the other half to purchase a large truck for community use. This kind of individual payment for a community project was another departure from my experience.

The Kĩsêdjê began changing other things at about the same time as the sandal negotiations. Perhaps related to considerations of authenticity and identity, they began to replace Upper Xingu Indian words for things in common usage with words in their own language. Some replacements were old Ge-language words. For example, the Kĩsêdjê who saw whites for the first time called them “big-skinned people”. This referred to their baggy clothing. Since before 1971 they had been using the Upper Xingu word *carai* for whites, which was shared by most groups in the region. But they decided to change back to the earlier word. They also changed their group name from *Suyá*, a word of outside origin, to the original Ge-language name they had used for themselves, *Kĩsêdjê*.¹⁶ These are just two of many word changes. On my more recent visits they have kept reminding me to use replacement words that were not in use during the first twenty-five years I talked with them because I use the old ones automatically.

CONCLUSION

“How do I say, ‘my pot’ in Kĩsêdjê?” I asked three Kĩsêdjê in September 2019. We were sitting in a Brazilian restaurant having dinner and I purposely asked them the question in Portuguese, which they all spoke. They were in New York to receive a United Nations award for a sustainable agriculture project they had initiated.¹⁷ Clearly the Kĩsêdjê are no longer isolated monolingual people, but experienced world travelers. “You say *I-ngoi* (my pot)”, the three agreed. “But what about *aji-ô-ngoi* (our pot)?” I asked, using the Kĩsêdjê word. “They mean the same thing” they said. In 1971 they did not mean the same thing. The first was an incompletely socialized child’s way of saying “my pot” and the second was the correct adult way of saying it: “our pot”. A small change,

16 One of my Kĩsêdjê teachers told me this was their name for themselves when I first started my research in 1971, but they didn’t mind being called *Suyá*, which had been used for them since they were first contacted by a German expedition in 1884. This change does create a certain confusion in the bibliography. Both names are used for the same group.

17 For a video of the trip to receive the UN Equator Prize with subtitles in portuguese, visit < https://www.youtube.com/watch?v=EflUzh07w_I >. For a video of the project itself, visit < https://www.youtube.com/watch?v=BZ1aW3_fqTk >.

perhaps, but one that expresses some of the larger transformations that have occurred within Kísêdjê society with respect to ideas of ownership and control.¹⁸

One of the advantages of long-term research is the opportunity to witness changes as they occur. I certainly have done so. The Kísêdjê have lost control over some things they used to claim as theirs. They have stopped making and selling certain artifacts. They have stopped using some body paint designs under certain situations. They have asked to have a track removed from a CD recording because it wasn't "theirs". They have removed many loan words from their spoken language. Associated with this process has been an apparent growth of individualism exemplified by individual payment for participation in a community project. I don't mean to imply that these changes are either good or bad. They are occurring through long discussion sessions in the men's house that women now attend. They are making an ongoing effort to deal with the many challenges of their close contact with Brazilians in a complex interethnic environment.

I may have visited the Kísêdjê over a span of nearly half a century, but Marcela Coelho de Souza has researched the Kísêdjê far more intensely than I during the past 15 years. Restudies and new research can be constructive, innovative, and helpful to both the community and to anthropology. Now, in 2020, former Kísêdjê school teacher Tempty Suyá, the son of a great singer, will write his MA dissertation in linguistics on the texts of a particular style of oratory related to hunting. He will be using some of my recordings from the 1970's and some of his own recent ones. One thing we anthropologists should consider as we do our research is to avoid angering our research associates in the field so much, they never want to work with another anthropologist. Our successors may understand more than we ever shall. And if we care for them, members of the community may find our recordings, publications, and other by-products of our research valuable for pursuing their own careers as knowledge creators. I cannot stress enough the importance of preserving audio and video recordings and organizing data for future use. There are, indeed, advantages to working with one group over a long period of time. But some of the best insights may develop when different people do the research and writing, including members of the community themselves. My research is not "my knowledge" but "our knowledge." Perhaps this long description of my activities with the Kísêdjê will not only be "my story" but "your story" in the future. I wish you success.

18 A word of caution here. I am not basing my discussion of changing ideas on this example. I use the pot example only as a symbol for much larger and complex processes. Since I was alerted to the complexity of the concept of "owner/controller" when I got my pot, I decided to ask three of the younger generation of leaders about it. My dinner companions may have thought Portuguese could not express the subtlety of the difference between the two words. They may have decided I wouldn't understand the difference. Or maybe they just wanted to eat their dinners after a long day.

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