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Studying Indigenous Brazil: The Xavante And The Human Sciences, 1958-2015

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Studying Indigenous Brazil: The Xavante And The Human Sciences, 1958-2015

Abstract

This dissertation is a history of how Indigenous people and scholars from the natural and social sciences have engaged one another since the 1950s in Brazil. Through a case study of the Xavante, one of the most intensely studied groups in Central Brazil, it traces the evolution of relationships between researchers and research subjects. Xavante communities began establishing contact with Brazilian national society in the mid-1940s in the wake of settler colonial expansionism. This high-profile process of contact drew interest from researchers, with the first long-term academic ethnographer arriving in 1958. Scholars from across the human sciences followed, particularly from the fields of anthropology, human genetics, and public health. During subsequent decades, the Xavante were constructed as a population, characterized, and circulated internationally in the form of data, biological samples, and publications. In this sense, this story provides a thread to follow the development of twentieth-century approaches to the characterization of human cultural and biological diversity. It is a history of the building of national research institutions in Brazil and a transnational account of knowledge production during the Cold War and after its end. However, by combining the national and transnational with attention to the intimate experience of research, this project traces the history of creation and circulation of academic scholarship back to its origin in the field. As an in-depth examination of the iterative fieldwork that underlay these large-scale processes, this study is locally grounded in the Xavante villages and the interpersonal interactions and labor that form the basis for knowledge production. It shows how Indigenous people have engaged in scientific knowledge making for their own social, economic, and political ends, and have, in the process, shaped the scholars and disciplines that sought to characterize them. It is a history of how researchers and subjects made and remade themselves through the human entanglement of research.

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STUDYING INDIGENOUS BRAZIL:
THE XAVANTE AND THE HUMAN SCIENCES, 1958-2015

Rosanna Jane Dent

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STUDYING INDIGENOUS BRAZIL: THE XAVANTE AND THE HUMAN
SCIENCES, 1958-2015

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To my mother,
Patricia Rushbrooke Higgins

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possible for me to join this project, which was the beginning of a steep learning curve.

In 2014 I met with Tsuptó Buprewên Wa'iri Xavante and three elders Barbosa Sidówi Wai'azase Xavante, Luiz Hipru Xavante, and Agostinho Seseru Xavante to discuss my project and its potential relevance for the village of Pimentel Barbosa. With their agreement, I joined *Wahömanazé* and began working to collect images and research materials that might be of use in the village. I traveled to Pimentel Barbosa Village in July 2015. I am grateful to the members of the *warã* who considered and agreed to my proposal to work on the project for a period of just under four weeks. Many people offered support and made my stay possible, and while I include names of only those individuals who specifically gave me permission, I am grateful to all that helped. Special thanks go to my adoptive father Sereburã and to my adoptive uncle Sidówi, who cared for me in many ways—likely more than I know. Sidówi, who passed on in 2017, is sorely missed. I am also particularly grateful for the work of the elders who spent time viewing digital materials for the archive and sharing and recording their memories. Tsuptó offered crucial support of interpretation and guided me in many ways. For their work on *Wahömanazé* and for their company, I thank Romeu, Marco Aurélio, and especially Lincon. Teacher Delci Lúcia Schmitz offered company, delicious food, and encouragement at a few key moments at the post.

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ABSTRACT

STUDYING INDIGENOUS BRAZIL:
THE XAVANTE AND THE HUMAN SCIENCES, 1958-2015

Rosanna Jane Dent

M. Susan Lindee

This dissertation is a history of how Indigenous people and scholars from the natural and social sciences have engaged one another since the 1950s in Brazil. Through a case study of the Xavante, one of the most intensely studied groups in Central Brazil, it traces the evolution of relationships between researchers and research subjects. Xavante communities began establishing contact with Brazilian national society in the mid-1940s in the wake of settler colonial expansionism. This high-profile process of contact drew interest from researchers, with the first long-term academic ethnographer arriving in 1958. Scholars from across the human sciences followed, particularly from the fields of anthropology, human genetics, and public health. During subsequent decades, the Xavante were constructed as a population, characterized, and circulated internationally in the form of data, biological samples, and publications. In this sense, this story provides a thread to follow the development of twentieth-century approaches to the characterization of human cultural and biological diversity. It is a history of the building of national research institutions in Brazil and a transnational account of knowledge production during the Cold War and after its end. However, by combining the national and transnational with attention to the intimate experience of research, this project traces the

history of creation and circulation of academic scholarship back to its origin in the field. As an in-depth examination of the iterative fieldwork that underlay these large-scale processes, this study is locally grounded in the Xavante villages and the interpersonal interactions and labor that form the basis for knowledge production. It shows how Indigenous people have engaged in scientific knowledge making for their own social, economic, and political ends, and have, in the process, shaped the scholars and disciplines that sought to characterize them. It is a history of how researchers and subjects made and remade themselves through the human entanglement of research.

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GLOSSARY OF TERMS

Xavante Language Terms:

A'uwe – Person, or Xavante person

A'uwe uptabi – True people, auto denomination of Xavante

Owawë – One of the exogamous Xavante moieties, “Big water”

Poreza'õno – The other exogamous Xavante moiety, “Tadpole”

Wai'a – An initiation ceremony for men

Warã – The men's council, composed of all adult men considered “mature” within the Xavante age-grade system. This word also refers to the location of their meetings in the middle of the village, and to the meeting itself.

Warazú – Non-Xavante person

Portuguese Language Terms:

Cacique – Chief, in the case of the Xavante, often referring to a leader who holds the recognized position of representative for their community for interactions with the government. Xavante leadership is not traditionally held individually.

Carioca – Person from the city of Rio de Janeiro

Cerrado – A savannah ecosystem found in large parts of Central Brazil

Chefe do Posto – Post Chief, or government agent in charge of an SPI or FUNAI post

Fazendeiro – Ranchers or large landowners

Indigenista – Relating to government policy regarding Indigenous affairs

Terra Indígena – Indigenous Territory

Acronyms:

ASI - Assessoria de Segurança e Informações, Security and Information Committee

CNPq – Conselho Nacional de Desenvolvimento Científico e Tecnológico, National
Research Council

CONEP – Comissão Nacional de Ética em Pesquisa, National Commission for Research
Ethics

ENSP – Escola Nacional de Saúde Pública, National School of Public Health

FAPESP – Fundação de Amparo à Pesquisa do Estado de São Paulo, São Paulo Research
Foundation

FIOCRUZ – Fundação Oswaldo Cruz

FUNAI – Fundação Nacional do Índio, National Indian Foundation

SPI – Serviço de Proteção aos Índios, Indian Protective Service

T.I. – *Terra Indígena*, Indigenous Territory

UFMG – Universidade Federal de Minas Gerais

UFRGS – Universidade Federal do Rio Grande do Sul

UFRJ – Universidade Federal de Rio de Janeiro

USP – Universidade de São Paulo

Introduction

Subjects of Study

Sidówi's Appeal

“You are going to do important work,” Sidówi Wai’azase Xavante told me one afternoon early in my stay in *Terra Indígena* Pimentel Barbosa. We had just looked through a series of digitalized photographs, scanned from books and researchers’ papers, which documented the village in the 1950s, 60s, and 70s. “You’re going to bring back the work of people who passed through the village. David, Laura.”

Sidówi, during his life, knew and hosted more researchers than he could list to me that day, as we sat sitting together on two plastic chairs in his house with the laptop computer I brought propped up on a stool and the audio recorder balanced next to it.

“He is waiting for news from James,” Tsuptó would tell me a few days later as he translated Sidówi’s speech to the microphone (and me) from Xavante into Portuguese. “Because James is his son, his nephew. Carlos, Ricardo ... he misses them. They are people who did important work here. And Nancy, she is his aunt. When she was here, he became very attached to her. She also played an important role. And that is how they developed a friendship, and a friendship doesn’t disappear from one day to the next. He feels nostalgia when the person leaves.”¹

In the two years leading up to my time in Pimentel Barbosa, I had been cultivating relationships with a network of researchers in Brazil and beyond that included Ricardo

¹ Sidówi Wai’azase Xavante, interview with Rosanna Dent, trans. Tsuptó Buprewên Wa’iri Xavante, 15 July 2015, T.I. Pimentel Barbosa, MT. All translations are my own, unless otherwise noted. In this

Ventura Santos, Carlos E. A. Coimbra Jr., Nancy Flowers, and James R. Welch. I had visited the late David Maybury-Lewis' family, and had a cup of coffee in a hurried conference lobby with Laura R. Graham. I had also been willingly enrolled by Sidówi and a number of other elders in the village to help on a cultural documentation project that the community was conducting in collaboration with the *Museu do Índio* and a group of researchers in Rio de Janeiro.

Sidówi's speech to me was not a simple recording of memories, jogged by the slideshow of images. It was an enjoinder to do "important work." It was an affirmation of the mutual obligation of researcher and research subject, directed at me and at the long line of scholars whose trail I had traced to the village. It was an articulation and confirmation of his ties with these *warazú*—non-Xavante people—who have come over the years to conduct research in a place they understood to offer unique insight into human life.

This study is a "history of the present"² in the sense that it begins from the question of how and why, in 2015, Sidówi had such deep experience of *warazú* knowledge production. How did the Xavante community of Pimentel Barbosa village and the other villages of *Terra Indígena* (Indigenous Territory, T.I.) Pimentel Barbosa become privileged sites of academic fieldwork?³ And how did the Xavante, more

² Michel Foucault, *Discipline & Punish: The Birth of the Prison*, trans. Alan Sheridan (New York: Vintage, 1995[1975]), 31.

³ For the purposes of this dissertation, I have limited my study to academic knowledge production, despite the fact that boundary making for this category is fraught. I have not included attention to the extensive research of Salesian missionaries, which has resulted in a large body of work particularly from the T.I.s of São Marcos and Sangradouro. This scholarship and the individuals responsible for the long-term studies were important interlocutors for the researchers I study here. Paula Montero has analyzed their work to great effect, in comparison with Salesian knowledge production with Bororo and Amazonian Indigenous

broadly, come to be the topic of almost three hundred academic inquiries, published in theses, journal articles, and books in a span of less than sixty years?

This dissertation presents the deep history of research conducted in Xavante communities in the second half of the twentieth century and the first decades of the twenty-first. During these years the Xavante (self denominated *A'uwe* or *A'uwe uptabi*)⁴ were constructed as a population, characterized, and circulated internationally in the form of data, biological samples, and publications.⁵ In this sense, this story provides a thread to follow the development of anthropology and human biology through the second half of the twentieth century. It is a history of the building of national research institutions in Brazil and a transnational account of knowledge production during the Cold War and after its end.⁶ However, the central intervention of this project is to combine this focus on

groups. She traces change over time in religious discourse about Indigenous peoples, as well as the foundational relationships between missionaries and Indigenous subjects. See Paula Montero, *Selvagens, civilizados, autênticos: a produção das diferenças nas etnografias Salesianas, 1920–1970* (São Paulo: EDUSP, 2012), especially 439–489 on the Xavante.

⁴ When speaking Xavante (*a'uwe mremre*) the Xavante refer to themselves as *A'uwe*, but in interactions with outsiders and in public settings the Xavante refer to themselves as Xavante. Following their lead, and because I am interested in the construction of knowledge at this public interface, I use the name Xavante throughout this work.

⁵ A note on spellings and terminology: In my own text I privilege Xavante names for villages and sites, such as Wedezé village, also known as São Domingos in Portuguese. I maintain the Portuguese names when quoting other sources or when used commonly by Xavante actors. There are multiple spellings of names of Indigenous groups (e.g. Chavante, Shavante, and Xavante) and individuals' names (e.g. Apowê, Apewe, Apoená). For Xavante names, here I use the orthography currently under development in the local school of Pimentel Barbosa and Etênheritipá villages. For other Indigenous group names, I use the spelling currently recognized by the *Instituto Socio-Ambiental* in my own prose. I maintain all original spellings when citing primary sources. Likewise, I use terms such as *indio*/Indian, or Amerindian in quotations when these were the terms employed by my historical actors.

⁶ This project joins a welcome surge in attention to science, technology, and medicine in the English language historiography of the Cold War in Latin America. This scholarship emphasizes Latin America as a site of creativity and innovation within larger transnational pressures of the Global Cold War. On medicine, see Marcos Cueto, *Cold War, Deadly Fevers: Malaria Eradication in Mexico, 1955–1975* (Baltimore: Johns Hopkins University Press, 2007). Two compelling works on the imbrications of science and politics with US-Latin American relations are: Eden Medina, *Cybernetic Revolutionaries: Technology and Politics in Allende's Chile* (Cambridge, MA: MIT Press, 2011) and Gisela Mateos and Edna Suárez-Díaz, "Peaceful Atoms in Mexico," in *Beyond Imported Magic: Essays on Science, Technology, and*

the national and transnational with attention to the iterative nature and intimate experience of research. *Studying Indigenous Brazil* brings the history of creation and circulation of academic scholarship back to its origin. As an in-depth examination of the fieldwork that underlay these large-scale processes, this study is locally grounded in the Xavante villages and the interpersonal interactions that form the basis for knowledge production across the fields of the human sciences.⁷

Re-centering the story in the field and situating knowledge production in the human relations that made it possible uncovers diverse ways Xavante subjects contributed to and participated in the process that Ian Hacking has called “making up people.”⁸ As Susan Lindee has shown in a different context, “special, isolated human populations have sometimes been active participants in the scientific enterprise.”⁹

Scientists from almost every discipline of the human sciences have visited Xavante communities.¹⁰ In a political context of extreme challenges under settler colonialism¹¹—

Society in Latin America, ed. Eden Medina, Ivan de Costa Marques, and Christina Holmes (Cambridge, MA: MIT Press, 2014), 287–304; On family planning and population politics see: Raúl Necochea López, *A History of Family Planning in Twentieth-Century Peru* (Chapel Hill: University of North Carolina Press, 2014); Gabriela Soto Laveaga, *Jungle Laboratories: Mexican Peasants, National Projects, and the Making of the Pill* (Durham: Duke University Press, 2009). On the global cold war, see Odd Arne Westad, “The New International History of the Cold War: Three (Possible) Paradigms,” *Diplomatic History* 24, no. 4 (2000): 551–65.

⁷ This project is informed by feminist STS approaches that posit that all knowledges are partial, and to be properly understood must be situated. See: Donna Haraway, “Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective,” *Feminist Studies* 14, no. 3 (1988): 575–99; Sandra Harding, *Sciences from Below: Feminisms, Postcolonialities, and Modernities* (Durham: Duke University Press, 2008).

⁸ Ian Hacking, “Making Up People,” in *Historical Ontology* (Cambridge, MA: Harvard University Press, 2002 [1983]): 113.

⁹ M. Susan Lindee, *Moments of Truth in Genetic Medicine* (Baltimore: Johns Hopkins University Press, 2005), 21.

¹⁰ For the purposes of this dissertation, I take the widest possible definition of the human sciences, including not only those fields most traditionally considered—such as anthropology, history, economics, and sociology—but also natural sciences and biomedical inquiry such as human ecology, public health, and human genetics. This broad definition groups together almost all of the research that has been carried out in

arrogation of land, food insecurity, socioeconomic marginalization, and persistent dangers to health—Xavante actors have engaged these scholars for their own reasons. In the process, they have shaped and been shaped by the knowledge produced about them. They have learned to express, perform, and record their identities in new ways as they seek to secure rights and recognition under the liberal state. They have also increasingly developed strategies to guide and direct scholars, exercising their influence on the resulting depictions of Xavante life.

As the Xavante shaped how they were “made up” they also “made up” their academic *warazú* and shaped the academic disciplines to which they belonged. In early years of research (1950s-1970s), social and natural scientists constituted the Xavante as an object of study, and Xavante actors had little say in how they were used to shape scientific imaginaries. During this period, they indirectly influenced new methodologies as well as debates about human microevolution and the structures of human society and mind. This scholarship provided the basis for subsequent layers of investigation, with a steady increase in academic interest over the years. From the 1970s on, Xavante-researcher relationships shifted with communities’ increasing contact and experience

Xavante territory, and emphasizes the interconnected quality of knowledge production. Most of those fields traditionally included in the category of human sciences are represented within the body of research on the Xavante, with the exception of the psychological sciences. Refer to Appendix 1 for a comprehensive list of academic publications on the Xavante.

¹¹ I use the term settler colonialism to denote the land-based form of colonialism that affects Indigenous people on an ongoing basis throughout Brazil. Patrick Wolfe has described settler colonialism as “an inclusive, land-centered project that coordinates a comprehensive range of agencies, from the metropolitan center to the frontier encampment, with a view of eliminating Indigenous societies.” He emphasized that settler colonialism is a structure and not an event. I understand the ongoing interactions of Xavante and non-Indigenous people within this frame. See Wolfe, “Settler Colonialism and the Elimination of the Native,” *Journal of Genocide Research* 8, no. 4 (2006): 393. On the use (and lack thereof) of the concept in Latin America, see Richard Gott, “Latin America as a White Settler Society,” *Bulletin of Latin American Research* 26, no. 2 (2007): 269–89.

with research, and with changes in broader ideas about the ethics and politics of research. As the discipline of anthropology professionalized in Brazil and concurrently faced crisis and reorientation at the international level, *warazú* researchers played important roles in the construction of new forms of anthropological advocacy. Xavante leaders, confronting serious threats to their land and lives, helped compel their researchers to engage in new ways, both in the field and with international and state actors.

Finally, by examining field practices as they developed over iterative experiences in Xavante communities, *Studying Indigenous Brazil* provides insight into the situated, embodied realities of fieldwork, both for researcher and subject. This project emphasizes the permeability and incompleteness of these categories, conceptualizing research in the human sciences as fundamentally a question of “self-in-relation.”¹² At the heart of this dissertation is the argument that affective labor is central to the human sciences in the field. I take seriously Sidówi’s nostalgia or longing (*ipezede* in Xavante and *saudade* in Tsuptó’s translation to Portuguese) for the researchers with whom he shared years of experience. Care necessarily emanates both from researcher and research subject, consisting of “acknowledgement, concern, affirmation, assistance, responsibility, solidarity, and all the emotional and practical acts that enable life.”¹³ I also understand the affective field as qualified by the murky, conflicted and at times conflictual entanglements of the practical acts that enable life in the field. Xavante affective labor turned *warazú* researchers into adoptive aunts, nephews, and daughters. This relational

¹² Fyre Jean Graveline as quoted in Margaret Kovach, *Indigenous Methodologies: Characteristics, Conversations and Contexts* (Toronto: University of Toronto Press, 2009), 14.

¹³ Arthur Kleinman and Brigitte Hanna, “Catastrophe, Caregiving and Today’s Biomedicine,” *Biosocieties* 3, no. 3 (2008): 291.

work—care work—has epistemological and political implications. It constitutes part of how the Xavante make up their researchers, and part of how researchers make meaning of their work. Moving through the intimate affective realm of Xavante-researcher relationships to transnational knowledge production in the context of the settler-colonial state, this project follows the reverberations of fieldwork through the subjectivities, lives, and institutions of the human sciences.

How the Xavante Got Their Researchers

It was during Brazil's *Estado Novo* (1937-1945) that the Xavante first became famous. As Xavante resistance stalled president Getúlio Vargas' "March to the West"—a program of developmentalism and western expansionism—the group made headlines.¹⁴ Fueled by the national media coverage that accompanied the government's program of "pacification,"¹⁵ research in Xavante territories began a few years after a Xavante group established relations with the Brazilian state in 1946. Early *warazú* presence primarily consisted of government agents, who tried to manage the Xavante, offering gifts of material goods and attempting to convince them to settle so as to free up territory for

¹⁴ For an overview of discourses regarding Indigenous peoples under the *Estado Novo*, see Seth Garfield, *Indigenous Struggle at the Heart of Brazil: State Policy, Frontier Expansion, and the Xavante Indians, 1937–1988* (Durham: Duke University Press, 2001), 23–44. Also see, Tracy Devine Guzmán *Native and National in Brazil: Indigeneity after Independence* (Chapel Hill: University of North Carolina Press, 2013): 124–130.

¹⁵ Lincoln de Souza, *Entre os Xavante do Roncador* (Rio de Janeiro: Ministério de Educação e Saúde, 1952); Lincoln de Souza, *Os Xavante e a civilização* (Rio de Janeiro: IBGE, 1953); Sylvio da Fonseca, *Frente a frente com os Xavantes* (Rio de Janeiro: Pongetti, 1948); Garfield, *Indigenous Struggle*, 59. Xavante versions of this history emphasize that it was the Xavante who attracted and "pacified" the *warazú*. See: Sereburã, Hipru, Rupawê, Serezabdi, and Sereñimirãmi, *Wamrémé za'ra, nossa palavra: mito e história do povo Xavante*, trans. Paulo Supretaprã Xavante and Jurandir Siridiwê Xavante (São Paulo: Editora SENAC, 1997), 135–138. For other perspectives on contact from the point of view of Indigenous groups, see Bruce Albert and Alcida Rita Ramos, eds. *Pacificando o branco: cosmologias do contato no Norte-Amazônico* (São Paulo: Editora UNESP, 2002).

ranchers. David Maybury-Lewis, the first academic researcher to spend an extended period of time in Xavante territories, arrived in 1958. Maybury-Lewis' classic structuralist study of Xavante society laid a foundation for future research in socio-cultural anthropology. In 1962 Maybury-Lewis returned in collaboration with human geneticists James V. Neel and Francisco M. Salzano. The biomedical researchers turned their attention to the Xavante for the same qualities that had attracted Maybury-Lewis—they valued what they perceived as the group's isolation and maintenance of a culturally uninterrupted lifestyle. With these two sets of research, the Xavante debuted in the anthropological and biomedical literature.

While the national context of the March to the West brought the Xavante to the attention of scholarly centers in Brazil, Europe, and the United States, it was longer traditions of scientific interest in peoples classified as Indigenous that made the Xavante compelling subjects for study. Ideas about Native people moved seamlessly from scientific writing into popular conceptions and back. Both in imaginaries of the Brazilian nation and in scientific work in the broader Western tradition, Indigenous peoples were consistently placed in the past, or outside of history.¹⁶ Beginning at least in the nineteenth-century, anthropologists conceptualized societies that they visited—particularly those they described with terms like pastoral, traditional, primitive, hunter-gatherer—as stable, isolated, and coherent entities, which with the proper methodology

¹⁶ The classic study on temporal othering is Johannes Fabian, *Time and the Other: How Anthropology Makes Its Object* (New York: Columbia University Press, 2002). Another foundational critique is Eric R. Wolf, *Europe and the People Without History* (Berkeley: University of California Press, 2010 [1982]). Also of relevance is Mary Louise Pratt's analysis of travel writing and the construction of Latin America in earlier narratives. See Mary Louise Pratt, *Imperial Eyes: Travel Writing and Transculturation* (New York: Routledge, 1992).

could be decoded to understand the basis of human character.¹⁷ The notion that these societies represented the past, what Johannes Fabian refers to as the “denial of coevalness,” was central to how scholars valorized them as sources of knowledge well into the mid-twentieth century.¹⁸

Temporal linking to the past has also been central to the conceptualization of Native peoples in Brazil, where ample discursive space accommodates the idea of Indigenous people as progenitors of the tri-partite racial body politic. Nineteenth-century “foundational fictions” celebrated idealized Indian ancestors who inevitably sacrificed themselves or their connections to their community in order to rescue the Portuguese and give birth to the *mestiço* (mixed-race) nation.¹⁹ The political salience of these portrayals persisted into the twentieth century when the Xavante became the focus of state

¹⁷ The literature critiquing these notions is extensive. For two particularly influential examples, see George W. Stocking, Jr., *Race, Culture, and Evolution: Essays in the History of Anthropology* (New York: Free Press, 1968), 110–122; Michel-Rolph Trouillot, “Anthropology and the Savage Slot: The Poetics and Politics of Otherness,” in *Recapturing Anthropology: Working in the Present*, ed. Richard Fox (Santa Fe: SAR Press, 1991), 17–44. For a discussion of the importance placed on isolation as a defining feature of populations for study in human biology, see Veronika Lipphardt, “‘Geographical Distribution Patterns of Various Genes’: Genetic Studies of Human Variation after 1945,” *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 47, Part A (September 2014): 50–61. Joanna Radin also discusses the prevalence and force of notions of “isolation” for biomedical and bioanthropological research: see, Joanna Radin, “Latent Life: Concepts and Practices of Human Tissue Preservation in the International Biological Program,” *Social Studies of Science* 43, no.4 (2013), 498. For a more general discussion, including attention to the technologies that made these approaches feasible and appealing, see Joanna Radin, *Life on Ice: A History of New Uses for Cold Blood* (Chicago: University of Chicago Press, 2017). Also, see Ricardo Ventura Santos, Susan Lindee, and Vanderlei Sebastião de Souza, “Varieties of the Primitive: Human Biological Diversity Studies in Cold War Brazil (1962–1970),” *American Anthropologist* 116, no. 4 (2014): 723–35. doi:10.1111/aman.12150.

¹⁸ Fabian, *Time and the Other*, 37.

¹⁹ Doris Sommer, *Foundational Fictions: The National Romances of Latin America* (Berkeley: University of California Press, 1993), 138–171. On the incorporation of Indigenous people into royal iconography in Brazil, see Lilia Moritz Schwarcz, *The Emperor’s Beard: Dom Pedro II and the Tropical Monarchy of Brazil*, trans John Gledson (New York: Hill and Wang, 2004[1998]), 105–107. Schwarcz makes a compelling argument that part of the emphasis on *indigenismo* under Dom Pedro II’s reign was initially a form of distancing the government from association with slavery and the African and Afro-Brazilian presence in the empire. For an interesting analysis of how “atemporality” has been applied also to black Brazilian communities, particularly the reified notion of Bahia, see Anadelia A. Romo, *Brazil’s Living Museum: Race, Reform, and Tradition in Bahia* (Chapel Hill: University of North Carolina Press, 2010).

propaganda; as Garfield has written, “As a vestige of primordial Brazil—a living archaeological relic—the Indian stood as a valuable symbol for a state whose political legitimacy rested on its nationalist developmental project.”²⁰ Tracy Devine Guzmán has traced these moments of celebration of the historic *índio* to the most intense periods of risk to Native communities, suggesting how the usurpation of Indigenous identity in national discourse can complement and cover acute threats to Native lives.²¹ In Brazil, especially prior to redemocratization in 1988, Indigenous peoples were generally accepted only in their historical role as progenitors of the mixed-race nation. Their concurrent existence as persisting, differentiated peoples with their own languages, practices, and territories has constituted a challenge to the dominant nation and national identity. This challenge has led experts and policy makers to repeatedly interpret Indigenous peoples as on a path to incorporation into the nation, whether through biological or cultural mixing.²² Popular and political representations were intertwined with practices of governance and knowledge production, and at each turn, scientific thinking about the place of Native peoples mirrored national concerns and trends.²³

²⁰ Garfield, *Indigenous Struggle*, 18–19.

²¹ Guzmán, *Native and National in Brazil*, 67–68.

²² On the positivist project of Marechal Rondon, the founding of the *Serviço de Proteção aos Índios* (Indian Protective Service, SPI), and the positivist project to incorporate the *índio* through modernizing projects, see Todd A. Diacon, *Stringing Together a Nation: Cândido Mariano Da Silva Rondon and the Construction of a Modern Brazil, 1906–1930* (Durham: Duke University Press, 2004), 79–99. On the late nineteenth and early twentieth-century practice of physical anthropology and notions of race in the work of Edgard Roquette-Pinto, see Ricardo Ventura Santos, “Guardian Angel on a Nation’s Path: Contexts and Trajectories of Physical Anthropology in Brazil in the Late Nineteenth and Early Twentieth Centuries,” *Current Anthropology* 53, no. S5 (2012): S17–32 and Vanderlei Sebastião de Souza, “Em busca do Brasil: Edgard Roquette-Pinto e o retrato antropológico brasileiro (1905–1935),” (PhD diss., Casa Oswaldo Cruz-FIOCRUZ, 2011). On discourses of acculturation, see Chapter 4.

²³ Santos, “Guardian Angel”; Nancy Leys Stepan, *The Hour of Eugenics: Race, Gender, and Nation in Latin America* (Ithaca: Cornell University Press, 1991). Ricardo Ventura Santos and colleagues divide the late nineteenth and early twentieth-century thinking about the racial identity in Brazil into two periods: first

Researchers from abroad, however, tended to see more value in the continued distinctiveness of Indigenous groups, whose geographical location also lent to their intrigue due to a long history of Amazonia being seen as a site of hardship and danger paired with marvels and curiosity.²⁴ In the mid-twentieth century, when the Xavante came into the sights of scholars interested in studying them, paradigms of salvage and preservation repositioned the so-called primitive both as a window into human past and a source of knowledge for the future.²⁵

Important changes would take place with the rise of pan-Indigenous activism, human rights discourse, and multiculturalism after Brazil redemocratized and passed the 1988 Constitution. During the intervening years, Xavante leaders became prominent actors on the national stage in Brazil. As historian Seth Garfield has shown, through astute political action, tenacity, and strategic performances of identity, Xavante actors challenged and shaped state policy.²⁶ During these years, from the 1970s to the present, Xavante villages also began hosting a consistent and increasingly constant stream of researchers. The trail of publications, the active academic lives of the earliest authors and the theoretical debates they entered attracted a wide range of scholarly attention.

However, it was not merely the prestige of early scholarship that would generate the

a deep pessimism regarding the value and future of Native and black people; followed by a more optimistic, positivist view of the potential of education, health and environmental interventions to improve the racialized body politic. See: Ricardo Ventura Santos, Michael Kent and Verlan Gaspar Neto “From Degeneration to Meeting Point” in Peter Wade, Carlos López Beltrán, Eduardo Restrepo, and Ricardo Ventura Santos, eds., *Mestizo Genomics: Race Mixture, Nation, and Science in Latin America* (Durham, Duke University Press, 2014). Santos, Kent, and Gaspar Neto add a third period, from 2000 to the present, which they describe as being defined by thinking about genetic mixture and the non-existence of genetic race. This schematic begs the question of what happened between these periods, a question of relevance to this study. Also, see Santos, Lindee, and de Souza, “Varieties of the Primitive.”

²⁴ Pratt, *Imperial Eyes*, 20.

²⁵ Radin, *Life on Ice*, especially 95–117.

²⁶ Garfield, *Indigenous Struggle*.

sustained interest of researchers. Nor was it simply a question of visibility on the Brazilian public stage. The Xavante came to be one of the most-studied Indigenous groups in South America not only because they were valued by the scientists, but also, as I show here, because they valued researchers.

Making Populations

Practitioners of the human sciences are concerned with understanding their subjects at a variety of levels or resolutions. In order to make generalizable knowledge, they tend to focus on groups of people, sometimes framed as populations, culture groups, or personality types. To make these generalizations they must study and interact with individuals and communities, who in turn participate in these processes. At the same time, they must cultivate their own professional and personal identities in order to wield the expertise to study others. This project examines research on the Xavante to address each of these processes. How were the Xavante made into a coherent and characterized population? How did they participate in the process, and thus influence the production of knowledge? Finally, how did these interactions reverberate back into the fields that studied the Xavante and onto the scientists doing the studying?

When Maybury-Lewis, Neel, Salzano, and collaborators arrived in Xavante territory in 1962, they had no doubt that the village that they were visiting was part of a coherent human group, defined by social *and* biological boundaries. And yet, even if the process was naturalized to them, their work constituted participation in a central aspect of

post-war human sciences. They were creating a population.²⁷ Population making resonates with broader questions of power and state authority,²⁸ with a wealth of scholarship exploring the rise of statistics and use of enumeration and forms to manage people.²⁹ But while the scientific process of making a population is wrought with the same forms of power and is often interrelated with state and colonial processes, it is also a practical and epistemological challenge that scholars face. Race, nation, geography, or language might seem and may be treated like natural kinds, but in collecting data and characterizations, academic scholars seldom encounter neat boundaries.³⁰ This is something researchers often willingly concede, and which only emphasizes how much work is necessary to make populations legible. This project follows Bangham and de

²⁷ Lisa Gannett, "Making Populations: Bounding Genes in Space and in Time," *Philosophy of Science* 70, no. 5 (2003): 989–1001, doi:10.1086/377383.

²⁸ Much work in this vein draws on Michel Foucault, particularly: Michel Foucault, "Governmentality" in *The Foucault Effect: Studies in Governmentality*, ed. Graham Burchell, Colin Gordon, and Peter Miller (Chicago: University of Chicago Press, 1991), 87–104 and Michel Foucault, *The History of Sexuality, Vol. 1: An Introduction* (New York: Vintage, 1990). Also critical to expanding Foucault's notions to consider colonial situations is the work of Ann Laura Stoler, including, *Race and the Education of Desire: Foucault's History of Sexuality and the Colonial Order of Things* (Durham: Duke University Press, 1995).

²⁹ For approaches focused on the rise of statistics: Ian Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990); Theodore M. Porter, *Trust in Numbers* (Princeton: Princeton University Press, 1995). On statistics and the state: Talal Asad, "Ethnographic Representation, Statistics, and Modern Power," in *From the Margins: Historical Anthropology and Its Futures*, ed. Brian Keith Axel (Durham: Duke University Press, 2002); Samuël Coghe and Alexandra Widmer, "Colonial Demography: Discourses, Rationalities, Methods," in *Twentieth Century Population Thinking: A Critical Reader of Primary Sources*, ed. Regula Argast, Corinna R. Unger, and Alexandra Widmer (New York: Routledge, 2015), 37–64.

³⁰ For example: On linguistics, Josh Berson, "The Dialectal Tribe and the Doctrine of Continuity," *Comparative Studies in Society and History* 56, no. 2 (2014): 381–418, doi:10.1017/S0010417514000085. On geography and race, see: Lisa Gannett and James R. Griesemer, "The ABO Blood Groups: Mapping the History and Geography of Genes in Homo Sapiens," in *Classical Genetic Research and Its Legacy: The Mapping Cultures of Twentieth-Century Genetics*, ed. Hans-Jörg Rheinberger and Jean-Paul Gaudillière (New York: Routledge, 2004), 117–72. On categorization of race as a site of political struggle in South Africa, and the technical and bureaucratic difficulties in assigning people to race categories in this context, see Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences* (Cambridge, MA: MIT Press, 2000), 195–225; on genetic research and ideas about the nation as a salient category for analysis in Latin America, see Peter Wade, Vivette García Deister, Michael Kent, María Fernanda Olarte Sierra, and Adriana Díaz del Castillo Hernández "Nation and the Absent Presence of Race in Latin American Genomics," *Current Anthropology* 55, no. 5 (2014): 497–522, doi:10.1086/677945.

Chadarevian's suggestion to "move populations center stage" in order to make sense of the human sciences.³¹

In centering the story on a specific population, this project provides a different picture of the political and social implications of research, specifically for the kinds of communities that are repeatedly studied. It contributes to a small but growing body of literature on the history of studies of human variation in the post-war period that looks beyond the persistence and transmutation of eugenic thinking.³² Likewise, it takes a different tack from the wealth of scholarship that has explored the tenacity of race as a salient and often-employed biological category in the mid and late twentieth century,³³ as well as from the particularly fruitful subfield of this literature that examines the genetic

³¹ Jenny Bangham and Soraya de Chadarevian, "Human Heredity after 1945: Moving Populations Centre Stage," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 47, Part A (September 2014): 45–49, doi:10.1016/j.shpsc.2014.05.005. Bangham and de Chadarevian write specifically about human heredity. I extend this to the human sciences more generally. My focus on population making is also informed by a workshop in December 2015 on "Populations of Cognition" organized by Edna Suárez-Díaz, Viviette García Deister, Ricardo Ventura Santos, and Alexandra M. Stern. A special issue of *Perspectives on Science*, currently under review, includes a series of articles that emerged from this workshop and take "population" as the object of study.

³² A classic study on the history of eugenics in the United States and the United Kingdom is Daniel Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (Cambridge, MA: Harvard University Press, 1998[1985]). Alexandra M. Stern draws attention to the many iterations of eugenics in a variety of understudied US geographies (with refreshing attention to the broader Americas) in *Eugenic Nation: Faults and Frontiers of Better Breeding in Modern America* (Berkeley: University of California Press, 2005), with chapters 5 and 6 focused on the post-war period. Recently Nathaniel Comfort has argued that the shift from a focus on human improvement to an emphasis on relief of suffering defined the rise of medical genetics, but the field continues to be an essentially eugenic project since these two foci were always present: Nathaniel Comfort, *The Science of Human Perfection: How Genes Became the Heart of American Medicine* (New Haven: Yale University Press, 2012).

³³ Much of this scholarship responds to Elazar Barkan's *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars* (Cambridge: Cambridge University Press, 1993). Other foundational texts include, Stocking, *Race, Culture, and Evolution*; and Nancy Leys Stepan, *The Idea of Race in Science: Great Britain, 1800–1960* (Hamden, Conn: Archon, 1982). The literature is extensive, but a few works that have been particularly influential to my thinking include: Troy Duster, *Backdoor to Eugenics* (New York: Routledge, 2003[1990]); Jonathan Marks, *What It Means to Be 98% Chimpanzee: Apes, People, and Their Genes* (Berkeley: University of California Press, 2003).

continuities and re-articulations of racial categories.³⁴ While broad discourses and histories of research on race and ethnicity underlay scholars' interest in studying the Xavante in the twentieth century, this project follows recent works that have shifted the focus to a broader conception of human variation.³⁵ This scholarship has opened new questions about the material basis for global networks of technoscience, the interactions of multiple disciplines, and the varied sources and forms of labor needed for these research programs.³⁶

The Cold War witnessed an unprecedented rise in the study of human populations through the examination of genetic material. Building on older technologies of linguistics and anthropometry, human geneticists pioneered population research through blood collection with the support of international organizations such as the World Health

³⁴ On the Human Genome Diversity Project, see Jenny Reardon, *Race to the Finish: Identity and Governance in an Age of Genomics* (Princeton: Princeton University Press, 2004), specifically 17–44 for a discussion of the history of expert discourse on race; Nadia Abu El-Haj makes the very necessary point that “race” is not a stable category, even if it has been used in some consistent ways, and discusses the rise of market-based thinking associated with late twentieth-century genetic notions of race in “The Genetic Reinscription of Race,” *Annual Review of Anthropology* 36, no. 1 (2007): 283–300, doi:10.1146/annurev.anthro.34.081804.120522. See also: Duana Fullwiley, “The Biological Construction of Race: ‘Admixture’ Technology and the New Genetic Medicine,” *Social Studies of Science* 38, no. 5 (2008): 695–735, doi:10.1177/0306312708090796; Barbara A. Koenig, Sandra S. Lee, and Sarah S. Richardson, eds. *Revisiting Race in a Genomic Age* (New Brunswick: Rutgers University Press, 2008); Ian Whitmarsh and David S. Jones, eds., *What’s the Use of Race?: Modern Governance and the Biology of Difference* (Cambridge, MA: MIT Press, 2010).

³⁵ Defining populations or approaching the study of populations can involve slippages between concepts such as race, ancestry, and heritage. For example, see Joan H. Fujimura, Ramya Rajagopalan, Pilar N. Ossorio, and Kjell A. Doksum, “Race and Ancestry: Operationalizing Populations in Human Genetic Variation Studies,” in Whitmarsh and Jones, *What’s the Use of Race?*, 169–86. On racialization of Native peoples and population making as a biopolitical project of settler-colonialism: Mark Rifkin, “Making Peoples into Populations: The Racial Limits of Tribal Sovereignty,” in *Theorizing Native Studies*, ed. Audra Simpson and Andrea Smith (Durham: Duke University Press, 2014), 149–87.

³⁶ Bangham and Chadarevian, “Human Heredity after 1945,” 45; Alexandra Widmer and Veronika Lipphardt, *Health and Difference: Rendering Human Variation in Colonial Engagements* (New York: Berghahn Books, 2016), 3–4.

Organization.³⁷ Field research with the Xavante served as a pilot study for this new regime of collection practices.³⁸ By foregrounding the collection and use of human tissues for the study of genetic variation, Joanna Radin's *Life on Ice* has uncovered the practical, technological, and ethical dilemmas of these new practices of biology. Focusing on the freezer, Radin has linked the discourses and field realities of "salvage biology" during the Cold War,³⁹ showing how tens of thousands of biosamples from Indigenous groups around the world came to rest in the suspended animation of cold storage. Radin's study has illuminated the extensive network of laboratories and scientists—primarily in the global north—that responded to anxieties about the nuclear present by researching populations they understood as both geographically and temporally isolated from their urban academic centers. *Studying Indigenous Brazil*, by concentrating specifically on the Xavante communities where this research was performed, traces its enduring influence at a local level. Much as the biological samples discussed by Radin fueled future techniques that had not even been imagined at the time of fieldwork, the data and experiences of research from the 1960s had a lasting impact on the local matrix of research that

³⁷ Soraya de Chadarevian, "Human Population Studies and the World Health Organization," *Dynamis* 35, no. 2 (2015): 359–88; Joanna Radin, "Unfolding Epidemiological Stories: How the WHO Made Frozen Blood into a Flexible Resource for the Future," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 47, Part A (September 2014): 62–73, doi:10.1016/j.shpsc.2014.05.007.

³⁸ Radin, *Life on Ice*. For Radin's discussion of research on the Xavante within Neel and Salzano's broader research trajectory, see Chapter 3. This research is also the topic of Chapters 2 and 3 of this dissertation.

³⁹ Salvage biology is Radin's term for the biological corollary of salvage anthropology, which functioned under many of the same priorities and assumptions. *Life on Ice*, 6. Rebecca Lemov has also argued that concern for loss of data sometimes outstripped concern for loss of forms of life: The *emotion* of salvage, in her account, was mostly directed at data. See Rebecca Lemov, "Anthropological Data in Danger, c. 1941–1965" in *Endangerment, Biodiversity and Culture*, ed. Fernando Vidal and Nélia Dias (New York: Routledge, 2015). Adrianna Link also discusses anthropological salvage as a mid-twentieth-century sensibility: Adrianna Link, "Salvaging a Record for Humankind: Urgent Anthropology at the Smithsonian Institution, 1964–1984," (PhD diss., Johns Hopkins University, 2016).

developed in Xavante territories, influencing both researchers and research subjects in unexpected ways.

A focus on populations also helps unsettle disciplinary histories, elucidating close relationships between academics with distinct trainings, and rendering a clearer picture of the colonial and neocolonial drive for complete documentation.⁴⁰ Medical anthropology fieldwork was made possible in collaboration with linguists and thanks to the infrastructure of socio-cultural anthropology in 1950s Mexico; populations were illegible without these supports.⁴¹ To understand genetic disease in Amish communities in the 1960s, Susan Lindee has shown how biomedical researchers had to employ history, sociology, and anthropology.⁴² A wide network of physicians, anthropologists, government agents, and demographers collected knowledge about human difference in colonial regimes to facilitate the administration of subject populations, especially in regards to labor, nutrition, and reproduction.⁴³ Rebecca Lemov has explored how large-scale field expeditions to the South Pacific sought to comprehensively characterize populations, omnivorously incorporating data produced by a cadre of social scientists—everything from psychological test results to anthropometric measurements to a catalogue

⁴⁰ This also follows Hans-Jörg Rheinberger's call to look beyond a "more or less well-defined disciplinary matrix of twentieth-century biology" to trace epistemic things that move through and beyond single fields or subfields. Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford: Stanford University Press, 1997), 34.

⁴¹ Edna Suárez-Díaz, "Indigenous Populations in Mexico: Medical Anthropology in the Work of Ruben Lisker in the 1960s," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 47, Part A (September 2014): 108–17. Of relevance here, too, is Suárez-Díaz's contention that connected (as opposed to comparative) histories of these international projects of population-making can shed light on "the ways in which people, materials, and tools travel, and on the practices that make national boundaries selectively permeable and transnational histories possible" (ibid., 108).

⁴² Lindee, *Moments of Truth*, 58–89.

⁴³ Widmer and Lipphardt, *Health and Difference*, 2.

of dreams—in a frenzy of salvage and archiving.⁴⁴ What the Xavante story has to add to these larger explorations of post-war population making is the possibility to trace the after-effects of these exhaustive and interdisciplinary projects.⁴⁵ Early research programs that aspired to comprehensiveness had lasting influences on how scholars would understand the Xavante.

As the Xavante became a population with average blood pressure, gene frequencies, kinship structures, and political factions, they also became increasingly legible for future researchers. The layers of knowledge produced about the Xavante both enabled and were enabled by interdisciplinary work. The community that first hosted Maybury-Lewis and the first genetics field season would later receive anthropologist Nancy Flowers and her doctoral study in human ecology.⁴⁶ These data would be taken up again in the 1990s for a series of comparative studies, producing one of the earliest and most comprehensive diachronic inquiries into Indigenous health in lowland South

⁴⁴ Rebecca Lemov, “X-Rays of Inner Worlds: The Mid-Twentieth-Century American Projective Test Movement,” *Journal of the History of the Behavioral Sciences* 47, no. 3 (2011): 251–278. On the remarkable history of dream collecting, see Rebecca Lemov, *Database of Dreams: The Lost Quest to Catalog Humanity* (New Haven: Yale University Press, 2015). Another compelling example of these interdisciplinary population-oriented projects is the Vicos project. See: Jason Pribilsky, “Development and the ‘Indian Problem’ in the Cold War Andes: Indigenismo, Science, and Modernization in the Making of the Cornell-Peru Project at Vicos,” *Diplomatic History* 33, no. 3 (2009): 405–26, doi:10.1111/j.1467-7709.2009.00778.x.

⁴⁵ For work that centers knowledge production across disciplinary boundaries in the bio-geological sciences, see Paul N. Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (Cambridge, MA: MIT Press, 2010) and Stefan Helmreich, *Alien Ocean: Anthropological Voyages in Microbial Seas* (Berkeley: University of California Press, 2009). On discourses of interdisciplinarity in the Cold War, and the funding structures that accompanied them, see Jamie Cohen-Cole, *The Open Mind: Cold War Politics and the Sciences of Human Nature* (Chicago: University of Chicago Press, 2014).

⁴⁶ Nancy M. Flowers, “Forager Farmers: The Xavante Indians of Central Brazil,” (PhD diss., City University of New York, 1983).

America.⁴⁷ Public health researchers Carlos Coimbra Jr. and Ricardo Ventura Santos continued to build productive working relationships in the village, and gradually began taking and sending students to the field. By 2000, T.I. Pimentel Barbosa, where they worked, became a veritable field school for scholars of public health. The population might even have become a kind of “experimental system,” one which is “designed to give unknown answers to questions that the experimenters themselves are not yet able clearly to ask;” a “[vehicle] for materializing questions.”⁴⁸ The long-term and layered study of a population itself creates a kind of path-dependency. The Xavante case thus raises another set of questions: When a population is made into an enduring site of research, what questions are foreclosed? And how do the characteristics and opinions of the population cogenerate the phenomena under study?

Making the Xavante Up

These questions drive the second major contribution of the project. By focusing on the daily interactions of fieldwork, *Studying Indigenous Brazil* documents not only how experts have come to study and understand the particularities and generalizable qualities of the Xavante, but also how over time, Xavante individuals and communities have, to varying degrees, helped make this knowledge.⁴⁹ In asking these questions, I draw

⁴⁷ Carlos E. A. Coimbra Jr., Nancy M. Flowers, Francisco M. Salzano, and Ricardo V. Santos, *The Xavante in Transition: Health, Ecology, and Bioanthropology in Central Brazil* (Ann Arbor: University of Michigan Press, 2004).

⁴⁸ Rheinberger, *Toward a History of Epistemic Things*, 28. There are essential differences in studying proteins and studying people, and research in Xavante territory has tended to be observational rather than experimental. Yet I think Rheinberger’s notion still offers insight.

⁴⁹ This relates to a much broader literature on how local informants, guides, and intellectuals in Latin America have participated in knowledge production about populations and spaces, both contributing to

on the work of science studies scholars, who have inquired into the dynamic relationship between the processes and objects of knowledge making in a wide variety of ways.⁵⁰ Classification, categorization, and naming have been of particular interest due to their epistemological and political power.⁵¹ Of relevance for this project is the way knowledge and power interface in the definition of human categories and human experience.⁵² Being categorized as “Xavante” or as “Indigenous” has phenomenological, political, and material implications. As Xavante interlocutors observed their anthropologists recording “Xavante culture,” they developed new ideas about the value and performance of particular aspects of their identity. Categorizations can be oppressive and productive, disciplinarian and redemptive, sometimes both at the same time. They both reflect and inform social organization; for example, when the medical establishment accepts a new

knowledge in colonial centers and innovating to create new interpretations, and challenging existing epistemologies. For example, see: Jorge Cañizares-Esguerra, *Nature, Empire, and Nation: Explorations of the History of Science in the Iberian World* (Stanford: Stanford University Press, 2006); Gabriela Soto Laveaga, *Jungle Laboratories*; Medina, Marques, and Holmes, eds. *Beyond Imported Magic*; Neil Safier, *Measuring the New World: Enlightenment Science and South America* (Chicago: University Of Chicago Press, 2008).

⁵⁰ On protein synthesis, Rheinberger, *Toward a History of Epistemic Things*. On the creation of PCR and changing meanings of genes: Paul Rabinow, *Making PCR: A Story of Biotechnology* (Chicago: University of Chicago Press, 1997). Rabinow comments, “the truly astonishing thing about PCR is precisely that it wasn't designed to solve the problem; once it existed, problems began to emerge to which it could be applied” (7). Duana Fullwiley’s study of research and clinical practice in Senegal provides a compelling analysis of how “culture” and “biology” interact to form knowledge of the HbS haplotype for sickle cell trait: Duana Fullwiley, *The Enculturated Gene: Sickle Cell Health Politics and Biological Difference in West Africa* (Princeton: Princeton University Press, 2011).

⁵¹ On the prevalence of the classificatory project underlying perception in Western thought, see Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (New York: Vintage, 1994 [1966]). As Geoffrey Bowker and Susan Leigh Star have shown, the practices of classification are so integrated into daily life—at least in urban spaces of countries like the United States and Brazil—as to become invisible, built into “the information environment.” This categorization and the concurrent development of standards are inescapable, and not necessarily negative; they can be very powerful tools for making sense of the world and organizing it. However, as Bowker and Star emphasized, “each category valorizes some point of view and silences another,” making choices about classification inherently moral and ethical. Bowker and Star, *Sorting Things Out*, 5.

⁵² Much scholarship on the knowledge/power is foundationally informed by Michel Foucault’s explorations of genealogies of social control. See especially, Foucault, *Discipline & Punish*; Foucault, *History of Sexuality, Vol. 1*.

disease category, sufferers may gain social recognition.⁵³ A diagnosis can offer legitimacy,⁵⁴ or alternately pathologize not only a patient but also others who share aspects of their social identity.⁵⁵ The legal status of “Indigenous” and the political potency of performing certain kinds of Indigeneity simultaneously bear privileges and burdens.

Ian Hacking considered the generative process of naming through the lens of “making up people.” Drawing on examples of multiple personality disorder, homosexuality, and suicide, Hacking has argued that these human kinds and human acts “come into being hand in hand with our invention of the ways to name them.”⁵⁶ This relationship between the articulation and inhabiting of a social category has opened up investigation into the dynamic relationship between the self and the collective.⁵⁷ This relates to Hacking’s discussion of a “looping effect” in the construction of human kinds. “To create new ways of classifying people is also to change how we can think of ourselves, to change our sense of self-worth, even how we remember our own past,” he wrote, “This in turn generates a looping effect, because people of the kind behave

⁵³ Charles E. Rosenberg, “Framing Disease: Illness, Society, and History,” in *Framing Disease: Studies in Cultural History*, ed. Charles E. Rosenberg and Janet L. Golden (New Brunswick: Rutgers University Press, 1992), xiii–xxvi. As Rosenberg writes, “agreed etiologies at once incorporate and sanction a society’s fundamental ways of organizing its world” (ibid., xiv).

⁵⁴ Robert A. Aronowitz, *Making Sense of Illness: Science, Society and Disease* (Cambridge: Cambridge University Press, 1998).

⁵⁵ Jonathan Metzl, *The Protest Psychosis: How Schizophrenia Became a Black Disease* (Boston: Beacon Press, 2009).

⁵⁶ Hacking, “Making Up People,” 113.

⁵⁷ An interesting example that emphasizes the individual and collective aspects is Bert Hansen’s suggestion that the emergence of the category of “homosexual” in the nineteenth-century not only created a medical and identity category out of something that had previously been seen as a deviant act or sin, it also facilitated new self-knowledge and made it possible for people who fell within this category to find others like them. See: Bert Hansen, “American Physicians’ ‘Discovery’ of Homosexuals, 1880–1900: A New Diagnosis in a Changing Society” in Rosenberg and Golden, *Framing Disease*, 104–33.

differently and so are different.”⁵⁸ *Studying Indigenous Brazil*, by focusing on the daily interactions of fieldwork, documents how iterative experiences of being studied interfaced with contact with settler society and fed back into Xavante villagers’ sense of their public presence and meaning on the national and international stage.

Prior to colonization, one might say that the “Xavante” did not exist as such. Self-denominated as *A’uwe* or *A’uwe uptabi*, “the true people,” they only became Xavante in the mid-eighteenth and early nineteenth-century. Colonial mapmakers and administrators cemented this new name in official documents as the Xavante were brought together with other Native groups under early systems of governance in what would become Central Brazil.⁵⁹ Labeled with this new title, the Xavante maintained relations with the *warazú* only for a short time. Soon they retreated, moving west to avoid continued contact. The name was stabilized, but the meaning of “Xavante,” both for those who applied it to the

⁵⁸ Ian Hacking, “The Looping Effects of Human Kinds,” in *Causal Cognition: A Multi-Disciplinary Debate*, ed. Dan Sperber, David Premack, and Ann James Premack (New York: Oxford University Press, 1995), 369. Hacking draws a distinction between the definition of a group for practical purposes of intervention, and for purely academic or aesthetic reasons. Presumably, he posits, social scientists create “adolescent pregnancy” because they wish to change present realities; he contrasts this with the abstract study of kinship systems or grammatical forms (*ibid.*, 360–361). I would counter that social scientists almost always have justified their work as producing useful knowledge, and as Audra Simpson and others have shown and I discuss below, the abstract study of kinship can have very consequential implications for the peoples that it documents. See: Audra Simpson, *Mohawk Interruptus: Political Life Across the Borders of Settler States* (Durham: Duke University Press, 2014).

⁵⁹ In colonial Goiás, Xavante contact with the *warazú* was conflictual, punctuated by land invasions and violent resistance. Gold miners and missionaries came in waves. During a period of approximately twenty-five years in the late eighteenth-century, colonial authorities successfully convinced or coerced a group of Xavante to settle in official government *aldeias* (villages). This was part of a broader program to concentrate Indigenous people in small regions, and free up land for agriculture and fishing. After acquiescing for a quarter of a century, the Xavante then rejected ongoing settlement, spending a number of years migrating through the region of Goiás, before crossing the Araguaia river and settling in the area of the *Serra do Roncador*, where they would be mostly free from *warazú* interference until the twentieth-century. This early history is discussed at length in Oswaldo Martins Ravagnani, *A experiência Xavante com o mundo dos brancos* (Araraquara: UNESP, 1991), 17–87; See also, Aracy Lopes da Silva, “Dois séculos e meio de história Xavante,” in *História dos índios no Brasil*, ed. Manuela Carneiro da Cunha (São Paulo: Companhia das Letras, 1992), 362–367. Garfield provides a more detailed overview of this history in *Indigenous Struggle*, 3–6.

A'uwe and for the *A'uwe* who apply it to themselves, continued to evolve. This dissertation argues that one of the key factors contributing to its evolution has been the process of research.

Scholarship on social movements makes clear that “people of the kind” can adopt newly available categories with purpose. According to Ronald Niezen’s analysis, in some cases Indigenous identity has “been taken control of by its living subjects—reverse-engineered, rearticulated, and put to use as a tool of liberation.”⁶⁰ As a category, “Indigenous” is first a product of the colonial encounter (*indigena*, in both Spanish and Portuguese),⁶¹ and more recently a re-articulated global category in the context of human rights and humanitarianism.⁶² As Indigenous identity has become a recognized form in political engagements with the state, Native people in Brazil perform their identity in new ways, sometimes in manners that coincide with a set of Western ideas about authenticity.

⁶⁰ Ronald Niezen, *The Origins of Indigenism: Human Rights and the Politics of Identity* (Berkeley: University of California Press, 2003), 221. Another hopeful interpretation of Indigenous movements in Latin America is Alison Brysk, *From Tribal Village to Global Village: Indian Rights and International Relations in Latin America* (Stanford: Stanford University Press, 2000).

⁶¹ Irene Silverblatt suggests convincingly that the counterpart of the construction of “*indigena*,” in the sixteenth-century viceroyalty of Peru was a new notion of what it meant to be Spanish; the classification of *indio* and the bureaucratization of governance through these new categories marked a radical change in governmentality. See: Irene Marsha Silverblatt, *Modern Inquisitions: Peru and the Colonial Origins of the Civilized World* (Durham: Duke University Press, 2004).

⁶² Niezen, *Origins of Indigenism*. As a result of this colonial and globalized history, “Indigenous” is a second-order identity category: Individuals almost always first identify as Xavante, Kayapó, Quechua, or Masaai, and self-identify as Indigenous second. Scholars debate when exactly to “date” the emergence of a global or transnational category of “Indigenous.” For example, Audra Simpson posits the League of Nations 1919 *Covenant of the League of Nations* as the legal constitution the category (Simpson, *Mohawk Interruptus*, 218). Niezen’s temporalization does elide, a longer history of Native organizing in the Americas, as Shane Greene points out. See: *Customizing Indigeneity: Paths to a Visionary Politics in Peru* (Stanford: Stanford University Press, 2009), 14.

This may be simultaneously due to political assertiveness,⁶³ and in reaction to external pressures to present a particular kind of self-image.⁶⁴

Some efforts to inhabit and mobilize the human category of the “Indigenous” are more successful than others, with a great deal depending on political context. Dorothy Hodgson has explored Maasai activists’ and organizations’ efforts to protect their way of life by “positioning” themselves within the Tanzanian state first as Indigenous (with the support of international organizations), and subsequently as pastoralists (when they failed to win state recognition).⁶⁵ Shane Greene’s ethnographic study has examined how the Aguaruna “customize” their articulations of indigeneity to fit their needs within the restrictions of the Peruvian context.⁶⁶ Both cases highlight constraints and creativity: neither group was free to “create itself” as it pleased. Rather, individual actors and community groups with diverse interests had to test and accommodate different options in dialogue with outside actors from the state to NGOs to missionaries to other local people. The two authors both emphasize the contingent nature of boundary constructions, emphasizing the dialectical relationship with market forces, nationalist ideology, and the involvement of international organizations.

⁶³ Terence Turner, “Representing, Resisting, Rethinking: Historical Transformations of Kayapo Culture and Anthropological Consciousness,” in *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*, ed. George W. Stocking Jr. (Madison: University of Wisconsin Press, 1993), 292.

⁶⁴ Beth A. Conklin, “Body Paint, Feathers, and VCRs: Aesthetics and Authenticity in Amazonian Activism,” *American Ethnologist* 24, no. 4 (1997): 711. For a series of discussions on performances of Indigeneities, see: Laura R. Graham and H. Glenn Penny, eds. *Performing Indigeneity: Global Histories and Contemporary Experiences* (Lincoln: University of Nebraska Press, 2014).

⁶⁵ Dorothy Louise Hodgson, *Being Maasai, Becoming Indigenous: Postcolonial Politics in a Neoliberal World* (Bloomington: Indiana University Press, 2011).

⁶⁶ Greene, *Customizing Indigeneity*.

In this project, I am likewise interested in the changing nature of being Xavante. I pay particular attention to the role of research interactions in informing outsiders' understandings of these meanings and recursively shaping the ideas of the local community members who interacted with them. Like the Aguaruna and Maasai, Xavante individuals and communities are constantly interfacing with outsiders other than researchers—government agents, tourists, missionaries, artists and musicians, and NGOs. But researchers have been uniquely important actors in these boundary-making processes because of the authority ascribed to academic knowledge, and due to the peculiar nature of academic fieldwork which often has involved serious, long-term engagement, and an ambiguous or even oppositional relationship with local governmental agents.

This is true for other groups as well. Terence Turner emphasized the impact of research in Kayapó communities, also in Central Brazil, where the fact that “a series of anthropologists, photographers, ethnozoologists, ethnomusicologists, museum collectors, journalists, cinematographers, and others” were willing to spend their resources and time studying their “cultures” conveyed “the awareness that their traditional way of life and ideas were phenomena of great value and interest in the eyes of at least some sectors of the alien enveloping society.”⁶⁷ Jan Hoffman French's comparative study of two neighboring communities, one legally recognized as a Xocó Indigenous community and the other as a *quilombo* (community descended from escaped slaves), offers a particularly nuanced discussion of the interrelated nature of legal openings for recognition and

⁶⁷ Turner, “Representing, Resisting, Rethinking,” 300–301. See also, Alcida Rita Ramos, “Do engajamento ao desprendimento,” *Campos* 8, no. 1 (2007): 11–32.

articulations of identity.⁶⁸ As in the Kayapó case, for the Xocó community she describes, outsiders—Catholic priests, anthropologists, NGO employees, and lawyers—engaged with people as they wove new identities out of existing experiences and political context.

Understanding how the Xavante participate in the process of making themselves up not only offers insight into how people come to understand themselves within identity categories by highlighting how they participate in forming, stretching, and consolidating them. It also complicates simplistic narratives about resistance and agency of research subjects in the human sciences. While there is ample scholarship that suggests people mobilize identity categories that become available to them, there are few studies that document how individuals and communities come to understand themselves within these changing frames,⁶⁹ or how exactly they participate in forming, stretching, and consolidating them. In other words, while historians of science have looked in depth at how experts make up their subjects, and historians of social movements have looked at how marginalized people occupy categories in a struggle for recognition from the state, there is little scholarship that documents how people come to understand themselves within these identity categories. How do subjects participate in the science of making themselves up?

⁶⁸ Jan Hoffman French, *Legalizing Identities: Becoming Black or Indian in Brazil's Northeast* (Chapel Hill: University of North Carolina Press, 2009).

⁶⁹ French's *Legalizing Identities* is an exception. Her study is particularly strong in taking seriously the lived reality and performance of identity of community members beyond an instrumentalist interpretation.

Opportunities and Double Binds

Subjects may participate in “making up” themselves or choose to otherwise engage bioscientific and social scientific knowledge production for a variety of reasons. Part of the impetus for Xavante communities to acquiesce to the earliest visits by researchers may have been due primarily to the inevitable gifts of material goods that came with any visit from outsiders.⁷⁰ But as this dissertation shows, research relationships and the gift exchanges within them were not simple instrumental engagements. The political context of settler colonialism and constant threats to Xavante lands and lives permeated these interactions, as well as Xavante demands for their scientific interlocutors to ally in action. Scarcity and precarity can compel subjects to form and reform themselves within externally sanctioned categories out of material and political need. This complicates the ethical and moral terrain of research and care.

In her study on post-Chernobyl Ukraine, anthropologist Adriana Petryna has shown how claiming the status as a sufferer of radiation exposure became a means of survival in a moment of political and economic crisis. State recognition, however, was partial and inconsistent, contingent on evidence and diagnoses that could not be easily attained. Petryna’s study makes clear that, with differing levels of facility and success, citizens engage the social utility of science to make claims on the state.⁷¹ Although the

⁷⁰ The question of material exchange is an essential one, and is discussed at more length in Chapters 1 and 5. A particularly insightful analysis of how anthropologists and other outsiders are upset by the perceived materialism of Indigenous people who emphasize gift giving is Beth Conklin’s “For Love or Money: Indigenous Materialism and Humanitarian Agendas,” in *Editing Eden: A Reconsideration of Identity, Politics, and Place in Amazonia*, ed. Frank Hutchins and Patrick C. Wilson (Lincoln: University of Nebraska Press, 2010), 127–150.

⁷¹ Adriana Petryna, *Life Exposed: Biological Citizens after Chernobyl* (Princeton: Princeton University Press, 2002).

institutions, vulnerabilities, and socio-political realities of Native groups in Brazil differ widely from those of sufferers in post-Soviet Ukraine, a shared logic prevails. As Judith Butler suggests, a critical examination of whose “precariousness” counts is vital in questioning how “the other” is created and accepted.⁷² The Brazilian state requires the documentation of existential threats, whether biological or cultural, in adjudicating territorial or other political demands; this proof of authenticity and suffering has often been most successfully mediated by expert interlocutors, a fact that speaks to the privileging of some knowledges over others.

The push for inclusion in research regimes can also be a strategy to compel the production of certain kinds of knowledge that will be of use to a community. In response to the unequal attributions of authority, Xavante communities have cultivated certain kinds of research and scholarship that will help protect their land and traditional practices—such as fire hunting—within a system that values ecologists’ publications over elders’ traditional knowledge. In this sense some Xavante leaders are like patients and activists who have advocated for their involvement in studies on the basis of human or health rights. These forms of engagement upset clear binaries between subjects and knowledge producers. In the case of early HIV/AIDS research, for example, Steven Epstein has documented how over time activists constructed their own credibility as lay experts, eventually becoming genuine participants in the construction of scientific knowledge, as well as claiming the *right* to participate in research as subjects.⁷³ The

⁷² Judith Butler, *Frames of War: When is Life Grievable?* (London: Verso, 2009), 39.

⁷³ Steven Epstein, *Impure Science: AIDS, Activism, and the Politics of Knowledge* (Berkeley: University of California Press, 1996).

relative affluence and political connections of these activists enabled them to transmit urgency and a sense of existential threat to scientific researchers, while their relationship to scientists in turn shaped their understanding of the limits of pharmaceutical research. Facing a different set of challenges, the Xavante similarly sought to shape researchers' agendas within a context of scarcity. This dissertation highlights how incentives for participation have changed over time and how the Xavante have become not only lay experts on their own "condition" but experts in interacting with and directing the kind of *warazú* who want to study them.

These kinds of fights for inclusion in research can have unintended consequences, especially by reifying regimes of difference. As Karen Engle has shown, the rise of the discourse of "right to culture" for Indigenous people has had mixed results, particularly in Latin America, where states sometimes readily adopted the extension of cultural rights, but refused or even dismantled systems for socio-economic redistribution and land reform.⁷⁴ Structural issues are left unattended. Additionally, culture has to be demonstrated in the "correct" ways for the liberal multicultural state to extend recognition, what Elizabeth Povinelli referred to as the "invisible asterisk."⁷⁵ As knowledge about a particular Indigenous group builds up, it also risks becoming hegemonic even if deeply flawed, maintaining the settler state's "monopoly on the

⁷⁴ Karen Engle, *The Elusive Promise of Indigenous Development: Rights, Culture, Strategy* (Durham: Duke University Press, 2010).

⁷⁵ Elizabeth A. Povinelli, *The Cunning of Recognition: Indigenous Alterities and the Making of Australian Multiculturalism* (Durham: Duke University Press, 2002), 12 and 173–185. In this Povinelli highlights, among other things, how the moral and rational conflict producing moment of deep incongruence or panic as subjects face their conflicting sentiments.

legitimate exercise of legitimacy.”⁷⁶ Audra Simpson’s work analyzes a particularly potent example of this. She has explored how a body of literature produced in the nineteenth-century and early twentieth century—what she refers to as the “Iroquois canon”—haunts communities in the present, used as a measure for what Haudenosaunee (Iroquois) people should be like, with serious implications for questions of sovereignty.⁷⁷ Xavante engagement with researchers must be seen within similar circumstances. On the one hand they choose and are forced to seek representation through the researchers who study them, and on the other hand the representations that are produced can reinforce narrowly bounded notions of authenticity or produce barriers to self-determination. In this context of precarity, some Indigenous groups in Brazil and beyond have rejected the presence of outside researchers. The Xavante have responded instead by working to build relationships that will be enduring. Their efforts to establish mutual recognition involve the affective labor of care.

Labor, Affect, and Making Scientists

In 1980, Brazilian anthropologist Aracy Lopes da Silva reflected in her doctoral dissertation that an introduction ought to be “something that documents an intensive process of learning corresponding to a transformation suffered by its author.” Having recently completed a study on Xavante friendship and naming practices, she went on to say that particularly for a first project in anthropology, “this transformation is, without doubt, profound due to the discovery of the experience and daily exercise of alterity and

⁷⁶ Rifkin, “Making Peoples into Populations,” 91.

⁷⁷ On the Iroquois canon, see Simpson, *Mohawk Interruptus*, chapter 3.

for the human and political commitment it demands of the researcher.”⁷⁸ *Studying Indigenous Brazil* takes seriously Lopes da Silva’s portrayal of how the research experience changed her. I explore how the Xavante labor that made possible the characterization of their “population” or “culture” also served to ensnare and shape the scholars, some of whom came to think of themselves as “*xavantólogos*” or “studiers of the Xavante.”⁷⁹

People make sense of their research experiences in the human sciences through face-to-face interaction. This is particularly salient in the context of projects where the researchers stay with or live as a part of a community. Geissler et al. described these relationships as “premised not upon detachment but familiarity,” where mutual expressions of need mean that research relations take on “ontological character in that the knowledge of another person resides in being with him, unfolding in an open process.”⁸⁰ As research relations unfold, communities make demands that can, with varying degrees of success, influence the knowledge and change the people who are making it. Warwick Anderson’s study of research with Fore subjects in mid-twentieth century Papua New Guinea shows how scholars had to accrue social relations in order to access tissue samples for the study of the neurodegenerative disease kuru. As they hosted a stream of

⁷⁸ Aracy Lopes da Silva, “Nomes e amigos: da prática Xavante a uma reflexão sobre os Jê,” (PhD diss., Universidade de São Paulo, 1980), 25.

⁷⁹ In a fax to anthropologist Lux Vidal at the *Universidade de São Paulo* in 2000, David Maybury-Lewis used the term “*xavantólogos*” to apply to what he called a small group of researchers who studied the Xavante. His note was in reference to his colleague, friend, and postdoctoral mentee, Aracy Lopes da Silva, sending his condolences after her untimely death at the young age of 54. See Lux Boelitz Vidal, “Maria Aracy de Pádua Lopes da Silva (1949–2000),” *Revista de Antropologia* 43, no. 2 (2000): 15.

⁸⁰ P. Wenzel Geissler, Ann Kelly, Babatunde Imoukhuede, and Robert Pool, “‘He Is Now Like a Brother, I Can Even Give Him Some Blood’: Relational Ethics and Material Exchanges in a Malaria Vaccine ‘Trial Community’ in the Gambia,” *Social Science & Medicine* 67, no. 5 (2008): 702, doi:10.1016/j.socscimed.2008.02.004.

anthropologists, colonial administrators, and scientists, the Fore “kept making claims on the investigators as persons, entangling them in local communities and sometimes managing to transform the white man in the process.”⁸¹ This process of attempting to “transform the white man” took work.

Studying Indigenous Brazil draws on two decades of histories that have explored the classed and gendered nature of labor and attributions of scientific authority in the field sciences. These histories have documented the invisibilized labor of Native guides and scientists’ wives, and demonstrated how local artists, collectors, and research assistants shaped the formation of academic fields in the metropole.⁸² The case of the Xavante enriches this literature by examining how affective and caring labor of Indigenous subjects not only shaped knowledge, but shaped the subjectivities of the scholars committed to its creation.⁸³ Examining research in the Xavante territory of

⁸¹ Warwick Anderson, *The Collectors of Lost Souls: Turning Kuru Scientists into Whitemen* (Baltimore: Johns Hopkins University Press, 2008), 4.

⁸² On Native and Indigenous informants, see Margaret M. Bruchac, “My Sisters Will Not Speak: Boas, Hunt, and the Ethnographic Silencing of First Nations Women,” *Curator: The Museum Journal* 57, no. 2 (2014): 153–71, and Jane R. Camerini, “Wallace in the Field,” *Osiris*, 11 (1996): 44–65. On women and wives in the field, see Andrew Bank, “The Making of a Woman Anthropologist: Monica Hunter at Girton College, Cambridge, 1927–1930,” *African Studies* 68, no. 1 (2009): 29–56; Mariza Corrêa, *Antropólogos e antropologia* (Belo Horizonte: Editora UFMG, 2003); and Alex Soojung-Kim Pang, “Gender, Culture, and Astrophysical Fieldwork: Elizabeth Campbell and the Lick Observatory–Crocker Eclipse Expeditions,” *Osiris*, 11 (1996): 17–43. On illustrators, see Daniela Bleichmar, *Visible Empire: Botanical Expeditions and Visual Culture in the Hispanic Enlightenment* (Chicago: University of Chicago Press, 2012). On collectors: Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: University of Chicago Press, 2010). Lyn Schumaker’s insightful study on local informants underscores how influential field assistants are in the shaping of anthropological knowledge. See Schumaker, *Africanizing Anthropology: Fieldwork, Networks, and the Making of Cultural Knowledge in Central Africa* (Durham: Duke University Press, 2001).

⁸³ On affect and epistemology, see: Donna J. Haraway, *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1990); Alison M. Jaggar, “Love and Knowledge: Emotion in Feminist Epistemology,” in *Gender/Body/Knowledge: Feminist Reconstructions of Being and Knowing*, ed. Alison M. Jaggar and Susan R. Bordo (New Brunswick: Rutgers University Press, 1987). In using the term “affective labor” I follow Michael Hardt’s usage, including: Michael Hardt, “Affective Labor,” *Boundary 2* 26, no. 2 (1999): 89–100.

Pimentel Barbosa, it is clear that the field site became an affective field, a location where researchers were incorporated into kinship structures and subjects worked to compel their researchers into a sociality of knowledge. Constructing the researcher as *xavantólogo* required a process of enrolling or enchanting the *warazú*, creating social and emotional ties that would compel scientists and scholars to see the social relations inherent in epistemologies.⁸⁴

Troubling the Subject-Researcher Dyad

As I write about these histories of human interaction and the products they engender, I use the terms “researcher” and “subject” even as I seek to unsettle this dichotomy by showing their interrelated nature. Kim TallBear has called this binary the “crippling disease... [of] knowledge production” and written, “This is a fundamental condition of our academic body politic that has only recently been pathologized...”⁸⁵ For knowledge to serve all involved in its production, she has suggested, “we must soften that boundary erected long ago between those who know versus those from whom the raw materials of knowledge production are extracted.”⁸⁶ This study examines both how that boundary was erected, and how it has morphed, changed, and in some cases softened over time. I maintain these terms, however, because the uneven power dynamics that have characterized research roles persist, and in both my own work and the work of some of the scholars I study I see ample room to continue this softening. I also use terms such

⁸⁴ Margaret Kovach, *Indigenous Methodologies*, 21.

⁸⁵ Kim TallBear, “Standing With and Speaking as Faith: A Feminist-Indigenous Approach to Inquiry,” *Journal of Research Practice* 10, no. 2 (2014): 2.

⁸⁶ *Ibid.*

as interlocutor or informant, which have their own histories.⁸⁷ My tendency to center the term “subject” is due to my interest in understanding how research produces subjectivities, and how the dynamic terrains of knowledge production inform social position and experience.

This project focuses on the interaction of researcher and researched, and in so doing centers Xavante subjects who have been studied again and again. Like a long genealogy of scholarship that places people in reified subject categories, this focus risks contributing to the sense of fundamental otherness or exotification that has been so often and productively critiqued.⁸⁸ The conceptual Self/Other divide has structured the colonial encounter as well as the anthropological-scientific encounter (although they can never be disaggregated) and so is doubly present throughout this history. However, rather than taking this divide to be a pre-existing reality, a “generative opposition between ethnographic Self and native Other” that allows for knowledge production through the interface of researcher and research subject, I attempt to explore the “historical production and ethnographic reproduction” of this very cultural boundary.⁸⁹ How, in other words did the Xavante become the Xavante in their interactions with *warazú*? And how did becoming *warazú* change the researchers who profess social and natural science as their vocation? This approach, which Matti Bunzl has characterized as part of a neo-

⁸⁷ Edward Said discusses the term “interlocutor” and its connotations under colonial rule as either compliant to the colonial regime or stoically silent in the face of it. He also describes the term as “suggesting the calm as well as antiseptic, controlled quality of a thought experiment.” See Edward W. Said, “Representing the Colonized: Anthropology’s Interlocutors,” *Critical Inquiry* 15, no. 2 (1989): 209–10.

⁸⁸ This literature is extensive. Two influential approaches are: Edward W. Said, *Orientalism* (New York: Vintage, 1979); Trouillot, “Anthropology and the Savage Slot.”

⁸⁹ Matti Bunzl, “Boas, Foucault, and the ‘Native Anthropologist’: Notes toward a Neo-Boasian Anthropology,” *American Anthropologist* 106, no. 3 (2004): 440.

Boasian tradition, employs “rigorous historicization in place of ethnographic naturalization.”⁹⁰

In addition to historicizing the categories of subject and researcher, this project works to trouble this distinction in its design and methodology. In studying the process of research, the scholars who might otherwise be “researchers” became my subjects. As “their historian” I accrued debts, obligations, and affective ties to the natural and social scientists who hosted me.⁹¹ The scholars, who took time to record oral history interviews, open their personal archives, and teach me about their experiences, peopled my dispersed field of study. It was a field that was profoundly affective: They cared for me in many ways, from helping me get a visa and ethics committee approval, to correcting my Portuguese, to advising me and hosting me in their homes.⁹² To further complicate matters, when I joined James Welch and Carlos Coimbra on an ongoing cultural documentation program in Pimentel Barbosa village, I became the newest studier of the Xavante. I spent a short period—just under four weeks—in Xavante territory in 2015 in what was the first trip in an ongoing project.⁹³ This process gave me an initial experience

⁹⁰ Ibid.

⁹¹ In my first month working on my dissertation research in Brazil in 2013, geneticist Maria Cátira Bortolini introduced me to one of her colleagues at a genetics conference as “our historian.” She went on to explain that I had IRB/ethics committee approval for my research and that I would be studying the work of Francisco M. Salzano. This public introduction also served as a reminder and an enjoiner, not unlike Sidówi’s announcement to me that I would do “important work.”

⁹² Geneticist Maria Cátira Bortolini sponsored my stay at the UFRGS as a Fulbright IIE fellow from 2013–2014. This project was reviewed and approved by the IRB of the University of Pennsylvania under the supervision of M. Susan Lindee (protocol # 818367), and also reviewed and approved by the Ethics Committee at UFRGS with the sponsorship of Maria Cátira Bortolini (número de parecer 384.899). Maria Cátira Bortolini also supported my project in countless ways during my time in Porto Alegre and I am profoundly grateful for that support.

⁹³ My fieldwork in July of 2015 in T.I. Pimentel Barbosa was conducted as part of the cultural documentation project “Wahöïmanazé: Documentação das tradições culturais e históricas Xavante,” sponsored by the Museu do Índio–FUNAI, and co-coordinated by James R. Welch, Tsuptó Buprewen

of becoming one of the Xavantes' *warazú* researchers. I learned what it is like to be studied by them in the village, and how they worked to find common ground with my interests and appealed to me at affective, practical, and political levels. It also shifted my relationships with many of the researchers whose scholarship and experiences I had been studying. New conversations were opened to me, both mundane and existential.

Chapter Outline

Chapter 1, "Becoming Xavante, Becoming a Xavantólogo: The Formation of Research Subjectivities" compares two foundational field seasons. Contrasting David Maybury-Lewis' experience in 1958 with Nancy Flowers' work in the same community in 1976 provides insight into how Xavante interlocutors came to recognize researchers as a particular kind of *warazú*. As this happened, they also came to understand that there were certain aspects of their lives and identities that these notebook- and pencil-wielding outsiders were interested in documenting. Likewise, the researchers were passing through the sometimes-searing process of becoming anthropologists, developing notions of who they were and what their work meant. This chapter introduces the concept of *research subjectivities* to examine the co-constitutive nature of human interactions in the field.

Chapter 2, "Fission-Fusion: Interdisciplinarity in the Human Geneticists' Tribe" examines how Xavante villages became the site of a pilot study in human genetics in the

Wa'iri Xavante, and myself. The visit and my work in Pimentel Barbosa village was discussed and accepted by the *warã* or men's leadership council following my arrival. My government authorization to visit Pimentel Barbosa village was issued in *ordem de serviço* 026/2015-GAB/MI on 2 July 2015. All documentation created during this trip is archived in the project collection at the Museu do Índio, and is available only with authorization from the leadership of Pimentel Barbosa village. The project is a renewal of the earlier initiative "Danhiptetezé: Iniciativa de Cultura Alimentar Xavante." More information on the project is available online at <http://prodocult.museudoindio.gov.br/etnias/xavante/projeto>.

early 1960s. Scientists' pre-existing conceptualization of Indigenous peoples as more natural and more cultural than so-called civilized populations motivated them to develop and institute an interdisciplinary approach. In order to do so, geneticists James V. Neel and Francisco M. Salzano sought out the expertise of physical and socio-cultural anthropologists. At a moment of unbridled optimism regarding the capacities of science and the possibilities of interdisciplinarity, the geneticists' approach won funding and attention, but the implementation of these ideals was anything but simple.

Building on this account of the conceptualization and logistics of the geneticists' project, Chapter 3, "Subject 01: Cold War Masculinities and the Exceptional Indigenous," examines Apowẽ, the first human subject to be included in the 1962 pilot study. As a famous Xavante leader and a human subject, he had extraordinary influence on theories of human microevolution. Building on feminist science studies, I show how the confluence of geneticists' notions of idealized, primordial masculinity and Apowẽ's life story became a powerful tool for scientists' thinking. Reconstructing the inclusion of Apowẽ into the initial research program, this chapter uncovers the experience and influence of one Xavante individual on an emerging field of study. Apowẽ was more than a charismatic leader widely represented in the popular media. He was an exceptional subject who shaped theories of human genetic differentiation and is studied to this day, four decades after his death.

Chapter 4, "*Militantes*: Studying the Indigenous under Military Rule," elucidates the fraught political and moral economies of academic research during the Brazilian Military Dictatorship (1964–1985). I focus on the *xavantólogos* as a thread to explore the contradictions of the period. The developmentalist agenda both imperiled and enabled the

study of Indigenous peoples in Brazil; it fundamentally threatened Indigenous territory, health, and sovereignty while proffering generous investment in academic research and universities. Imbricated in systems of state patronage, employed as NGO-consultants to assess and mitigate harm, and actively protesting the ongoing genocide on the international stage, researchers faced complex professional and ethical obligations. As Brazilian (and Brazilianist) anthropology institutionalized during this period, its practitioners responded by articulating a research politics of *militância* (militancy or activism), spurred by their interlocutors to prioritize action and advocacy.

Chapter 5, “Xavante Affective Labor” returns to the field to look at the experience of a series of researchers from recent years. Comparing accounts of fieldwork by Genographic Project geneticists in 2010, sociocultural anthropologist James Welch from 2004 on, and my own experience beginning a digital archive project in 2015, I examine the affective labor that makes possible our presence in the field.

Grounded in the view from the field, *Studying Indigenous Brazil* examines how research has unfolded in Xavante communities over the past half century. Documenting the steady stream of experts who arrived in Central Brazil, this project shows how characterizations of human diversity fueled the growth of their disciplines and transnational careers. Through the Cold War and Brazil’s military dictatorship, scholars’ characterizations circulated nationally and internationally, shaping the fields of human genetics, anthropology, and public health. Over these repeated interactions, Xavante subjects also studied their experts, and in the process developed strategies to manage and influence their academic interlocutors, even though the scope and reach of their control was curtailed by structural inequities. This project is a history of how these people,

researchers and subjects, made and remade themselves through the human entanglement of fieldwork. It is also about the political and epistemic reverberations of this work, which extended beyond the site of encounter into Brazilian bureaucracies and professional imaginaries. It is a history of the situated and embodied knowledges that researchers and subjects produced over recurring interactions under the structures of settler colonialism.⁹⁴ It is also in and of itself, a situated and partial perspective informed by the personal relationships and social debts I accrued as a historian and a novice *xavantóloga*.⁹⁵

⁹⁴ Wolfe, "Settler Colonialism and the Elimination of the Native," 393.

⁹⁵ Haraway, "Situated Knowledges," 583.

Chapter One
Becoming Xavante, Becoming a *Xavantólogo*:
The Formation of Research Subjectivities

Introduction

David and Pia Maybury-Lewis were elated when they finally glimpsed Wedezé village from the small windows of their Beechcraft as it circled to land: “It was just like the old style settlements we had seen described in countless books by travellers and anthropologists,” David Maybury-Lewis wrote, “About twenty big beehive-shaped thatch huts were ranged in a long oval, open at one end. A web of well-used trails glinted like bones as they converged on it. In the centre were two circular patches of cleared ground. It was too good to be true! The meeting places of the two moieties!”⁹⁶ Bumping to a stop on the barely existent runway, the small plane was quickly surrounded by members of the village as the family disembarked. It was March of 1958.

When the British anthropologist first set his sight on the Xavante, they were already famous in the Brazilian press. This was due to their widely discussed resistance to so-called pacification, the process by which the Brazilian government established “peaceable” contact with previously hostile Indigenous groups. Prior to the Maybury-Lewis family’s arrival, the village had quite extensive experience of visitors. They had

⁹⁶ David Maybury-Lewis, *The Savage and the Innocent* (Boston: Beacon Press, 1988[1965]), 161. “Moiety” refers to one of two social groups in a dual social organization. In the strictest usage of the term, moieties are exogamous social groups, meaning that members of one moiety can only marry a member of the other moiety. Maybury-Lewis would later come to recognize that, in fact, there were not separate meeting places for the two moieties. This early expectation was undermined by the data he collected.

received photographers, filmmakers, and government officials.⁹⁷ They had been the focus of a number of popular accounts, but beyond the armchair suppositions of a few influential anthropologists, little serious scholarship existed.⁹⁸ Likewise while the Xavante had posed for photographs and films, and had hosted curious journalists for a day or two, they had never had sustained interactions with *warazú* (non-Xavante people) interested in extensively documenting their habits, peculiarities, traditions, and politics. Upon the arrival of the David Maybury-Lewis, his wife Pia, and their one-year old son Biorn, both visitors and subjects faced steep learning curves.

The Maybury-Lewis family was embarking together on a fundamental rite of passage within the discipline of anthropology. Maybury-Lewis was the first of many scientists to test their mettle in Xavante territory. “The field,” glorified and mystified, has long been the site of foundational learning and formation of the academic self. This notion is particularly prominent in anthropology, but has also been central in the long tradition of narratives of daring scientific expeditions from across the natural and social sciences; “the field” has often been the crucible in which the researcher forges his or

⁹⁷ The first medical study of the Wedezé region was A. S. Freitas-Filho “Inquérito médico sanitário entre os índios Xavante,” in *Relatório de atividades do Serviço de Proteção aos Índios durante o ano de 1954*, ed. M. F. Simões (Rio de Janeiro: Serviço de Proteção aos Índios, 1955), 145–172. As I discuss at more length in Chapter 3, there were many early publications that emphasized the warrior-like nature of the Xavante and their masculine resistance to contact with outsiders. See Souza, *Entre os Xavante do Roncador*; Souza, *Os Xavante e a civilização*; Fonseca, *Frente a frente com os Xavantes*. This literature and the media storm that accompanied this early contact with the Xavante is covered in detail in Garfield, *Indigenous Struggle*, 59.

⁹⁸ Herbert Baldus, “Tribos da bacia do Araguaia e o Serviço de Proteção aos Índios,” *Revista do Museu Paulista* 2 (1948): 137–69; Herbert Baldus, “É belicoso o Xavante?” *Revista do arquivo municipal* 142 (1951): 125–29. Claude Lévi-Strauss was particularly interested in Jê-speaking groups for analysis of dual organization, and in addition to his own fieldwork among the Bororo, drew on publications by Curt Nimuendajú and others on fieldwork conducted in Xerente, Apinayé, and Kanela communities to generalize about Central Brazilian groups. See, Claude Lévi-Strauss, *Structural Anthropology*, trans. Claire Jacobson and Brooke Grundfest Schoepf (New York: Basic Books, 1963), 120–166.

herself.⁹⁹ Two decades of scholarship on the history of field research have explored the dynamics of knowledge production in Indigenous villages, rural communities, and temporary field camps, highlighting the classed and gendered nature of attributions of scientific authority and credit.¹⁰⁰ This literature has excelled in documenting the invisibilized labor of Native guides and scientists' wives, and demonstrated how local artists, collectors, and research assistants shaped the formation of academic fields in urban centers and far away metropolises.¹⁰¹ Few studies, however, have focused more broadly on communities of subjects who have been repeatedly studied, or on how the subjects of study have come to understand and engage with researchers in context of sustained interactions such as longitudinal research.¹⁰² "The field" is not a passive social

⁹⁹ On "the field" and perceptions of its value across the natural sciences, see Robert E. Kohler and Jeremy Vetter, "The Field," in *A Companion to the History of Science*, ed. Bernard Lightman (John Wiley & Sons, 2016); on the importance of "the field" as a location and a methodology for anthropology, see Akhil Gupta and James Ferguson, eds., *Anthropological Locations: Boundaries and Grounds of a Field Science* (Berkeley: University of California Press, 1997). As they point out, "'The field' of anthropology and 'the field' of 'fieldwork' are thus politically and epistemologically intertwined; to think critically about one requires a readiness to question the other," (ibid., 3). George W. Stocking Jr. was one of the earliest scholars to discuss the rise of fieldwork as "the basic constituting experience both of anthropologists and of anthropological knowledge," in *The Ethnographer's Magic and Other Essays in the History of Anthropology* (Madison: University of Wisconsin Press, 1992), 282. On the cult of heroic fieldwork, Henrika Kuklick has shown, the rise of the notion of "the field" in anthropology can be linked to a longer tradition of gentlemanly field work in natural history: "... the cult of fieldwork could not have developed without Victorian-era expectations that personal growth (of an implicitly masculine sort) could be effected through pilgrimages to unfamiliar places, where the European traveler endured physical discomfort and (genuine or imagined) danger." See: Henrika Kuklick, "After Ismael: The Fieldwork Tradition and Its Future" in Gupta and Ferguson, *Anthropological Locations*, 48. See also Henrika Kuklick and Robert E. Kohler, "Introduction: Science in the Field," *Osiris* 11 (1996), 6; Bruce Hevly, "The Heroic Science of Glacier Motion," *Osiris* 11 (1996): 66–86. <http://www.jstor.org/stable/301927>.

¹⁰⁰ Kuklick and Kohler, "Introduction: Science in the Field."

¹⁰¹ On Native and Indigenous informants, see Margaret M. Bruchac, "My Sisters Will Not Speak," and Camerini, "Wallace in the Field." On women, wives, and lovers in the field: Corrêa, *Antropólogas e antropologia*; Richard Handler, ed., *Significant Others: Interpersonal and Professional Commitments in Anthropology* (Madison: University of Wisconsin Press, 2004). On illustrators: Bleichmar, *Visible Empire*; on collectors: Endersby, *Imperial Nature*; and on field assistants: Schumaker, *Africanizing Anthropology*.

¹⁰² Two exceptions include a number of contributions in P. Wenzel Geissler and Catherine Molyneux, eds., *Evidence, Ethos and Experiment: The Anthropology and History of Medical Research in Africa* (London: Berghahn Books, 2011). These essays, focused on research initiatives in diverse parts of Africa, consider

landscape that molds the researcher; scientists and the hosting communities constitute and re-constitute themselves and each other through research.

In this chapter, I examine the process by which the members of one Indigenous village became self-aware of their status as subjects of study. “Xavante” is not a static category, and what it meant to be Xavante shifted and developed over time. Part of this development resulted from hosting scholars who sought to understand and explain what they saw as basic facts about being human through their study of the Xavante. Comparing the experiences of David Maybury-Lewis and Nancy Flowers, socio-cultural anthropologists who conducted extensive fieldwork in the same village two decades apart, this chapter shows that over time Xavante interlocutors learned to “make themselves up” through the mediation of the ethnographer.¹⁰³ This process did not occur insulated from other kinds of interactions with outsiders, and the Xavante fashioned themselves on the political stage in myriad ways that did not depend solely on their academic visitors, but the process of interacting with researchers shaped their notions of what was distinctive, what was valuable, and what was valorized about who they were. It gave them a sense of their collective image to outsiders, which they subsequently deployed for their own ends.

The historical record for these interactions is biased, as the anthropologists in question produced the vast majority of sources available to me for analysis. Community members did resist the incursions of outsiders, as described here through the lens of the

the concept of the “Trial Community” as all of those involved in the research from principle investigator and administrators to nurses, interpreters, and subjects.

¹⁰³ Ian Hacking, “Making Up People.”

anthropologists' reflections. It is likely that there were individuals and families that objected more forcefully to the presence of the outsiders, and also likely that they interacted less with the *warazú* and occupied less space in their writings. But ultimately both Maybury-Lewis and Flowers were allowed to stay in the field. Without discounting the importance of Xavante resistance, this chapter focuses on how it was possible for the anthropologists to do their work even when it seemed strange or invasive, or contributed to disagreements within the village that hosted them. I emphasize that resistance and collaboration co-existed to shape what the anthropologists were able to do and also what they were induced to do during their time in the field.

An extensive critique of the colonial origins of the field of anthropology and the participation of its practitioners in colonizing processes emerged in the mid-twentieth century.¹⁰⁴ Early works within this literature tended to foreground resistance and domination as the axes of the colonial encounter of anthropological research. Since these initial critiques were raised, a subsequent set of works turned to examine colonial

¹⁰⁴ Native scholars have been among the most vocal critics. For example, Vine Deloria Jr. opened his 1969 chapter dedicated to the topic of social scientists in Indian Country with the damning indictment that “Indians have been cursed above all other people in history. Indians have anthropologists.” See: Vine Deloria Jr., *Custer Died for Your Sins: An Indian Manifesto* (Norman: University of Oklahoma Press, 1969), 78. Likewise, Maori scholar Linda Tuhiwai Smith opened the second edition of her *Decolonizing Methodologies* saying “The word itself, ‘research,’ is probably one of the dirtiest words in the indigenous world’s vocabulary.” See: Linda Tuhiwai Smith, *Decolonizing Methodologies: Research and Indigenous Peoples*, (London: Zed Books, 2012), 1. On anthropology as the “child of Western imperialism,” see: Kathleen Gough, “New Proposals for Anthropologists,” *Current Anthropology* 9, no. 5 (1968): 403; Gerald D. Berreman, “Is Anthropology Alive? Social Responsibility in Social Anthropology,” *Current Anthropology* 9, no. 5 (1968): 391–96. Most commonly cited for branding anthropology the “handmaiden of colonialism” are the contributions in Talal Asad, ed., *Anthropology and the Colonial Encounter* (Ithaca: Ithaca Press, 1973). The literature on Cold War anthropology from the United States has focused particularly on the neo-imperial nature of the field and the relation between anthropology and US systems of surveillance. See, for example, Dustin M. Wax, ed., *Anthropology At the Dawn of the Cold War: The Influence of Foundations, McCarthyism and the CIA* (Ann Arbor: Pluto Press, 2008); and David H. Price, *Cold War Anthropology: The CIA, the Pentagon, and the Growth of Dual Use Anthropology* (Durham: Duke University Press, 2016).

imbrications in greater nuance, deemphasizing the notion of a uniform and totalizing colonialism to instead look to historically grounded studies—a multitude of specific colonialisms—and the individuals who inhabited them. This shift helped uncover contingency and ambiguity in the relationship between the social sciences and the larger political and economic systems in which they functioned.¹⁰⁵ As Talal Asad has emphasized, beyond its important discursive functions, anthropological knowledge has usually played a limited role in maintaining “structures of imperial domination.”¹⁰⁶ However, colonialism has been fundamental to anthropologists, facilitating access to peoples and territories they sought to study, but also shaping the power dynamics, material realities, and political positions they encountered and cultivated. This chapter examines how the changing conditions under settler-colonialism in Xavante territory informed the development of the researcher-subject relationship.

The tale of two field seasons presented here also complicates facile distinctions between researcher and subject. Just as “subjects” were establishing new senses of self, researchers were also in the process of forming themselves, developing their own subjectivities as anthropologists and as *xavantólogos*. Without discounting power differentials, I raise the question of who, in fact, was being studied. Over the course of their interactions with visiting *warazú*—the kind that carried pencil and notebook wherever they went—one Xavante village learned to recognize the culture of anthropology. They witnessed another way of being, and in the context of dramatic

¹⁰⁵ George W. Stocking Jr., ed., *Colonial Situations*; Henrika Kuklick, *The Savage Within: The Social History of British Anthropology, 1885–1945* (Cambridge: Cambridge University Press, 1993); Schumaker, *Africanizing Anthropology*.

¹⁰⁶ Talal Asad, “Afterword,” in Stocking, *Colonial Situations*, 315.

political, social, and environmental change, they began to consider very carefully what they might gain from research interactions and resulting relationships.

This chapter introduces the concept of *research subjectivities* to describe the fashioning of new notions of self that occurred through the engagement of researcher and researched. Despite the many and varied meanings and uses of the term “subjectivity,” it is of use here because it draws our attention to the dynamic relationship between internal and external worlds, political and lived realities, power relations and the construction of meaning. I draw on Sherry Ortner’s definition of subjectivity as both “an ensemble of modes of perception, affect, thought, desire, fear, and so forth that animate acting subjects,” and “the cultural and social formations that shape, organize, and provoke those modes of affect, thought and so on.”¹⁰⁷ This approach to subjectivity is less focused on the absorption of external structures, internalized to shape the thoughts and actions of the subject.¹⁰⁸ Certainly, aspects of what made an anthropologist an “anthropologist,” or a Xavante person “Xavante” were determined by external legal, social, and political constructs, but these cultural forms were not immutable, and within the organization that they offered there was space for self-formation that was creative, affective, and personal. Both researchers and researched had to contend with external notions of who they were and what they were doing as they began to interact with one another. Simultaneously, they were reworking their self-images, learning new things about what made them who they were and what factors of self were most salient. I focus on the social interactions of

¹⁰⁷ Sherry B. Ortner, “Subjectivity and Cultural Critique.” *Anthropological Theory* 5, no. 1 (2005): 31, doi:10.1177/1463499605050867.

¹⁰⁸ Pierre Bourdieu, “Structures, habitus, practices,” in Bourdieu, *The Logic of Practice* (Stanford: Stanford University Press, 1990), 52–79.

the field—the lived experience of research—and subsequent narratives, published and recounted in oral history, which documented these social interactions.

In employing the notion of subjectivity, I aim to avoid what Biehl, Good, and Kleinman refer to as “overstated, obscure, and even de-humanizing” theories of subjectivity, drawing attention instead to the process by which subjectivities are formed.¹⁰⁹ Locating lived experience of research in specific times, places, and historical contexts shows how people come to understand themselves and experience their lives differently through participation in formal, academic knowledge production. Building on feminist science studies’ calls for histories of *situated knowledges*, this examination of senses of scientific self pays attention to inner worlds and emphasizes emotional and bodily experiences.¹¹⁰ Hope, fear, mourning, disappointment, hunger and humor are often excluded from accounts of knowledge making, but, in fact, play a central role in why people participate in research, as investigators or subjects.¹¹¹ In this analysis, I do not intend to suggest that the “research subjectivities” of either researcher or researched described in this account can be rendered applicable to all anthropologists, or all Xavante interlocutors, transforming them into abstracted “subject positions.”¹¹² Rather, I hope to show that all individuals involved in the processes of fieldwork negotiated what Biehl, Good, and Kleinman call “the dynamic and unsolved tension between the bodily, self,

¹⁰⁹ João Biehl, Byron Good, and Arthur Kleinman, “Introduction: Rethinking Subjectivity” in João Biehl, Byron Good, and Arthur Kleinman, eds., *Subjectivity: Ethnographic Investigations* (Berkeley: University of California Press, 2007), 1–23.

¹¹⁰ Haraway, “Situated Knowledges.”

¹¹¹ On the importance of the body as an “instrument” in fieldwork, see Henrika Kuklick, “Personal Equations: Reflections on the History of Fieldwork, with Special Reference to Sociocultural Anthropology,” *Isis* 102, no. 1 (2011): 1–33.

¹¹² Biehl, Good, Kleinman, “Introduction: Rethinking Subjectivity,” 10.

and social/political processes” and which they hold, “is the core of subjectivity.”¹¹³

Subjectivity draws attention to the dynamic relationship between individual and collective, the bodily and the social. *Research subjectivities*, then, emphasizes the imbrication of scientific investigation in the formation of subjectivities.

David Maybury-Lewis arrived with his family in Xavante territory in 1958, at a moment when the Wedezé village leadership, and particularly the charismatic leader Apowê, maintained lucrative relationships with government officials characterized by generous gifts of manufactured goods. The villagers had a semi-nomadic lifestyle with unimpeded migrations through a large tract of their ancestral territory. Although by this period sustained interaction with the *Serviço de Proteção aos Índios* (SPI, the Indian Protective Service) and the expanding western edge of settler society had disrupted access to natural resources, hunting regions, and daily lifestyles in many Xavante communities across the region, the Xavante at Wedezé continued to control a substantial area with relatively limited incursion from unwelcome *warazú*.¹¹⁴ By the time cultural anthropologist Nancy Flowers arrived in 1976, local reality had dramatically shifted. The group from Wedezé had crossed the Rio das Mortes and settled in an area to the west referred to as Pimentel Barbosa. Their land was under constant siege and they had lost large portions of their territory to *fazendeiros*—ranchers and wealthy landowners—through questionable transactions, arrogation, and mismanagement by government agents. These differing contexts were critical in shaping villagers’ notions of what

¹¹³ Ibid., 15.

¹¹⁴ Disruptions in Xavante life due to settler colonialism began in the eighteenth-century, of course, and included fairly extensive contact and disruption during the first half of the nineteenth-century, before communities migrated west to avoid violence and disease. See Garfield, *Indigenous Struggle*, 3–6; Aracy Lopes da Silva, “Dois séculos e meio de história Xavante.”

researchers could offer.

In their first interactions with academics, Xavante villagers had little concept of what scholars wanted and what they did with their extensive documentation of language, social practices, and physiologies after they left the village. However, as I show here, villagers quickly came to distinguish academic *warazú* from other visitors, progressively learning how to interact with these curious outsiders. The Xavante discovered how to build common ground with the *warazú* who wanted to study them. Researchers' presence necessitated extensive work of teaching, translating, feeding, and guiding. Long-term fieldwork turned informants' attention to aspects of their social systems and daily practices in a way that short visits from journalists or the consistent presence of government administrators did not. It also potentially exacerbated political conflict within a single village, especially as scientists offered material goods in compensation for the community's collaboration. Furthermore, as local and national socio-economic and political realities shifted, so did Xavante expectations and interactions for the scientists who visited their communities. In dialogue with the researchers, whose codes of ethics and notions of academic responsibility to their subjects were changing, the Xavante increasingly demanded political engagement.

I take particular time to describe two foundational experiences of research both because they mark a major transition in terms of how villagers made sense of what it meant to be researched, and because they laid a foundation for later experiences of research. The formative quality of each experience was different. Maybury-Lewis' scholarship created an analytical matrix that all subsequent anthropological (and much public health) research would be expected to build on or respond to explicitly. In the

village, he also became the model for what a certain kind of research would be like; experience interacting with him constituted the basis for how the Xavante would respond to and work to influence future researchers. Flowers' research was foundational for a different reason. She arrived in the village at a politically fraught moment. Her presence influenced a generation of young adults and leaders who would come to understand researchers as a potential political resource, and the guiding, teaching, and presentation of Xavante culture and self as an important political strategy. Flowers' return to the village for subsequent research in the 1990s (as described in Chapter 5) would contribute to the consolidation of a valuation of long-term research engagement. The legacy of these two researchers is particularly pronounced both in the narratives that subsequent scholars articulated in oral history interviews,¹¹⁵ and in the descriptions of elders in Pimentel Barbosa village who spoke with me about their experiences with researchers.¹¹⁶ This chapter lays the foundation for following chapters' discussion of subsequent research and the evolving political and affective strategies that villagers use to engage with curious *warazú*.

Foundations in the Field

This section draws on Maybury-Lewis' published work to examine his arrival in

¹¹⁵ Researchers who trained at ENSP repeatedly spoke about how Flowers' scholarship and mentorship were essential to them. Silvia A. Gugelmin, interview with Rosanna Dent, 4 August 2015, Cuiabá, MT. Luciene Guimarães de Souza, interview with Rosanna Dent, 25 April 2015, Rio de Janeiro. In informal discussion, Gugelmin and de Souza described Nancy Flowers and her data as a "fountain" for many a researcher (5 August 2015, Cuiabá, MT).

¹¹⁶ In conversation with elders, they often emphasized their interest in materials collected by David Maybury-Lewis, their memories of him, and the delay in hearing about his death when he passed away. Tsuptó Buprewên Wa'iri Xavante, Barbosa Sidówi Wai'azase Xavante, Luiz Hipru Xavante, Agostinho Saseru Xavante, interview with Rosanna Dent, 4 June 2014, Água Boa, MT; Sidówi, interview.

Wedezé village, and the processes by which he came to be recognized as an anthropologist over the course of his eight-month stay in Central Brazil. A close reading of the anthropologist's accounts of his time in the field provides insight into how he struggled to make sense of his position and relationship with the community. It also offers oblique glimpses into the experiences of those who hosted him.

Maybury-Lewis was unusual in his choice to publish two accounts of his fieldwork. The first and most traditional product of his research was his doctoral dissertation and its subsequent reincarnation as the 1967 monograph *Akwẽ-Shavante Society*. A classic mid-century ethnographic study, the formal structuralist analysis of Xavante society and politics attempted to give a complete account of a society and included extensive detail. But even before his academic monograph was released, Maybury-Lewis published a semi-popular account that encompassed his field experience among the Xerente and the Xavante. His 1965 *The Savage and the Innocent*, he wrote, was “not an anthropological essay,” but rather included “many of those things which never get told in technical anthropological writings,” such as daily experiences, impressions of the region, and personal “feelings about the day-to-day business which is mysteriously known as ‘doing fieldwork’.”¹¹⁷ Few anthropologists at the time openly discussed the less flattering aspects of their own practices, but Maybury-Lewis commented on distrust, disgust, pride, and conflict that both he and his informants experienced during the process of research.¹¹⁸ In one sense, he anticipated the trend of

¹¹⁷ Maybury-Lewis, *Savage and the Innocent*, 9.

¹¹⁸ For an almost-contemporary review of the rise of self-conscious ethnography, see Dennison Nash and Ronald Wintrob, “The Emergence of Self-Consciousness in Ethnography,” *Current Anthropology* 13, no. 5

open self-reflection regarding his fieldwork experiences, and reportedly was worried enough about his colleagues' reactions to consider publishing the candid account under his wife's name.¹¹⁹ The compelling story-telling and lurid detail of *The Savage and the Innocent*, especially in comparison to the more formal prose of *Akwẽ-Shavante Society*, provide a window into how Maybury-Lewis understood his position as anthropologist, as well as those aspects of his presence that most fascinated and repelled his informants.¹²⁰

As David Maybury-Lewis disembarked from the small bush plane that had carried him, his family, and half of his possessions into Xavante territory in 1958, it quickly became clear to the villagers in Wedezé that the tall Englishman was of a rather different ilk from previous visitors. There were a number of things that set the Maybury-Lewis family apart. First, and perhaps most intriguingly, the anthropologist arrived with his Danish wife and tow-haired one-year-old son. While government employees of the SPI might live with their families at the posts, visitors rarely included women and even less frequently children. Pia Maybury-Lewis (née Henningsen) had accompanied Maybury-

(December 1972): 527–42, doi:10.1086/201287. Maybury-Lewis was by no means the first, but he predated the publishing of Malinowsky's field diaries—Bronislaw Malinowski, *A Diary in the Strict Sense of the Term* (Stanford: Stanford University Press, 1989[1967])—and has repeatedly been credited with being ahead of his time in reflexivity. See Harald Prins and Laura Graham, "Pioneer in Brazilian Ethnography and Indigenous Rights Advocacy: David Maybury-Lewis (1929–2007)," *Tipiti: Journal of the Society for the Anthropology of Lowland South America* 6, no. 1 (2008), <http://digitalcommons.trinity.edu/tipiti/vol6/iss1/8>; Richard Reed, "At the Intersection of Scholarship and Activism," *Anthropological Quarterly* 82, no. 4 (2009): 1065–68. For a popular obituary that highlighted his reflexivity and activism, see J. Lorand Matory, "David Maybury-Lewis: Anthropologist keen to protect the interests of the peoples of central Brazil," 5 February 2008, *The Guardian*.

¹¹⁹ Prins and Graham, "Pioneer in Brazilian Ethnography," 117. Other anthropologists also worried about the appropriateness of publishing candid accounts of fieldwork. For example, in 1954 anthropologist Laura Bohannon published *Return to Laughter* under the pseudonym Elenore Smith Bowen. See: Elenore Smith Bowen, *Return to Laughter* (Garden City, NY: Anchor, 1964[1954]).

¹²⁰ Many colleagues and others who were close to Maybury-Lewis during his lifetime emphasized that his use of the title *The Savage and the Innocent* was intended to trouble the notion of who the "savages" were by showing his own inability to conform to Xavante social standards. The fact that this is so often stated has made me wonder if, perhaps, many people tended to miss this intention as they read his depictions of his interlocutors.

Lewis on his first extensive field-season, their eight-month stay in a Xerente village in 1955-56. A nutritionist by training, her main task during their second field season would be to keep their one-year old out of trouble, and also to try to see that all members of their *warazú* household were fed.¹²¹ Arriving as a family was a calculated move. As the Xavante were thought to be particularly bellicose (see Chapter 3), Maybury-Lewis thought that his wife and child would attenuate the threat he might pose as an outsider: “I was convinced that the Shavante would be intrigued by the presence of Pia and Biorn, our son, and that this might solve at one blow the difficulties of getting to know such reputedly intractable Indians.”¹²² He had worried that he would be seen as a threat arriving as a twenty-nine year old man, alone. Bringing his family would help him soften his image, present himself as a loving husband and father, and attenuate any potential concern about his designs on women in the village. Much as Maybury-Lewis predicted, from the very beginning, the baby was their “trump card” for building rapport.¹²³ The villagers “were fascinated by him,” Maybury-Lewis wrote, “by his fair hair and blue eyes, by his clothes, his movements, everything.”¹²⁴ Indeed, the Englishman emphasized that it was not himself or his wife, but his son Biorn who most successfully won the

¹²¹ As Mariza Corrêa has eloquently pointed out, the wives of many influential anthropologists who conducted work in Brazil have been largely forgotten, including Dina Lévi-Strauss (subsequently Dina Dreyfus after divorcing and remarrying), Helen Pierson, and Frances Herskovitz. See Corrêa, *Antropólogas e antropologia*, 19–25. Pia Maybury-Lewis is somewhat of an exception, for David wrote her into his accounts of his fieldwork both in his popular and academic publications, and she was integral in their subsequent hosting of local and visiting anthropologists in Cambridge. Furthermore, she was widely recognized for her advocacy work as co-founder (with David) of the Indigenous rights NGO Cultural Survival in 1972 (see Chapter 4). This work was recognized with the American Anthropological Association’s Distinguished Service Award (jointly with David) in 1988. See Nur Yalman, “Remembering David,” *Anthropological Quarterly* 82, no. 4 (2009): 1073–76. doi:10.1353/anq.0.0104.

¹²² Maybury-Lewis, *Savage and the Innocent*, 154.

¹²³ *Ibid.*, 169.

¹²⁴ *Ibid.*, 169–70.

goodwill of their hosts over the course of their stay in Central Brazil.¹²⁵ Through the waves of excitement and dread of arriving in the village, Maybury-Lewis hoped the curiosity regarding his family might ease his acceptance.

To further differentiate the anthropologist and his family from previous visitors, they had the peculiar idea of living *in* the village with the community, rather than residing at the government post as other *warazú* were wont to do. Following the foreign family's first request to move into the village the Xavante "were not angry, so much as mystified" according to Maybury-Lewis' perception, and "not a little amused."¹²⁶ If they were perplexed by the idea of him and his family residing with them, they were even more incredulous when the anthropologist stated his plan to accompany them on their trek, a long, seasonal migration to collect and hunt: "Now there were roars of laughter."¹²⁷ But when Maybury-Lewis insisted, eventually his reluctant hosts relented. Staying in the village, and joining the families on trek meant much more contact, conversation, and opportunity to observe and be observed. Maybury-Lewis was part of a generation of anthropologists that was expected to conduct extensive fieldwork and maintain intimate, daily contact with his informants. E. E. Evans-Pritchard's 1951 vision of fieldwork reigned at the time, holding that the anthropologist would only produce nuanced and accurate scholarship if he were to "establish ties of intimacy with them [his interlocutors], and to observe their daily activities from within, and not from without, their community

¹²⁵ Maybury-Lewis did not consider the relationship he and his family established with the Xavante of Wedezé to be "satisfactory" until he had been in the village (and on trek) for three full months: "By the end of June 1958 we had established what might be considered a satisfactory relationship with the São Domingos Shavante, for research purposes." David Maybury-Lewis, *Akwẽ-Shavante Society*, 2nd ed. (Oxford: Oxford University Press, 1974[1967]), xxii.

¹²⁶ Maybury-Lewis, *Savage and the Innocent*, 171.

¹²⁷ *Ibid.*

life. He must live as far as possible in their villages and camps, where he is, again as far as possible, physically and morally part of the community.”¹²⁸ While familiar for David and Pia Maybury-Lewis, this physical and moral integration was alien to the villagers in Wedezé.

Much as adjusting to life in the Xavante village was challenging for the *warazú* family, the arrival and peculiarities of the scientific visitors were challenging for the Xavante families, tasked with hosting without much knowledge of why their visitors were there. While Maybury-Lewis had obtained government authorization for his research, and advised the SPI staff of the purpose, neither Maybury-Lewis nor the SPI staff had the Xavante language skills necessary to communicate the reasoning for and eventual products of the research at the beginning of the field season. The Xavante were left to learn, over time, what exactly the anthropologist and his family were doing. Noting that it was made clear during his early days in Wedezé that he was not welcome to enter homes in the village, David considered his own field research to be “by normal anthropological standards, difficult” due to the resistance of the community to his presence, linguistic challenges, lack of translators, and the impossibility of orchestrating one-on-one conversations.¹²⁹

In his first weeks, Maybury-Lewis understood himself to be a burden on his hosts. A first hunting expedition with Surupredu, who would become one of his closest informants, was an exercise in near exhaustion. “Hillocks of coarse grass thrust up through the endless dreary sea,” he wrote of trudging through a flooded region near the

¹²⁸ E. E. Evans-Pritchard, *Social Anthropology* (Routledge, 2013[1951]), 78.

¹²⁹ Maybury-Lewis, *Akwẽ-Shavante Society*, xxiii.

village on a “searingly hot” day. “I kept getting the nightmarish feeling that we had already passed through this bit of country before, so featureless did I find the landscape. But Surupredu knew every tuft of grass in it.”¹³⁰ Having bargained to accompany Surupredu in exchange for allowing him to borrow a rifle, the hunt would result in a deer, a small ostrich-like bird called a sariema, and extreme discomfort as the *warazú* tried to keep up with his host, who “just went on and on like a machine.”¹³¹ Maybury-Lewis underscored his own lack of knowledge, perception, and competence in recounting the story of how he had lagged behind his Xavante companion, slowing down the hunter and arriving back to the village through sheer will and fear of losing face. In his account, his self-effacing descriptions served to emphasize his initial feelings of distance from the people he hoped would soon accept him. In rhetorically positioning himself in the role of novice and burden, he could have set up the seemingly foretold story of eventually gaining true acceptance. However, even as he continued to live “morally and physically” with his hosts, in his popular work Maybury-Lewis never portrayed himself as achieving full acceptance or unlimited access to his hosts’ worldviews.

Only three weeks after arriving in Wedezé, David would leave Pia and Biorn in the village for a period of five weeks to set out on trek.¹³² The community split into smaller groups of a few dozen people, travelling and constructing temporary camps where they would stay for a few days at a time before moving on. It was the dry season

¹³⁰ Maybury-Lewis, *Savage and the Innocent*, 173.

¹³¹ *Ibid.*, 174.

¹³² During this period, Maybury-Lewis would face his first serious scare regarding his son’s health. He recounted his fear when a messenger arrived from the village after he had been away for a number of weeks saying that his son was very ill and only cried all the time. It turned out that Pia Maybury-Lewis sent a note, specifying that in fact the child had improved. However, later in their stay Biorn would sicken with dysentery and the family had to evacuate him to São Paulo. *Ibid.*, 199–200 and 213–214.

and the villagers migrated through large swaths of *cerrado*, savannah-like grasslands with low trees, little shade, and limited water. From their temporary camps, women would spread out to collect tubers, fruits, and other natural resources that were scarce near the village. Men spent the long days tracking game such as deer, anteaters, and wild pigs.

Maybury-Lewis joined the party a few days late, since he had been waiting on a sack of manioc flour that he had commissioned to both feed himself and offer his hosts in whichever shelter he was to share. Guided by an impatient son of Apowẽ and coaxing an equally uninterested pack donkey, Maybury-Lewis described himself as clumsily tramping through the backcountry on his trip to join the rest of the community. What the Xavante perceived as David's eccentricities became a source of amusement as he joined them migrating through the *cerrado*. Maybury-Lewis described the development of his comical entrée into life-on-trek as being "progressively cast in the role of camp jester, or perhaps mascot." He went on, "The guide who had accompanied me from São Domingos [Wedezé] reported to the men's council, as was customary, on the evening of his arrival. There he gave a detailed report on our two-and-a-half-days' journey, expertly mimicking my clumsy Shavante and recounting everything I had said and done *en route*. This included all the things I had failed to see [...] The Shavante found this uproariously funny, and were obviously amused by my general ignorance and incompetence in their habitat."¹³³ Bruised pride was compensated somewhat by the gradual opening to ask and learn about community ways. "In the weeks that followed," Maybury-Lewis wrote, "I

¹³³ Maybury-Lewis, *Akwẽ-Shavante Society*, xix. "São Domingos" is the name in Portuguese of Wedezé. It refers specifically to the village, while the Xavante name Wedezé refers to the village and the surrounding area.

found that I identified myself more and more with this little group of Indians, wandering in the wilderness.”¹³⁴

Maybury-Lewis’ awkward, uncomfortable experience on trek was also a chance to interact with and get to know a smaller group from the village. It was in this context, according to his account, that curiosity about an outsider who behaved so differently from previous visitors began to overcome suspicion: “They were intrigued by the fact that I was accompanying their trek, a thing no outsider had done previously, and this piqued their curiosity about me, especially the women.”¹³⁵ Maybury-Lewis punctuated his account with stories about bodily discomfort: sleep was an impossibility in shelters packed tightly with bodies; soot, animal entrails, and the unpleasant habit of spitting made the campsites an unsanitary nightmare; he accompanied men who seemed to know no fatigue while hunting, covering dozens of miles a day with little rest and little water. Again and again, Maybury-Lewis was ridiculed for his inability to find his way, his clumsy loss of a hunting knife (he had sunk to the waist in a marshy area while returning to camp carrying a deer over his shoulders), and his less-than-perfect aim while hunting.¹³⁶ Echoing long-standing narratives of the heroic nature of fieldwork, Maybury-Lewis described the dire and difficult conditions he faced in the search for knowledge, but he did so with a sense of irony and a self-critical tone. He painted himself as the savage, hopelessly outmatched by his hosts and by the *cerrado* itself.

Returning to the village, the Xavante continued to be wary of their guests. Even if

¹³⁴ Maybury-Lewis, *Savage and the Innocent*, 188.

¹³⁵ Maybury-Lewis, *Akwẽ-Shavante Society*, xix.

¹³⁶ Maybury-Lewis, *Savage and the Innocent*. On sleeping, 188. On grime, and offal, 188–189. On getting lost and losing his hunting knife, 191–194.

the trekking had broken the ice, they wanted to keep an eye on the outsiders. Apowẽ, in particular, kept the Maybury-Lewis close. While David Maybury-Lewis was under few illusions that community members were truly happy about hosting him and his family, he noted that to a great extent they made sense of him through his relationship with Apowẽ. The charismatic leader referred to the anthropologist by the term for ‘son’, and other members of the leader’s family and moiety also treated him as kin. According to David’s accounts, after some time in the village, “the old man had bestowed his own name on me and decided that Pia should be named after his present wife.” Pia would be known as Arenwain’on, and Biorn as Sibupá, after one of Apowẽ’s sons.¹³⁷ Undoubtedly, it was at Apowẽ’s bidding that the Maybury-Lewis’ small house was constructed next door to his own.

Their “adoption” into Apowẽ’s family and the closeness of their new home became a way for the Xavante to manage the *warazú*. Once established as “son,” “brother,” and “uncle,” Maybury-Lewis was thrust into fulfilling social responsibilities that accompanied these roles. Even though he did not yet understand the obligations that his insertion into the kinship structure entailed, his hosts used this newly articulated relationship to make sense of who was entitled to make demands on him.

David described his frustration regarding these dynamics writing, “It seemed that we were perpetually destined to be alone with Apewen’s sons. There were so many of

¹³⁷ Maybury-Lewis elaborated, saying, “I felt honoured and wished only that Apewen’s wife had not got such an unpronounceable name to pass on, it was Arenwain’on, containing three nasalized vowels and the accent on the final syllable,” *Savage and the Innocent*, 198. Many subsequent *xavantólogos* would receive names when researching in the village (myself included) but none would be named after one person specifically. Although the Xavante who live in Pimentel Barbosa generally referred to the Maybury-Lewis as “Davi” and “Bia,” Biorn Maybury-Lewis is still referred to as Sibupá in conversations with me.

them that they could keep a round-the-clock watch on us, and sometimes we felt that that was just what they did.”¹³⁸ Complaining that “They were the most difficult Shavante to deal with and the most demanding of presents and favours,”¹³⁹ Maybury-Lewis considered Apowẽ’s sons’ presence a major inhibitor of his ability to connect and converse with other members of the community. Furthermore, once anointed kin, the *warazú* family had no choice but to receive Apowẽ and his family while the chief’s house was being rethatched. As Maybury-Lewis wrote, “All of its occupants moved into our house... and we were cooped up with seventeen people and their pets in a hut the size of a small room.”¹⁴⁰

By bestowing his own name on the *warazú* and calling him “son,” Apowẽ claimed Maybury-Lewis, who now implicitly owed the chief his loyalty. However, other individuals in the community who wished to have access to the anthropologist, his family, and potentially the gifts they brought also went out of their way to establish relationships. As Maybury-Lewis focused his attention on factionalism and the political divisions within the village for the sake of his structural analysis of Xavante society, he also came to understand that he was caught up in the power dynamics of the moiety system, and had been since his arrival in the village. In his formal academic analysis of Xavante society, Maybury-Lewis would come to describe patrilineal moiety as one of the most important organizing institutions in Xavante life. He would discuss the implications of each Xavante individual inheriting his or her affiliation—either *õwawe* or

¹³⁸ Ibid., 177.

¹³⁹ Maybury-Lewis, *Akwẽ -Shavante Society*, xvii.

¹⁴⁰ Ibid., xviii.

poreza'õno—from their father, with individuals of the *õwawe* moiety further divided into two lineages, or “clans.”¹⁴¹ Moiety belonging also strongly influenced political allegiances, with conflict between members of opposing moieties a common factor in community politics. It was in this context that men pertaining to opposing political factions went out of their way to establish positive relationships with the *warazú*: “Now too I realized why Suwapte had appointed himself my patron and why Urbepte had formally become my friend. They were of different clans; Apewen’s clan was yet a third one. If Apewen adopted me, then Suwapte and Urbepte were probably anxious to see that I was not exploited to the political or economic advantage of a single faction in the community.”¹⁴² Xavante extension of kinship structures to include the visiting *warazú* resulted in a type of legibility, but the villagers did not take the classification too literally. They had carefully been observing the anthropologist and his family, often laughing at what they saw as his outlandish behavior, but also accumulating knowledge about what kinds of questions he asked, how he behaved, what he focused on, and what advantages could accrue from relationships with him.

Maybury-Lewis did not come to consider himself Apowẽ’s adoptive son, even if he was thought of that way in some senses in the village. Rather, in *The Savage and the Innocent*, he articulated extensive skepticism regarding his and his family’s acceptance in the village to the very end of his dramatic, emotional story: “I did not cherish the pathetic illusion that the Shavante accepted me as one of them or my family either,” he wrote,

¹⁴¹ Future researchers would contest Maybury-Lewis’ description of moieties and lineages. See James R. Welch, “Age and Social Identity among the Xavante of Central Brazil,” (PhD diss., Tulane University, 2011).

¹⁴² Maybury-Lewis, *Savage and the Innocent*, 238.

“They tolerated us. They might even be happy to see us come back, provided we brought plenty of presents. But they could not speak freely with us. Even if they could, we were separated by a barrier to further understanding which I wondered if years of field work could penetrate.”¹⁴³ But despite his doubts, his own sense of self had come to include his position and his work in the village.

On the final page of his popular account, Maybury-Lewis juxtaposed his fundamental lament that “People could not understand people,” with his own deep sense of loss as he prepared to leave the village, describing an emptiness “as if my own personal impetus were exhausted and I was adrift, purposeless.”¹⁴⁴ In the context of his account, his exhaustion was understandable: he had suffered through trials as simple as the frustrations of learning a language to those as dramatic as his son’s evacuation and near death due to dysentery. And yet by the time the researcher faced leaving, he described his sense of his own path as so tightly bound to his interlocutors that it seemed impossible that they would go back on trek without him and he would return to the United Kingdom. He closed his popular account writing, “Apewen embraced me and made a speech. We must come back, he said. Arenwain’on must come back. Sibupa must come back. We must bring many presents. We must bring many fish hooks. We must bring many balls of ammunition. We must bring many clothes. Yes, already they missed us. We must come back. I looked at the old fox, trying to follow his rhetoric, and it was then I noticed he had tears in his eyes. Perhaps, after all, he really meant it.”¹⁴⁵

¹⁴³ Ibid., 265.

¹⁴⁴ Ibid., 266.

¹⁴⁵ Ibid.

In Maybury-Lewis' version of his fieldwork, he implied but did not claim to have accomplished exactly what E. E. Evans-Pritchard prescribed: "An anthropologist has failed unless, when he says goodbye to the natives, there is on both sides the sorrow of parting."¹⁴⁶ By presenting his own sorrow as a slight surprise, and by opening the suggestive question of whether or not Apowẽ truly cared about the *warazú* family in addition to the manufactured goods he requested, the Englishman constructed a vision of himself that emphasized both his humility and dedication. He gave self-effacing weight to his promise, as stated in the preface to the first edition, that "every incident" in his account was true. His body was his instrument, and his excitement, fear, love and loathing were part of an empathetic ethnographic observer. He had exhausted himself—as one ought to—in his quest to make sense of another people few others were qualified to understand, and in the process he had collected data on which he would build the first fifteen years of his anthropological career.¹⁴⁷

Following his initial fieldwork in Xavante territory, Maybury-Lewis finished his doctorate under the supervision of Rodney Needham at Oxford, writing a classic structuralist account of Xavante society and then earning an appointment in the Department of Social Relations at Harvard.¹⁴⁸ In the years between his fieldwork and his tenuring in the Department of Anthropology, his discipline underwent a significant

¹⁴⁶ Evans-Pritchard, *Social Anthropology*, 79.

¹⁴⁷ Maybury-Lewis' student, Roberto DaMatta, offered a critique of this kind of "romantic" accounts of the field that were (falsely) separated from the more serious publications only a few years after *Savage and the Innocent* was published. See: Roberto DaMatta, "O ofício de etnólogo ou como ter 'anthropological blues'," *Boletim do Museu Nacional* 27 (May 1978 [1973]): 1–12.

¹⁴⁸ For critiques of structuralism: On Lévi-Strauss, see Fabian, *Time and the Other*, 52–69; on myth and narrative in structuralism, see Terence Turner, "Narrative structure and mythopoesis: A critique and reformulation of structuralist concepts of myth, narrative and poetics," *Arethusa: A Journal of the Wellsprings of Western Man* 10 no. 1 (1977): 103–63.

transformation. 1971 marked a shift in anthropology in the United States, with the American Anthropology Association specifying the obligation of researchers to contribute to public policy debates in a Statement on Ethics that affirmed “a positive responsibility to speak out publicly, both individually and collectively, on what they know and what they believe as a result of their professional expertise.”¹⁴⁹ It was in the wake of these changes that David and Pia Maybury-Lewis turned some of their attention to anthropological advocacy work, founding the NGO Cultural Survival in 1972 (discussed at more length in Chapter 4). Maybury-Lewis left the field with a sense of obligation, and would later cite his experience with the Xavante as the motivation for his advocacy work, but although he returned to the village on a number of occasions, he did not sustain fieldwork in Xavante territory after the publication of his monograph.

Recognizing Research: Daily Practices in the Field

Over the course of the Maybury-Lewises’ stay, the Xavante of Wedezé learned an enormous amount about what made a researcher a researcher. But much of what they learned was only visible once new academic visitors arrived. This became particularly apparent to the next researcher to spend an extended period in the field: Nancy Flowers, a graduate student from the City University of New York (CUNY), arrived in 1976 to begin fourteen months of fieldwork.¹⁵⁰ By the time Flowers’ study started, the

¹⁴⁹ AAA, “Principles of Professional Responsibility,” 1971, <http://www.americananthro.org/ParticipateAndAdvocate/Content.aspx?ItemNumber=1656>, accessed 11 September 2016. Cultural Survival was founded to try to cultivate this kind of public engagement.

¹⁵⁰ A series of other researchers would visit the village both at Wedezé (São Domingos) and in Etênhiritipá (Pimentel Barbosa). As discussed in Chapters 2 and 3, a team of geneticists accompanied Maybury-Lewis to conduct 10 days of research in Wedezé in 1962. Regina Müller, while a master’s student in anthropology

community had relocated to the far side of the *Rio das Mortes*, and settled at Etênhiritipá, or in Portuguese, Pimentel Barbosa village. Although perhaps not obvious to the Maybury-Lewises, in addition to keeping an eye on what the *warazú* were doing out of curiosity and an interest in making sure others did not escape with the majority of gifts, the Xavante had carefully observed their academic work. They started to identify the anthropologists' daily tasks as a specific genre of work, the researcher as a different category of *warazú*. Villagers came to associate certain kinds of observing activities with this new category.

Flowers arrived in Brazil as one of four students conducting a comparative investigation on the human ecology of Central Brazilian Indigenous groups. Daniel Gross, professor of anthropology at CUNY, designed the study to respond to the comparative structuralist analyses of Maybury-Lewis, Roberto Cardoso de Oliveira, and their students. Gross' ecological anthropology aimed to reintroduce attention to environmental and political realities that he considered neglected by the work of the *Harvard-Museu Nacional Central Brazil Project*. Each of the four graduate students would conduct similar fieldwork with a group that spoke a language of the Macro-Jê family, of which Xavante is a part, and traditionally inhabited the *cerrado*.¹⁵¹ Prior to setting out for her first experience of ethnographic fieldwork, Flowers had spent many years as a photojournalist throughout the Portuguese-speaking world. She was in her 50s

at the *Universidade de São Paulo* (USP), spent a few months in Pimentel Barbosa and another village in Areões in 1974. Müller, interview with Rosanna Dent, 17 September 2014, São Paulo. Also see Regina Aparecida Polo Müller, "A pintura do corpo e os ornamentos Xavante: arte visual e comunicação social," (master's thesis, Universidade Estadual de Campinas, 1976): 14–16.

¹⁵¹ Daniel R. Gross, George Eiten, Nancy M. Flowers, Francisca M. Leoi, Madeline Lattman Ritter, and Dennis W. Werner, "Ecology and Acculturation Among Native Peoples of Central Brazil," *Science* 206, no. 4422 (1979): 1043–50, doi:10.1126/science.206.4422.1043.

when she returned to university to complete a bachelors and PhD in anthropology.¹⁵² CUNY professor Daniel Gross offered Flowers the opportunity to join his team even before she had completed coursework and comprehensive exams for the doctorate. With National Science Foundation funding, it was a perfect opportunity to follow her interest in ecological anthropology to Central Brazil.

Before arriving in Mato Grosso, Flowers had already read *The Savage and the Innocent* and *Akwẽ-Shavante Society*; her preconceptions of what her role and her experience in the field might be were deeply influenced by Maybury-Lewis. While her colleagues headed to Kayapó, Bororo, and Eastern Timbira communities, Flowers was pleased to be working with the Xavante. She not only had a frame of reference to make sense of her work in the village, she had a preview of whom, specifically, she would meet. In a 2013 interview, Flowers reflected saying that her preparatory reading, “made me feel like I was meeting people that I knew *something* about through David Maybury-Lewis’ accounts. In fact it *was* like that because you could recognize people. He called people by their names. ... And one of the first people that I met was old Apowẽ, and his sons.”¹⁵³ As she met the individuals who featured prominently in Maybury-Lewis’ account, she compared her impressions with his descriptions. Flowers knew to take extra candies to satiate Apowẽ’s sweet tooth. She noted in her field notes that one of Apowẽ’s sons seemed “affable enough” in contrast to the “arrogant and greedy” impression that

¹⁵² Flowers “Forager Farmers,” vi. Flowers posited that it was her positive interactions with Columbia University’s Charles Wagley during her time as a photojournalist that had “perhaps unknowingly put in my head the idea of becoming an anthropologist.”

¹⁵³ Flowers, interview with Rosanna Dent, 22 August 2013, New York City.

the previous anthropologist had published.¹⁵⁴ Flowers, and the many researchers that came after her, could draw on the extensive detail of Maybury-Lewis' accounts to imagine what their field experience might be like even before arriving in a Xavante village.

When Flowers reached the field, the villagers already knew what an “anthropologist” was, and assumed that she shared some of the same “eccentricities” as the previous *warazú*. There were some key differences, too, in the eyes of her hosts. Flowers' status as a *predu* or “mature” woman was a key difference from the way Maybury-Lewis was seen to be a noviate adult during his time in the village.¹⁵⁵ Flowers' gender set her apart from her anthropological predecessor in the village in significant ways. And yet, despite the distinctions, Flowers realized almost immediately that community members were interpreting her in the context of their experience with the Englishman.

Flowers credited Maybury-Lewis, for example, with the fact that the community accepted her and her desire to move into the village rather than staying at the government post now run by the *Fundação Nacional do Índio* (FUNAI).¹⁵⁶ In the preface to her dissertation she wrote, “Fortunately the older people at Pimentel Barbosa accepted my request to live among them, because they remembered the visit of another anthropologist,

¹⁵⁴ Nancy M. Flowers, “Field Diaries, 1976–1977,” Papers of Nancy M. Flowers, Serviço de Gestão de Documentação–Museu do Índio/FUNAI (SEDOC–MI/FUNAI), Rio de Janeiro, 13. (Hereafter, “Field Diaries”).

¹⁵⁵ Maybury-Lewis, *Savage and the Innocent*, 269. Noviate adult refers to an age-set at the beginning of adulthood, where members do not yet have the full responsibilities of *predu* men.

¹⁵⁶ In 1968, the SPI was dissolved under wide suspicion of corruption and mismanagement. It was replaced with the *Fundação Nacional do Índio* (FUNAI), which continued to employ the same infrastructure, and in large part the same workforce.

David Maybury-Lewis, almost 20 years before. They understood that I was doing the same kind of work, and David, too, had insisted on living in the village. I also soon realized that he had set a model of appropriate behavior for anthropologists.”¹⁵⁷ At the most basic level, interactions between villagers and the Maybury-Lewis family established a precedent for inscription activities.

It was only Flowers’ third afternoon in the field when their influence became quite clear. As she sat in the shade by Apowẽ’s house for a few moments, a woman named Fernanda who would become one of Flowers’ closest friends, and “who remembered David Maybury-Lewis,” took it upon herself to teach a lesson in social organization. Although she had no command of Portuguese, interestingly, Fernanda chose to start by teaching the new *warazú* the names of the age-sets. Flowers carefully repeated back the names “*êtẽpá, tirowa, hõtörã, airere, sada’ro, anharowa, nozõ’u, abare’u,*” but despite the anthropologist’s best effort, the woman scolded her. Flowers noted in her field journal, “She said I should write them down right away like David always did, but I had my cameras with me and not a notebook. Very bad – one should always carry a notebook.”¹⁵⁸ Flowers was immediately understood to be practicing a peculiar type of work associated with the notebook, pencil, and constant scribbles of the fieldworker. Within three days of arriving in the village, she was expected to exhibit

¹⁵⁷ Flowers, “Forager Farmers,” ix.

¹⁵⁸ Flowers, “Field Diaries,” 10. Social and cultural anthropologists have described Xavante society as being organized around age-sets and age-grades in addition to moieties. The most recent discussion by Welch described age-sets as cohorts of individuals who move through hierarchical, sequential age-grades, or ranks. See Welch, “Age and Social Identity,” 24. Earlier work also discussed age and social organization in depth. See Laura Graham, *Performing Dreams: Discourses of Immortality among the Xavante of Central Brazil* (Austin: University of Texas Press, 1995), 91–102; Maybury-Lewis, *Akwẽ-Shavante Society*, 105–164; and Lopes da Silva, *Nomes e amigos*, 59–141.

the behaviors of the anthropologist: Xavante elders already had expectations of what researchers should do.

Flowers' interaction with Fernanda is salient for a number of reasons. First, it shows that almost immediately she was identified with Maybury-Lewis. This was true not only for village leaders and those with some competence in Portuguese, but also for members of their families who had no command of *warazú* language. Furthermore, for villagers, the anthropologist was not only defined by visible practices of questioning and recording responses; Xavante interlocutors were already sensitized to the content of interest for the anthropologist. Maybury-Lewis had been in the village to conduct a structuralist analysis of Xavante society, and as such he placed particular import on the social institutions organizing village life. The age-set and age-grade systems had been a primary interest. As part of a complex social system, it took him much time, questioning, repeating, and cross-referencing with missionaries and others outside of Wedezé to make sense of the system.¹⁵⁹ Fernanda made a peculiar choice when she began by teaching Flowers the names of age-sets. She tackled an abstract concept that had been of great interest to Maybury-Lewis rather than the vocabulary of daily life that could more easily be indicated by pointing. This choice emphasizes that at least some residents had a clear notion of what it was about Xavante culture that the anthropologist wanted to know. They had started to develop notions of how to portray themselves to a certain kind of visitor, and what they might present that would be of most value. Interactions with researchers

¹⁵⁹ There were no missionaries in Wedezé or in Pimentel Barbosa during Maybury-Lewis' and Flowers' stays, although some missionary linguists from the Summer Institute of Linguistics visited Pimentel Barbosa during Flowers' stay.

were thus subtly influencing how their informants fashioned themselves.

With repeated contact with *warazú* of all sorts, by 1976 villagers in Etênhiritipá were far more accustomed to hosting and relating to outsiders than they had been in 1958. While Maybury-Lewis may have underreported the presence of visitors as it would support his claims of having insight into an “unacculturated” village, the political and territorial reality *had* shifted greatly by the time Flowers arrived.¹⁶⁰ Almost twenty years of interactions with SPI and FUNAI representatives, visits from journalists and tourists, and much more extensive experience working on neighboring farms meant that villagers had a very different perception of their position in relation to *warazú*. While villagers had reportedly been puzzled and incredulous at Maybury-Lewis’ insistence to live with them and trek with them, by the time Flowers arrived they had new investments in building relationships with outsiders, especially those they thought might make good allies. One outcome of these changes included new familiarity with and approaches to teaching outsiders, whether lessons in social structure tailored to the anthropologist, or more general issues such as language tutoring.

While both Flowers and Maybury-Lewis struggled extensively to learn the Xavante language, their struggles reflected distinct issues. Maybury-Lewis, despite his

¹⁶⁰ Maybury-Lewis emphasized how dramatic the changes in the local reality were even from his first field season to his return in 1962 when he visited with a group of geneticists (see Chapters 2 and 3). “Many things had changed since 1958,” he would write, “The Shavante were no longer haughty in their dealings with outsiders. Their lands had been infiltrated. The population of the community was half its former size owing to epidemics and internecine warfare,” *Akwê-Shavante Society*, xiii. Even as geneticist James V. Neel understood his fieldwork experience as a glimpse into an untouched primitive past, Maybury-Lewis reported dismay at the changes that four years had wrought. His sentiment might be characterized as a mixture of despair at the changing circumstances of the people who had hosted him and what Renato Rosaldo has called “imperialist nostalgia.” See: Renato Rosaldo, *Culture & Truth: The Remaking of Social Analysis* (Boston: Beacon Press, 1993 [1989]), 68–87. Interestingly, in an interview, geneticist Francisco M. Salzano remembered tourists visiting São Domingos during their 1962 fieldwork. Francisco Mauro Salzano, interview with Rosanna Dent, 11 July 2012, Porto Alegre.

conversational skills in the closely related Xerente, another member of the Jê language family, described his efforts as plagued by difficulties. He commented that village residents “were little inclined for the tedium of instructing a foreigner in their tongue,” and that when asked for help to understand “they were not usually of much assistance, for they had no experience at that time either of translation or of paraphrase.”¹⁶¹ By stressing his struggles in language acquisition, Maybury-Lewis heightened the sympathies of his readers for the difficulties of his fieldwork and underscored his assertion that the group had little to no contact with Brazilians or foreigners. However, his scholarship depended on developing conversational competency. The authority of his account was ultimately tied to perceptions of his competent language skills, and so Maybury-Lewis described his progress over the pages of his account, subtly emphasizing his gift for language learning.¹⁶²

However, Maybury-Lewis never claimed complete fluency. In fact, he reported particular frustration at his difficulty understanding the formal discussion of the *warã*, the mature (*predu*) men’s council twice-daily meetings. At sunrise and sunset, the adult men came together to discuss the day’s activities and news. As Maybury-Lewis would describe it, their political debates often involved multiple individuals, simultaneously

¹⁶¹ Maybury-Lewis, *Akwẽ-Shavante Society*, xxi.

¹⁶² In his academic monograph, *Akwẽ-Shavante Society*, Maybury-Lewis described his own skills writing, “When I arrived in Brazil, I spoke fluent French, German, and Spanish, good Danish, and quite good Russian...” and went on to explain the difficulty of learning Xerente and Xavante “with nothing approaching either a grammar or a dictionary of the language,” (ibid., xiii–xiv). Many colleagues and students would comment on and remember Maybury-Lewis’ gift for languages. For example, see Wade Davis, “A Flash of the Spirit,” *Anthropological Quarterly* 82, no. 4 (2009): 1055–59. doi:10.1353/anq.0.0090; Yalman, “Remembering David.”

listening and responding to each other to create a collective voice.¹⁶³ With different cadences from day-to-day speech, Maybury-Lewis reported that the art of public speaking constituted a prized skill and marker of social status for those mature men who participated. It also presented major comprehension challenges. Maybury-Lewis was unable to make much sense of what they said:

When I first came among the Shavante I had been fascinated by the sound of their debates. Tonight I realized that that was all I could do and all I would be likely to do for a long while to come. ... The Shavante knew no other language into which they could translate and, what was worse, they were so unused to dealing with outsiders that they were incapable of putting one idea into different words for the benefit of foreigners. If I lost the thread of the speeches and nudged Urbepte to ask what was being said, he usually replied, “He is very angry,” or, “He talks very much.” Here was all this priceless information being flaunted before my nose every night and I was incapable of taking advantage of it.¹⁶⁴

Among the various techniques that Maybury-Lewis would employ included taking recordings with him to other Xavante communities, and discussing his findings at length with individuals from other villages who had more experience working with Summer Institute of Linguistics missionaries or by holding “seminars” with elders and younger, bilingual Xavante men who lived in São Marcos at a Salesian mission.¹⁶⁵ These individuals outside of Wedezé were essential for making sense of his material. At the

¹⁶³ In the early 1980s, Laura Graham conducted research on the socio-linguistics of Pimentel Barbosa. For a discussion of polyvocal speech and the impossibility of translating it, see Graham, *Performing Dreams*, 141–142.

¹⁶⁴ Maybury-Lewis, *Savage and the Innocent*, 219.

¹⁶⁵ Maybury-Lewis, *Akwê-Shavante Society*, xv.

time that Maybury-Lewis stayed in Wedezé, the villagers saw no real benefit or had little context for why they should be invested in Maybury-Lewis learning their language.

Flowers reported more acute difficulty with the language than Maybury-Lewis, both in her fieldnotes and in oral history interviews years later. Yet it is not an overstatement to say that she faced less daunting conditions, even allowing for some possible exaggerations in Maybury-Lewis' depiction of his field realities. The villagers engaged with Flowers in a different manner. Within days of arriving in the village, Flowers noted, "everyone seems to enjoy correcting my Xavante pronunciation and grammar."¹⁶⁶ By 1976 a number of Xavante individuals in the village spoke some Portuguese, and a few others were accustomed to the techniques necessary to teach a *warazú*. Flowers was able to work with another *warazú*, Basílio the "cowboy" as she called him, who had been hired by FUNAI to look after the village's small herd of cattle. He spoke Xavante well since he had grown up at the SPI post, and with his help Flowers was periodically able to explain what she was doing or interview individuals through his translation.¹⁶⁷ While it is difficult to gauge exactly how Maybury-Lewis' and Flowers' experiences compared, it seems that villagers had more interest and investment in outsiders learning their language by Flowers' time in the field. As discussed at greater length below, the political realities of the two moments were quite different. As the Xavante of Etênhiritipá faced arrogation of their land by settlers, and as they experienced the racism and prejudice that confronted those who went to work for local *fazendeiros*, they started to recognize that those who would stay and invest the time to try to learn the

¹⁶⁶ Flowers, "Field Diaries," 12.

¹⁶⁷ *Ibid.*, 80.

Xavante language were few and far between. At a moment when political allies were both scarce and crucially needed and as the Xavante became increasingly engaged with the state, they began to invest more effort in teaching the outsiders their language.¹⁶⁸

Recording Identities: Technologies of Inscription in the Field

Technologies of documentation were also deeply imbricated in the development of research subjectivities. In the time between Maybury-Lewis' fieldwork and Flowers' arrival in T.I. Pimentel Barbosa, attitudes towards recording technologies, whether photographs, or audio-recordings had shifted dramatically. The technologies—which were already crucial to the researchers' methods—would also become central to the potential of the researcher as a resource to the community. While Maybury-Lewis had struggled with his informants' resistance to technologies, Flowers' cameras and tape recorder came to constitute one of her most attractive features in the eyes of her hosts. In the political context that made it worthwhile to teach outsiders to speak Xavante, researchers' inscriptions in film, tape, and paper, came to be seen as a way to document and reproduce a certain vision of self for the consumption of the community and for the outside world. As this section shows, the act of recording both came to be seen as integral to the role of the researcher, and the act of being recorded made Xavante individuals into research subjects in a new way, blurring lines of agency and control and allowing

¹⁶⁸ While the initial interest in engaging with Flowers about Xavante language wore off rather quickly and she struggled to find support from her subjects in regards to her linguistic endeavors, the next researcher to spend an extended time in the field would confirm the energy and interest of certain members of the leadership regarding the documentation and circulation of Xavante language and discourse. Laura Graham found particular support for her project from Warodi, Apowê's eldest son, who put particular emphasis on her ability to write Xavante. Graham understood this in the context of his extensive interactions with Brazilian bureaucracy. See Graham, *Performing Dreams*, 12–13.

researcher and research subject to co-constitute one another.

As she settled into her rooms at the government post in the days before she had commissioned and moved into a small house in the village, Flowers set about unpacking her supplies. Among her materials were copies of Maybury-Lewis' two books, which she offered to Ismael Leitão and a group of young men who were at the post on the day after her arrival. She was hesitant, as she noted in her field notes, because "Some Indians don't like to be reminded of the dead," and so she observed anxiously as the group started to flip through the pages, turning directly to the photographs and beginning to identify the faces of family members.¹⁶⁹ Despite her worries, the books were a sensation, so much so that to avoid constant interruptions Flowers left them out on a table at the post where "About all the Xavante in the pictures are recognized -- some have found their own picture."¹⁷⁰ For the first time since Maybury-Lewis' stay, community members had access to the products of his research. And although the text was in English, they could now interact with the material products of his incessant scribblings. Without knowing what the books said, they were able to connect his work to the fact of publication, understanding the texts to circulate descriptions of Xavante ways of being to a wider, international audience. Their knowledge of what it meant to become a subject of research grew slightly richer.

Much as the images were the most interesting aspect of an otherwise still-unintelligible material legacy of Maybury-Lewis' visit, images would be essential to Flowers in the early days of her research. Flowers made it her first task to complete a

¹⁶⁹ Flowers, "Field Diaries," 7.

¹⁷⁰ *Ibid.*, 9.

photographic census of the village. She knew that if she was to make sense of hunting, fishing, food production, and distribution—the central variables she was interested in for her research—she would need to recognize each resident of the village and know which house they lived in. Despairing at how difficult she found Xavante names to be, Flowers thought that photographs would help. But despite their long history of interacting with journalists, initially the villagers resisted her use of the camera. Later she came to understand that this was because, “...visitors that had been in the village previously took many photos and promised to send copies back to those who had been photographed. But the copies never arrived.”¹⁷¹ Flowers, however, had a technological advantage over her predecessors. Her Polaroid camera “was a big success as pictures-in-a-minute were a novelty to the Xavante.”¹⁷²

On her second day in Pimentel Barbosa, she wrote “[I] visited Apowẽ’s house to announce that I wanted to photograph each family in the village by Polaroid -- one for them, one for me.”¹⁷³ With the gift of a photo as an enticement, Flowers’ picture project took off. “All I had to do was walk around the village,” she noted, “and people would call me emerging with their families in their best clothes for the picture taking.”¹⁷⁴ In fact, most of her census photos featured one member of the family holding a drying Polaroid, waiting for the chemical reveal of their family portrait. (See Figure 1.) Families were eager to be featured, as it gave them a chance to have their own image, to see themselves through the lens of the researcher, but also to present themselves in their best clothes, and

¹⁷¹ Nancy M. Flowers, *Entre os Xavante de Pimentel Barbosa: memórias fotográficas* (Rio de Janeiro: Museu do Índio-FUNAI, 2011), 16.

¹⁷² Flowers, “Field Diaries,” 4.

¹⁷³ *Ibid.*, 7.

¹⁷⁴ *Ibid.*, 10.

document their families in a medium that was normally beyond their reach. Flowers and her research project became a resource—at a very simple level—for community members to create and keep an image of themselves.

Another major aspect of technological inscription had also changed in the lead up to Flowers' fieldwork. Maybury-Lewis had complained that it was almost impossible to audio record his conversations with his interlocutors, writing, "The sight of my microphone, whether carelessly arranged close by or openly given to them to talk to, invariably dried them up completely."¹⁷⁵ Almost twenty years later, within her first week at the FUNAI post, Apowē arrived to Flowers' room and "chased everybody out... He then announced that he wanted to sing for the tape recorder. ... I hooked up the microphone and offered to hold it for him, but he took it out of my hand and held it himself, at the right distance and with a completely steady hand."¹⁷⁶ Flowers recorded the encounter in her field notes saying, "He sat gathering his thoughts for a few minutes while the machine ran, then he sang steadily for 20 minutes in a clear voice, one song after the other, sometimes speaking a few sentences in between. I played the recording back for him, and he seemed very pleased with his own performance."¹⁷⁷ It is difficult to know how much experience Apowē might have had in recording himself, but the overall excitement and nervousness of other villagers regarding the technology suggest the voice recorder was not a regular feature of village life.

Apowē knew he was being documented and was purposeful, choosing songs and

¹⁷⁵ Maybury-Lewis, *Akwē-Shavante Society*, xx.

¹⁷⁶ Flowers, "Field Diaries," 11.

¹⁷⁷ *Ibid.*

singing directly into the microphone. This was likely because, as Flowers noted, he “quite literally, like[d] the sound of his own voice,” but also because he saw some value in preserving his songs.¹⁷⁸ At one point early in Flowers’ stay, Apowẽ began to speak to her about Maybury-Lewis first asking her to write a letter, “telling him to come back soon to the Xavante because he (Apowẽ) was an old man and he missed David very much, so David should hurry. This was very touching,” Flowers reflected, “though the effect was spoiled slightly when Apowẽ added that David should also bring many good things for the Xavante as he did before: lots of fishhooks, sweets, cloth, knives, and so on.” Flowers had another suggestion: “I said perhaps Apowẽ would like to tape a message for David on the recorder. The idea pleased the old man greatly, and he spoke for about ten minutes, very expressively.”¹⁷⁹ Hearing himself, Apowẽ understood that his voice was made mobile and reproducible.

Flowers’ tape recorder quickly became one of her most compelling assets from the villagers’ point of view. The men of the mature age-grade (*predu*) repeatedly invited her to record and play back their discussions at the *warã*, providing her with an entrée where women generally were not allowed.¹⁸⁰ Hearing the recordings of the men, Flowers noted, “Isabel didn’t want me to get the idea that only the men sing, so she assembled several women to show me they could too.”¹⁸¹ Everybody wanted to hear the recordings, particularly those that documented important ceremonies. The recorder and the researcher who carried it enabled members of the community to revisit essential social moments,

¹⁷⁸ Ibid., 13.

¹⁷⁹ Ibid., 18.

¹⁸⁰ Unfortunately, even replaying the tapes of the *warã*, Flowers still found it difficult to make sense of the discussions.

¹⁸¹ Flowers, “Field Diaries,” 13.

analyze them, and create further layers of meaning through their discussion.

Flowers sometimes despaired that she would never be able to escape the nightly “concerts” of the recordings, held in her home and at the expense of her stock of batteries. The influence, however, as Flowers played the tapes back to the vocalists, was to bring a new awareness of how the documentation worked, and how each person or group of people could participate in the process of inscribing their voices on magnetic strips. Watching the men sing for the recorder and be inscribed as subjects of Flowers’ collecting project inspired the women to assert themselves for inclusion. They found it important to prove to the *warazú*—in between bouts of nervous giggling—that they could keep pace with the men.¹⁸² The act of recording in the context of research interactions contributed to Xavante individuals’ self-conscious sense of the mobility of sound and image. Thanks to Flowers’ long stay in the village, this was true not only for the leadership and those men who had experience traveling to town and interacting with government agents and *fazendeiros*. It also applied to children, women, and men who had less exposure to the technologies of the *warazú*. This was a new sense of self, hearing ones’ own voice, recording messages for delivery beyond *terra indígena*.

There had been a substantial shift in how the Xavante understood the recording of speech and image, not only in T.I. Pimentel Barbosa, but throughout Xavante communities. Not all members of the village were happy with Flowers’ photographs. One of Surupredu’s brothers, who was perceived by the staff at the post as having mental health problems, became agitated about Flowers’ photography early in her stay, and

¹⁸² Ibid.

“suddenly decided that he wanted no picture taken either of himself or his wife.”¹⁸³ But overall, mistrust gave way to fascination, enjoyment, and embrace of these technologies on the part of many villagers. And yet these shifts were not simply toward leisure and entertainment: inscription, inscription technologies, and the people who used them (especially *warazú* researchers) became a political resource. As Brazilian society expanded and settler-colonists occupied Xavante lands, documentation, recording, and writing had taken on new gravitas for communities. *Fazendeiros* and questionable government agents consistently undermined their claims to ancestral land, and Nancy Flowers’ stay coincided with a period of particularly active mobilization to pressure the government for accurate demarcation and adequate support.¹⁸⁴

Witnessing and Suffering with: Fieldwork in Fraught Times

As previous scholarship has eloquently discussed, since the 1930s, Central Brazil had been targeted as one of the “unsettled” regions of Brazil most ripe for the expansion of agribusiness.¹⁸⁵ The Xavante constituted a formidable obstacle in government plans to settle Mato Grosso, as they fought off invading ranchers, missionaries, and government

¹⁸³ Ibid., 12. Photographs have the potential to be used for sorcery, and so at various moments Xavante subjects have resisted photography or requested the limitation of circulation of photographs to other villages during difficult political moments.

¹⁸⁴ Garfield, *Indigenous Struggle*, 199; Graham, *Performing Dreams*, 37–42 and 44–55. Few figures better encapsulate Xavante creativity and politics in the use of recording equipment than Mário Juruna of *Terra Indígena São Marcos*. Juruna made headlines in the late 1970s and early 1980s using a tape recorder to document the promises of government officials from FUNAI and other institutions and then expose their blatant deception. In 1982, Juruna was elected congressman (*diputado federal*) for the state of Rio de Janeiro, the first and until today only Indigenous member of the House or Senate in Brazil. See Mário Juruna with Antônio Hohlfeldt and Assis Hoffman, *O gravador do Juruna* (Porto Alegre: Mercado Aberto Editora e Propaganda Ltda., 1982).

¹⁸⁵ Otávio Guilherme Velho, *Capitalismo autoritário e campesinato* (Rio de Janeiro: DIEFL 1976); Garfield, *Indigenous Struggle*.

employees until 1946.¹⁸⁶ By time of Flowers' arrival, however, the reality had shifted markedly: "This country is developing very fast," she wrote, on the third day of her journey from Brasília to the village, "mechanized rice growing, large farms, heavy trucks on dirt roads that weren't meant for that kind of traffic."¹⁸⁷ As Flowers observed during her time in the field, the 1960s and 1970s saw repeated attempts by government officials, first of the SPI and subsequently by FUNAI, to "civilize" the Xavante, turning them into sedentary agriculturalists and in the process freeing up the land that they traditionally relied on for hunting and collecting.¹⁸⁸ In 1976 the Xavante of Pimentel Barbosa were newly engaged in extensive rice cultivation, part of a project implemented by FUNAI.¹⁸⁹ However, unsatisfied with the poor infrastructure, inadequate training, and insufficient supplies, they were not easily wooed to abandon their ways of life in favor of rice farming. Encroaching *fazendeiros* further aggravated the community, and during Flowers' stay, the Xavante of T.I. Pimentel Barbosa actively patrolled their lands, evicted squatters, and traveled to Brasília to contest the boundaries to their territory inaccurately recorded by FUNAI.

Arriving at a particularly tense moment, Flowers was privy to the Xavantes' ongoing struggles to protect their land and to lobby for the health care and education that had been promised. Within days of her arrival, Flowers observed a local *fazendeiro* arrive by light aircraft to offer gifts and promise to be "good neighbors." "It so happened that

¹⁸⁶ Garfield, *Indigenous Struggle*, especially 45–65.

¹⁸⁷ Flowers, "Field Diaries," 2.

¹⁸⁸ From 1975 on, FUNAI attempted to implement various community development projects in different Xavante *Terras Indígenas*. For an in-depth discussion of these projects, and particularly of the high input agricultural project centered in T.I. São Marcos, see Garfield, *Indigenous Struggle*, 187–211. See also the introduction to Flowers, "Forager Farmers."

¹⁸⁹ Flowers, "Forager Farmers," 217–284.

the Xavante men had just gotten painted up for a log race, so Surupredu and the others looked quite impressive,” Flowers noted with satisfaction, going on to say “I talked briefly to the *fazendeiro* who said that what the Xavante needed was more mechanization, more cattle, and less land.”¹⁹⁰ Although not explicit, Flowers’ skepticism was palpable in her phrasing. The leaders who spoke with the visitor “were not impressed,” she wrote, but they maintained polite composure.

Even in her earliest days in the village and through the language barrier, Flowers could see the difficult position that the community faced. Fenced in with increasingly disrupted access to game and wild produce, facing a heavy burden of disease, they had little choice but to maintain civil interactions with their *warazú* neighbors. The same *fazendeiros* who sought to move boundary markers and redraw maps also employed men from the village as laborers. Lack of resources in the village and the understaffing and inconsistency of FUNAI meant that the community often depended on nearby farms to transport sick villagers to town for medical treatment, or to radio for support since the post’s transmitter rarely functioned. Flowers’ field notes are punctuated with visits from *fazendeiros*, notes about the corruption of previous FUNAI officials who traded land for trucks and cattle, and comments about how the Xavante were modifying their hunting and gardening practices to secure their territory, more effectively occupying their land by spreading out to the edges of their territory.¹⁹¹ She was acutely aware of the challenges.

Flowers’ research agenda in human ecology also drew her attention to core issues

¹⁹⁰ Flowers, “Field Diaries,” 9.

¹⁹¹ *Ibid.*, 71.

of concern to the community.¹⁹² Flowers arrived in Pimentel Barbosa to participate in a comparative study of “Central Brazilian” societies that directly responded to the work of Maybury-Lewis and his students.¹⁹³ Pushing back against a structuralism that posited an isolated system of largely coherent internal meaning, the four fieldworkers would pay attention to the material conditions of life and work in the context of socio-economic change. Flowers was interested in food production and consumption, child growth curves, and reproductive histories. With her language limitations, she worked primarily with issues that she could observe, measure, and code without in-depth discussion. She completed time allocation studies, questioning families about the activities of each member at a given time and on a given day. In other activities that marked a somewhat bewildering addition to the activities of the anthropologist from the point of view of her subjects, Flowers went to great lengths to try to weigh and tabulate the food consumed by specific households during 24-hour periods. Since a central aspect of Flowers’ research focused on nutrition and food production, she paid close attention to how much food was coming into Xavante households and how productive garden plots and hunting trips turned out to be—a major shared interest with her hosts. Flowers’ own nutrition during her time in the village was only slightly more stable than that of her neighbors’, and so her interest in food was not purely academic. She relied on friends and informants for gifts of game such as deer, tapir, and wild pig, and coveted small treats that she brought

¹⁹² Under the guidance of her advisor, Gross, Flowers and three colleagues were to complete year-long studies. From CUNY, Flowers was joined by colleagues Dennis Warner, who went to work in a Kayapó-Menkrangnotí village, and Madeline Ritter, who spent 14 months in a Kanela (Ramkókamekra) community. Additionally, Brazilian student Maria Francisca Almeida Leoi worked in a Bororo village.

¹⁹³ Maybury-Lewis and Flowers did not correspond prior to her fieldwork—there was no relationship of mentorship between them. However, after her return they corresponded and in addition to encountering one another in the professional circuit of US anthropology, also met in the field in Pimentel Barbosa in 1990.

for herself from town. “I must say,” she noted, “I spend a lot of time thinking about food when I am down to rice and beans.”¹⁹⁴

Gradually, members of the village of Pimentel Barbosa came to understand that Flower’s interests were not exactly the same as those of Maybury-Lewis. While the English anthropologist struggled with resistance to his questions about sorcery, factionalism, and violence, Flowers faced the more practical problem of women whisking food away to be prepared or serving their families without giving her a chance to record the mass of rice or tapir to be consumed.¹⁹⁵ Flowers described the struggle to document who was eating and how much, especially since game and food were shared between households with such frequency and the women, who prepared the food, were incredulous as to why she wanted to weigh all the food before it was consumed.¹⁹⁶ But even as they sometimes protested against her incursions, again they came to accept her research, at times begrudgingly and at times with a marvelous sense of playfulness.¹⁹⁷

Some of the older men, particularly those who were leaders in the village, came to advocate for Flowers. On one occasion early in her stay, Apowẽ guided her away from the *warã* when tensions ran high, keeping her out of trouble when one of the other men objected to the presence of a woman and leaving her at her house with a “grandfatherly

¹⁹⁴ Flowers, “Field Diaries,” 78.

¹⁹⁵ Maybury-Lewis described the difficulty of getting villagers to open up for “talks of murder and intrigue, haltingly and surreptitiously told,” saying “I was left under no illusions as to the fact that this was dangerous information which was being imparted. My informants wriggled with embarrassment and lowered their voices. Sometimes they refused to speak in the presence of their wives, sometimes even in the presence of their children.” Maybury-Lewis, *Savage and the Innocent*, 246.

¹⁹⁶ See Flowers, “Field Diaries,” 80–81, 86–89.

¹⁹⁷ Flowers, “Forager Farmers,” x–xi.

hug.”¹⁹⁸ At a particularly frustrating moment during a food intake study, when the women began to eat without allowing Flowers to weigh the food, the anthropologist performed her anger to make it clear how upset she was: “Partly out of genuine annoyance and partly to show my feelings,” she wrote, “I threw my pencil and notebook on the ground and stalked off, leaving the tape recorder running.” It was one of the older men who went after her to return her things. Another invited her in when she came back to the home to finish her work, calling her “sister,” and offering her the goods he had brought back from town so she could weigh and tabulate them “by way of making amends.”¹⁹⁹

Flowers was often looked after or called back by the men who were prominent leaders. This suggests something more than simple empathy on their part. Much as Maybury-Lewis had been “adopted” by Apowẽ, prominent men seem to have been Flowers’ most vocal supporters and advocates. And while it may have been due to their positions of authority and sense of having to look after their households, it may also reflect a developing sense that researchers ought to be cared for, because their presence was not simply a nuisance. As the recipients and re-distributors of gifts, they were most likely to benefit from the prestige of receiving and strengthen their positions in the village through redistribution. These leaders were also the most politically active members of the village, who engaged, negotiated with, and intimidated SPI personnel and local *fazendeiros* alike. The fact that academic visitors could offer potential benefit to the community was becoming increasingly clear, particularly for the men who cultivated a

¹⁹⁸ Flowers, “Field Diaries,” 16.

¹⁹⁹ *Ibid.*, 87.

sense of their role as protectors to the outsiders, even as younger members of the community might speak more fluent Portuguese. Gender dynamics may also have contributed; as a woman alone, Flowers was unusual in the eyes of her hosts. Her gender and age also allowed her to visit anyone, regardless of moiety or adoptive kinship, making it possible for her to transit Xavante homes in a way Maybury-Lewis never could.

Beyond access to food and nutrition, Flowers' research also drew her attention to child health. She was documenting child survival and growth, and so spent time measuring and weighing infants and children, and interviewing their mothers about their reproductive histories.²⁰⁰ Flowers' focus on health meant she kept careful note of each infant that died in the village. Her research interest, although quantitative and reported in technical and somewhat dispassionate language in her academic writing, was something she felt deeply. She mirrored a desperate concern in the village regarding the rapid rise of infant mortality. Maybury-Lewis witnessed two deaths during the duration of his stay in Wedezé in 1958, both of young children; Flowers witnessed three infant deaths in the space of two weeks during August of 1976.²⁰¹ Flowers' daily entries sadly noted these deaths, and recorded numerous attempts to help by gifting money for gasoline and paying for families' transportation to town, despite the fact these desperate trips were usually

²⁰⁰ Awkwardly, Flowers had to do these interviews through the male interpreters like Basílio, the local *warazú*-cowboy who had grown up in Xavante territory. Flowers, interview with Rosanna Dent, 23 August 2013, New York City.

²⁰¹ Maybury-Lewis, *Akwẽ-Shavante Society*, 280. These reported witnessed deaths do not include the murder that Maybury-Lewis reported occurring while he was on trek, since he was not with the group where it occurred.

insufficient to save the ailing children.²⁰² During her fourteen months, Flowers observations about child mortality showed that on average Xavante parents were seeing half of their children die due to disease.²⁰³

Flowers' own concern, investment, and grief were very real and apparent to her informants. Two and a half months into her stay, the village was hit with a major outbreak of flu. Contracting the virus herself, Flowers lay miserable in her hammock, and noted the lack of food, medicines and attention from FUNAI. During these quiet days with "the silence broken only by the sound of coughing," Flowers made an unplanned trip to town to stock up on children's aspirin, and offered what little she knew about health care to her neighbors in the village, which the Xavante were enthusiastic to combine with existing curatives.²⁰⁴ Flowers, through her extended time in the village, participated in the challenges and sorrows that her hosts experienced. Although always privileged with the funds and support to leave the village in case of her own illness, Flowers, at least to some extent, suffered with the village. Flowers' presence at a time of great difficulty meant that she was seen in a different light from Maybury-Lewis. Flowers witnessed their pain at a critical moment for the village.

Community members were also beginning to see Flowers and other researchers as a potential source of help. Her presence at a critical moment for land claims and health catalyzed thinking about what researchers might be able to offer. She arrived at a time when both elders and younger leaders were developing polyvalent political strategies. By

²⁰² Flowers, "Field Diaries." For example, Flowers witnessed deaths on August 14, 20, and 26, 1976.

²⁰³ Coimbra Jr. et al., *The Xavante in Transition*, 132–133.

²⁰⁴ Flowers, "Field Diaries," 52–53.

the 1970s, village leaders had recognized the importance of their children learning Portuguese and mathematics to be able to contest unfair payment, debate documentation containing false information, and lobby government agencies. A cohort of boys were studying with the *warazú* in Goiânia and Ribeirão Preto, in hopes that the education would pave the way for the young men to navigate both Xavante and *warazú* politics.²⁰⁵ Villagers in Etênhiritipá had always made demands on outsiders who visited, expecting and giving gifts. But in the period between Maybury-Lewis and Flowers' visits, they began to imagine potential for their interactions with researchers that went beyond material exchange relations. They came to view visitors as a resource to address a wide variety of issues.

Flowers had a more pessimistic vision of her potential. She explained her limited ability to help, writing, "Mothers would bring me their sick children, and often I had neither the medicines nor the knowledge to cure them. Fathers brought me their sons and asked me to teach them Portuguese and arithmetic... When I went to Brasília the elders, who believed me to have power that I knew I did not, demanded that I prod the government agency to secure their lands."²⁰⁶ Leaders were hopeful that Flowers could help them, but their hopes seemed naive to her, expectations she could never meet. Remembering her experiences in a 2013 interview, she described how "Warodi would say, 'well when you get back to the United States you tell your president...'" and trailed

²⁰⁵ A documentary film follows the experience of a number of young men who were sent to Ribeirão Preto for school. See "Estratégia Xavante," directed by Belisario Franca (Rio de Janeiro: Giros and IDETI, 2006), DVD.

²⁰⁶ Flowers, "Forager Farmers," xii.

off chuckling at the memory.²⁰⁷ Although Warodi and others were still unsure about the limitations of the researcher's power, they were beginning to conceptualize what a sympathetic *warazú* might be able to do. This realization was not uncoupled from the perception of what the *warazú* might be interested in learning about. So a link began to form between the interests of visitors like Maybury-Lewis or Flowers and the interests of the hosts. If outsiders were particularly keen to understand and photograph rituals and aspects of life understood to be distinctive or traditional to the Xavante, villagers could draw upon and cultivate those interests to enroll the researcher for political work that might help protect distinctive practices in the long run.

Flowers left the field feeling unable to provide the help and support desired of her. She closed the preface to her 1983 dissertation writing, "I think my greatest frustration came from my inability to help the Xavante as much as they helped me by instructing me and showing me their way of life. ... When I left to go home, Apowẽ's eldest son made a speech for my tape recorder, urging me to write about the Xavante 'So your people will know.' That, at least, I have tried to do."²⁰⁸ Flowers doubted her ability to make a difference, especially according to the grand plans and expectations that her hosts held for her. However, Flowers' connections to Etênhiritipá were not over. She settled back into life in New York and slowly worked towards completing her dissertation. Meanwhile, anthropology as a field was undergoing far-reaching changes, with new imperatives being articulated that went beyond simply being "morally and

²⁰⁷ Flowers, interview, 23 August 2013.

²⁰⁸ Flowers, "Forager Farmers," xii.

physically” present in the research context.²⁰⁹ As discussed in Chapter 5, fifteen years later upon returning to Brazil, Flowers would initiate a new collaboration, this time with Brazilian researchers, which would take her back to Pimentel Barbosa and establish a long-term engagement of exceptional productivity, both academic and political.

Conclusion

The interactions of the two researchers and Xavante villagers described here were uneven, sometimes affectionate and friendly, occasionally fraught with tension and reprimand. However, over the span of time from Maybury-Lewis’ arrival to Flowers’ departure, ideas about researcher and researched were formed, shaped, and enacted in the daily labor of fieldwork. Moreover, the research subjectivities that were evolving informed collective identity, shaping the *warazú* as members of the anthropologists’ tribe and shifting Xavante notions about what aspects of their lives might be most interesting to outsiders.

As Apowẽ, Surupredu, Isabel, Fernanda, Warodi, and other villagers began to

²⁰⁹ For a cogent analysis of the periodization and areas of “crisis” that resulted in the transformation of US anthropology, see Matti Bunzl, “Anthropology Beyond Crisis,” *Anthropology and Humanism* 30, no. 2 (2005): 187–95, doi:10.1525/anh.2005.30.2.187. João Pacheco de Oliveira argues that Brazilian anthropology, specifically the anthropologists to participate in the *Associação Brasileira de Antropologia* (ABA, the Brazilian Anthropological Association) share a certain “style of political action” [*um estilo de ação político*] that is intimately related to the history of indigenist anthropology and activism. ABA, as Pacheco de Oliveira documents, became much more active at cultivating this *estilo* from 1974 onwards, after major disruptions in the possibility of holding professional meetings during the earlier period of the military dictatorship (1964–1985). However, ABA as an organization did not take action intervening in state indigenist affairs until 1980. See João Pacheco de Oliveira, “Uma ABA Indigenista” in *Associação Brasileira de Antropologia: 50 anos*, ed. Cornelia Eckert and Emília Pietrafesa de Godoi (Florianópolis, SC: Nova Letra, 2006), 71–78. Contributors to Deborah Poole’s *A Companion to Latin American Anthropology* (Malden, MA: Wiley-Blackwell, 2008), especially “Indigenous anthropologies beyond Barbados” by Stefano Varese, Guillermo Delgado, and Rodolfo L. Meyer (375–398) provide broader context for changes in anthropological practice across the region. Also see Chapter 4 for a more extensive discussion of this context and the notion of *antropologia militante* or “militant anthropology” that emerged in Brazil during the military dictatorship.

imagine what productive futures might come out of interactions with researchers like Maybury-Lewis and Flowers, they also crafted a collective vision of self that they could present to other curious outsiders. This crafting, from the careful perception of *warazú* interest in age-sets to a sense that living in the village or witnessing ritual practices was central to the work of the notebook-wielding visitor, worked not only to create a niche for academic investigation. In the process, the Xavante created and reinforced certain notions about what made them unique. They started to conceptualize these ways of being as a resource to enroll Flowers to petition “her president” for the protection of their lands.

Power dynamics between researcher and Xavante were undeniable: Maybury-Lewis and Flowers had access to material wealth, mobility, and political protection in great excess compared to the villagers who hosted them. But details of their stories illuminate not only the confrontational nature of resistance but also the exercise of other kinds of agency. Biehl and Moran-Thomas have written that “subjectivity does not merely speak as resistance, nor is it simply spoken (or silenced) by power. It continually forms and returns in the complex play of bodily, linguistic, political, and psychological dimensions of human experience, within and against new infrastructures and the afflictions and injustices of the present.”²¹⁰ The research subjectivities that evolved out of interactions were the result of a “complex play” of multiple experiences: Maybury-Lewis’ exhaustion or the laughter as he was “cast as camp jester”; the genuine affection of community members for his son Biorn combined with his doubt that he was anything more than a source of gifts; Flowers’ experience of hunger and her gratitude at being

²¹⁰ João Biehl and Amy Moran-Thomas, “Symptom: Subjectivities, Social Ills, Technologies,” *Annual Review of Anthropology* 38 (2009): 274.

offered a piece of meat by one of the villagers combined with her loneliness and difficulties communicating; her sense of helplessness in the face of the flu epidemic. These experiences were imbued with the changing socio-political and economic contexts, and so the role Xavante subjects envisioned for researchers shifted as the afflictions and injustices of the settler state extended ever more pressingly into Xavante territory.

During her fieldwork at the end of the 1970s, Flowers did not see how she could fulfill the demands of her hosts as a political advocate, even as the tide of anthropology was turning toward more visible activism. But her interlocutors had ideas about how particular performances of identity might constitute a magnet for resources. Xavante actors, witnessing the interest of anthropologists, began to see certain aspects of their lives as part of what they would soon call “*cultura*,” using the Portuguese word. These practices and ideas could attract the interest of outsiders, who although still part of colonizing society, might be more sympathetic and more useful than the *fazendeiros* and other settlers encroaching on their territory. They were beginning to mobilize performances of identity or “Xavante culture,” both in their territory and in the offices of public officials in Brasília. The early publications and the great depth of data that Maybury-Lewis, Flowers, and others created between 1958 and 1977 created a kind of intellectual infrastructure that would enable research at Wedezé and Etênhiritipá; it would constitute a major draw to future scholars. At the same time, the residents of this Xavante territory were building their own systems to interpret, enroll, and manage the *warazú* that would come to study them.



Figure 1. The first picture from Flowers' photographic village census of 1976. Apowẽ stands in the middle, holding his copy of the Polaroid in his right hand. From Flowers, *Entre os Xavante*, 27. Copyright Nancy Flowers, used with permission.

Chapter Two

Fission-Fusion: Interdisciplinarity in the Human Geneticists' Tribe

Introduction: That Very Important Fifth Man

“I hope it will soon be possible to locate that very important fifth man, the anthropologist or other person well acquainted with whatever tribe or tribes of Indians we propose to contact,” geneticist James V. Neel wrote on a dreary March afternoon in Ann Arbor. Penning a letter to his Brazilian colleague, Francisco M. Salzano, Neel emphasized, “It seems to me that he is the key person in the project.”²¹¹ They had been searching for the final member of their expeditionary team throughout the early months of 1962. Preparing for a period of interdisciplinary research in Central Brazil, the two researchers were in dire need of a socio-cultural anthropologist.

The geneticists' search for a social scientist was part of an ambitious research plan. They proposed to work with scholars from diverse disciplinary backgrounds to comprehensively document an Indigenous village over the course of ten days in the field. Their joint research agenda posited that to characterize the genetic population structure of a “relatively unacculturated” Indigenous group, they would need to merge disciplinary approaches. Imagined as both closer to nature and more deeply marked by cultural traits, Indigenous groups represented a resource and a challenge to the growing field of human

²¹¹ James V. Neel to Francisco M. Salzano, 20 March 1962, Salzano Correspondence (1 of 10), Box 66, Papers of James V. Neel – Manuscript Collection 96, American Philosophical, Philadelphia (hereafter Neel Papers, APS).

genetics.²¹² The lead investigators prioritized working with colleagues in physical anthropology, medicine, and social anthropology in order to characterize what they considered “parameters of genetic interest.” This chapter asks, why did the geneticists understand an anthropologist as “the key person” for their study in 1962? What was the imagined and actual role of the social scientist in the human genetics project? And how did their initial work with the Xavante become an enduring model that would shape the careers of the two geneticists, and the wider field of human genetics?

In this chapter, I explore the underlying rationale and implemented reality of the geneticists’ first interdisciplinary study, conducted in Wedezé in 1962. The perceived need for interdisciplinarity was intimately connected to colonial and post-colonial histories, the Cold War moment, and prevailing notions about Indigenous peoples. They worked to turn the Xavante villages that they visited into coherent populations, from which they sought to glean generalizable knowledge. The Xavante would come to stand in both for other Indigenous groups and for prehistoric ancestors.

In order to create this comprehensive and generalizable profile of a population, Neel and Salzano prioritized work with scholars from distinct academic fields, as well as the challenges of implementing a vision that crossed disciplinary boundaries. Their final six-man team included Harvard social anthropologist David Maybury-Lewis, Rio-based hematologist Pedro Clovis Junqueira, German physical anthropologist Friedrich Keiter, and research assistant Girley Simões. Each member of the assembled group offered both

²¹² For a series of pieces exploring the nature/culture divide in the field of genetics, see Alan H. Goodman, Deborah Heath, and M. Susan Lindee, eds., *Genetic Nature/Culture: Anthropology and Science beyond the Two-Culture Divide* (Berkeley: University of California Press, 2003).

practical and epistemological contributions to the ambitious project; I show that the academic benefits and responsibilities of this project were unevenly shared. Furthermore, the vigorous promotion of and attention to interdisciplinary collaboration served to obscure other power relations—those structured by divides between the “First” and the “Third World,” expert and lay knowledge, and researcher and research subject.

The Geneticists’ Initiative

Neel and Salzano had begun discussing the possibility of collaboration a number of years before their search for a socio-cultural anthropologist. From 1957-1958 Salzano completed a Rockefeller Foundation Fellowship for post-doctoral training under Neel’s supervision at the University of Michigan. It was during this period that the two men discussed the great potential they saw in research on Brazilian Indigenous populations.²¹³ Looking back on the choice in 2012, Salzano described Neel as the primary advocate: “Neel said to me, ‘what is the study population that you can do better than anyone else in any other part of the world? It’s the Indians, the Brazilian Indians. You’re geographically closer and have the facilities. A foreigner who wanted to do that work would have more difficulties.’ And so the first study in Amerindian populations was planned, and as soon as I returned I started to work with populations here in the South of Brazil.”²¹⁴ Upon returning to the *Universidade Federal do Rio Grande do Sul* (UFRGS), Salzano began studies of blood groups in Kaingang communities of Southern Brazil. Neel, for his part,

²¹³ Salzano to Rockefeller Foundation, “Report of Francisco M. Salzano,” Folder 305E, Record Group 10.1, Rockefeller Foundation records, Rockefeller Archive Center.

²¹⁴ Salzano, interview 11 July 2012.

first publically articulated the potential he saw in populations he referred to as primitive in 1958, shortly after Salzano's departure from Ann Arbor.²¹⁵

Through the study of populations that maintained lifestyles classified as “primitive,” “hunter-gatherer,” and “traditional,” Neel and Salzano hoped to cast light onto human pre-history. “The existing hunting and gathering groups presumably represent man's population structure until very recent time,” Neel would explain later to the American Society of Human Genetics. And yet he lamented the lack of “extensive and accurate” data on demographics, anthropometrics, reproductive histories, consanguinity, and genetic variation necessary to understand the distant human past.²¹⁶

The geneticists believed that collecting such comprehensive data required more expertise than a team of geneticists could offer. Such precise information about the factors that influenced the evolution of Indigenous populations could only be gathered, as Salzano suggested at a conference on the biology of the Amazon region, through “the work of an eminently interdisciplinary group.”²¹⁷ Drawing on published work from distinct disciplines alone was insufficient for the geneticists to complete their analyses. In the years leading up to their fieldwork, both Neel and Salzano worked their way through a selection of literature on lowland Brazil, contemplating the form that future research might take. In his autobiography, Neel remembered the period writing, “During 1960-1961, my thoughts as to what could and should be done marinated in a rich stew of very mixed anthropological reading. But while the stew was flavorful, it very quickly became

²¹⁵ James V. Neel, “The Study of Natural Selection in Primitive and Civilized Human Populations,” *Human Biology* 30, no. 1 (1958): 43–72.

²¹⁶ James V. Neel, “Between Two Worlds,” *American Journal of Human Genetics* 18, no. 1 (1966): 12.

²¹⁷ Francisco M. Salzano, “Estudos de biologia humana na Amazônia - retrospecto e perspectiva,” *Atas do Simpósio sobre a Biota Amazônica*, 2 (1967): 203.

apparent that the data collected by the Social and Physical anthropologist had simply not been the data the geneticist requires for any very precise approach to the subject of the dynamics of human evolution.”²¹⁸ And yet, even if the social and physical anthropologists’ data were not right for precise genetic studies, they also were not disposable. The very first objective for the 1962 pilot study was to “to identify those cultural elements with particularly biological implications,” and the second, “to obtain as complete a pedigree of a Xavante village as possible.”²¹⁹ The social anthropologist would be essential to these tasks. Furthermore, the geneticists needed a physical anthropologist for anthropometric studies. Salzano as geneticist would collaborate on creating genealogies. Neel as geneticist and physician would complete the physical examinations, while Girley V. Simões, Salzano’s field assistant, would collect biosamples of blood and urine, and help with a multitude of other tasks.

Each researcher would bring their particular methodologies and tools to the project. Nevertheless, according to the vision that the geneticists articulated, it was not a simple division of tasks where each expert would be responsible for the analysis of their own data. “This would be a very different sort of undertaking from the traditional fieldwork of the single cultural or physical anthropologist, or the dash of a geneticist to a remote area to obtain some blood samples,” Neel wrote looking back on the design of their study years later. “Central to this plan,” he continued, “was a close-quarters interaction between diverse disciplines out of which would either emerge an exciting intellectual interaction

²¹⁸ James V. Neel, *Physician to the Gene Pool: Genetic Lessons and Other Stories* (New York: J. Wiley, 1994): 119.

²¹⁹ James V Neel, Francisco M Salzano, Friedrich Keiter, David Maybury-Lewis, and Pedro Clóvis Junqueira, “Studies on the Xavante Indians of the Brazilian Mato Grosso,” *American Journal of Human Genetics* 16, no. 1 (1964): 53.

and synthesis or mutual rejection, which in the field is not so easily handled as at home.”²²⁰

In a short piece examining the history of the Institute for Advanced Study, Renato Rosaldo described two forms of interdisciplinarity. One approach, which Rosaldo qualified as “outdated,” is motivated by “the notion that [one] can master the knowledge of different disciplines and that each discipline contains truths of findings that do not require critical assessment.”²²¹ The second approach “uses each discipline as a corrective or supplement to the others,” seeking consensus among diverse fields.²²² The Xavante Pilot Study had aspects of both systems. The geneticists understood anthropological knowledge as a crucial contextualization to understand the biology that interested them, and they hoped including various different approaches towards one population of study would provide for a nuanced approach to the interactions of culture and nature. However they also worked to master the basic concepts of socio-cultural anthropology, which they would apply and use in future studies.

Interdisciplinarity and Indigenous Nature-Culture

The prominent role of interdisciplinarity in the Neel-Salzano research agenda is

²²⁰ Neel, *Physician to the Gene Pool*, 121. Even at the time of proposing and executing the initial research, Neel was aware that interdisciplinarity was not always either simple or successful. In his 1966 address to the American Society for Human Genetics, he said, “And lest I be accused of going overboard for team research, let me recognize clearly that too much of team research seems dominated by the concept that if one confused man can’t solve a problem, then perhaps six equally confused men working together can. There is, to be sure, a finite probability that they can combine their respective limping insights into a moment of truth. There is also a finite probability that they can combine their respective confusions into a catastrophic mess.” (Neel, “Between Two Worlds,” 16.)

²²¹ Renato Rosaldo, “Reflections on Interdisciplinarity,” in *Schools of Thought: Twenty-Five Years of Interpretive Social Science*, ed. Joan Wallach Scott and Debra Keates (Princeton: Princeton University Press, 2001), 71.

²²² Ibid.

linked to two key threads of the mid-twentieth century human sciences. First, the Xavante Pilot Study and the subsequent transnational research Salzano and Neel would conduct on Indigenous groups throughout South America were part of a broader body of work that found unique value in the study of Indigenous groups. Researchers from fields as diverse as genetics, psychology, and human ecology thought Indigenous bodies, families, and communities could inform understandings of the right relationship of humans to the natural world. The emphasis that the geneticists placed on incorporating social and natural science in the construction and study of Indigenous populations fits into a longer trajectory of knowledge production about Native people, both in transnational and immanently national contexts. Secondly, the scientists' promotion of interdisciplinarity was conditioned and rewarded by pervading priorities in natural and social science research. In this section and the next, I explore the broader contexts in which Neel and Salzano articulated and won support for their research agenda.

The concept that Indigenous people are in some way saturated with scientific data stems from a long Western European intellectual tradition. Explorers, ethnologists, and anthropologists have measured and categorized groups, whether under colonial rule or occupying land targeted for settler colonialism. Classified as “primitive,” Native people have repeatedly been linked discursively to the past, and understood to occupy a different temporality from that of their “civilized” observers.²²³ In twentieth-century

²²³ Fabian, *Time and the Other* is a classic study on temporal distancing in anthropology, including analysis of how the “ethnographic present” conveys a notion of static, timeless, unchanging traits of Indigenous societies. For the role of Indigenous blood as a resource for genetic studies of the past, see: Ganett and Griesemer, “The genetics of ABO blood groups,” 155; Radin, *Life on Ice*; Reardon, *Race to the Finish*. For a discussion of the discursive linking of Indigenous peoples to the distant founding of the Brazilian nation, see Sommer, *Foundational Fictions*, 138–177; Guzmán, *Native and National in Brazil*, 63–104.

anthropological and biomedical research, these discursive links persisted, with scientists interpreting Native communities as “geographically isolated portals to the past,” as Radin has suggested.²²⁴

Geneticists’ work during the post-war period linked contemporary Indigenous communities to human pre-history. This theoretical move was predicated on a notion that Indigenous people were more natural, “more biological,” and more pure than other kinds of populations.²²⁵ A key factor in the scientific value of a particular group or community, both for the geneticists and their colleagues in anthropology, depended on whether or not the community under study could be constituted as an “isolated” population. Veronika Lipphardt has argued convincingly that in the post-war era, documenting isolation and endogamy was essential in conferring legitimacy to the genetic study of a population.²²⁶ For Neel and Salzano, certain Indigenous communities of Brazil were ideal targets for such research; those who had resisted contact with Brazilian society, lived in small communities in remote locations, and demonstrated cultural and linguistic difference were convincing as isolated populations.²²⁷ As Radin has shown, and I discuss at more length below, this perspective would be endorsed by a group of internationally prominent

²²⁴ Radin, “Latent Life,” 487.

²²⁵ This idea of some populations being “more biological” is from Joanna Radin as cited in Susan Lindee and Ricardo Ventura Santos, “The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks: An Introduction to Supplement 5,” *Current Anthropology* 53, no. S5 (2012): S7. Theorists from Science and Technology Studies have long pointed out the implications of scientific work that locates some people closer to nature than others, often along gendered or racialized axes. See Donna Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1990); Bruno Latour, *We Have Never Been Modern*, trans. Catherine Porter (Cambridge, MA: Harvard University Press, 1993).

²²⁶ Lipphardt, ““Geographical Distribution Patterns of Various Genes’.”

²²⁷ For a compelling discussion of two different conceptions of what constituted a “primitive” population in Brazil, see the debate between Neel and his compatriot and colleague Newton Morton as described in Santos, Lindee, and Souza, “Varieties of the Primitive.”

geneticists and physical anthropologists first under the auspices of the WHO in the 1960s, and subsequently in the development of the Human Adaptability arm of the International Biological Program.²²⁸

Studying human variability in the wake of WWII was a fraught endeavor. At a moment of high suspicion regarding “racial science,” one of the key innovations in genetics was the rise of blood group studies. This approach parsed diversity by identifying polymorphisms in the form of antigens expressed on blood cells. It was a particularly promising technique in the view of prominent scientists because it was perceived as objective. Since it relied on invisible markers in the blood rather than phenotypic classifications, the scientists reasoned, the expanding use of this technique would help distance the field from accusations of eugenic science.²²⁹

By the time Neel and Salzano turned their attention to Native groups within the Brazilian borders, geneticists and physicians conducting blood group studies had already reached a wide variety of Indigenous groups throughout South America.²³⁰ As Susan Lindee has pointed out, this kind of research into human variation was not new, but in the coming decades it would accelerate greatly.²³¹ Furthermore, Lipphardt and others have shown that even as the majority of human geneticists emphasized their distance from pre-war eugenics and the objective nature of new technologies, the studies of the 1960s

²²⁸ Radin, “Latent Life,” 494–498.

²²⁹ Broadly, blood groups mapped onto and were interpreted in terms of existing “common sense” notions of race. See Jenny Bangham, “Blood Groups and Human Groups: Collecting and Calibrating Genetic Data after World War Two,” *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 47, Part A (September 2014): 74–86, accessed 11 October 2015, doi:10.1016/j.shpsc.2014.05.008; Gannett and Griesemer, “The ABO Blood Groups.”

²³⁰ Francisco M. Salzano, “The Blood-Groups of South-American Indians,” *American Journal of Physical Anthropology* 15, no. 4 (1957): 555–79, accessed 3 October 2011, doi:10.1002/ajpa.1330150407.

²³¹ Lindee, *Moments of Truth in Genetic Medicine*, 59.

continued to widely employ methodologies such as anthropometric measurements, craniology, and other physical documentation linked to longer traditions of racial typology.²³²

Neel and Salzano's Xavante Pilot Study, the model they subsequently proposed, and their future work throughout Amazonia, was built of a combination of old and new techniques. But it went further than simply incorporating methodologies of physical anthropology. As the enduring object of socio-cultural anthropology, groups such as the Xavante were culturally "other" enough to need the expertise of a socio-cultural anthropologist. In addition to helping clarify the influence of kinship and other cultural factors of biological interest, the social scientists might help to substantiate arguments for the isolated nature of the groups under study. As in other national contexts, in Brazil the existing intellectual and practical research infrastructures of socio-cultural anthropology would be an important resource for geneticists.²³³

Interdisciplinarity and Cold War Science

The crossing of disciplinary boundaries, perhaps especially the boundary separating the natural from the social, offered other benefits for the Neel-Salzano collaboration. In addition to promising an intellectual advantage to their proposed project, interdisciplinarity was a strategic choice. At the height of the Cold War, as Jamie Cohen-Cole has argued, work in multiple disciplines was increasingly understood as a virtuous,

²³² Lipphardt, "Genetic Studies of Human Variation after 1945," 52; Radin, "Latent Life," 487.

²³³ Suárez-Díaz, "Indigenous Populations in Mexico," 111-113. As Suárez-Díaz shows, in the Mexican case, both the post-revolution *indigenista* infrastructure and anthropological scholarship on linguistic categories were essential in allowing for Rubens Lisker to conduct genetic and epidemiologic research.

democratic practice.²³⁴ Fitting snugly into the funding and research priorities of the 1960s, the geneticists' requests were strengthened by the trend toward promotion of interdisciplinary agendas. This section discusses how interdisciplinarity itself is an expression of values linked to the political and social priorities of the period.

Concerned scholars first began seriously promoting interdisciplinary research during the interwar period due to their own pessimistic views of overspecialization and fragmentation in the United States' academy. Large private foundations emerging at this time prioritized integrated approaches both in the social and natural sciences. They trumpeted interdisciplinarity as a means to improve the applicability of research to complex technical and social issues.²³⁵

Discourses of the 1940s and 1950s continued to valorize interdisciplinary work, linking this approach to creativity, open-mindedness, tolerance, and to the moral status of the individual researchers who adopted these methods.²³⁶ While pushes towards interdisciplinarity in the academy of the United States predated the outbreak of the Second World War, post-war funding structures expanded the approach to an unprecedented extent as the interdisciplinary successes of military mobilization during the war became a model for post-war practice. For example, in the physical sciences, engineering and physics were brought together in common laboratory space. New Material Sciences programs, which were literally invented by the Department of Defense to address defense questions, won huge grants. The field of Nuclear Science united

²³⁴ Cohen-Cole, *The Open Mind*.

²³⁵ Ibid., 76-77; Robert Seidel, "The Origins of the Lawrence Berkeley Laboratory," in *Big Science: The Growth of Large-Scale Research*, eds. Peter Louis Galison and Bruce Hevly (Stanford: Stanford University Press, 1994): 35. This was also true in psychosomatic medicine, which gained traction in the same period.

²³⁶ Cohen-Cole, *The Open Mind*, 67.

physicists, biologists, radiochemists, and physicians. Both government entities and private foundations funded these burgeoning initiatives, with the Department of Defense and the Atomic Energy Commission playing a key role along side the Ford Foundation, the Carnegie Corporation, and the Rockefeller Foundation.²³⁷

For the social sciences, proponents such as Talcott Parsons and Clyde Kluckhohn posited that interdisciplinarity should unify fields such as anthropology, sociology, and psychology. Theoretical integration would lead to more “rigorous” and scientific approaches.²³⁸ The Harvard Department of Social Relations, where Maybury-Lewis began an assistant professorship in 1960, was created based on this model. By practicing openness to other fields, researchers, and ideas, scientists could not only adequately study complex issues such as democracy, they could enact the kind of social order considered necessary in the face of authoritarianism and the communist threat.²³⁹

Neel, Salzano, and colleagues recognized that certain technological and social conditions of the early 1960s made their initiative possible. First, the growth in access to the key technologies of air travel and the laboratory freezer meant interdisciplinary work with Indigenous populations was now possible on an unprecedented scale. Neel explained that air travel, “not only gives the investigator ready access to populations of great interest previously reached only through exhausting journeys but, even more important, ensures that within a matter of days the all-important biological specimens can be in the

²³⁷ Seidel, “The Origins of the Lawrence Berkeley Laboratory”; Stuart W. Leslie, “Playing the Education Game to Win: The Military and Interdisciplinary Research at Stanford,” *Historical Studies in the Physical and Biological Sciences* 18, no. 1 (1987): 84.

²³⁸ Cohen-Cole, *The Open Mind*, 79.

²³⁹ *Ibid.*, 81.

hands of the individuals able to subject them to the full gamut of tests.”²⁴⁰ International air travel and the “full gamut” of laboratory tests, Neel went on, meant that “good population genetics is expensive.”²⁴¹ The move to “Big Science” was one of the essential factors in making the new model of fieldwork feasible. It was “the current availability of funds for large scale field work” that made research on the Xavante, Kayapó, Ticuna and Yanomami possible.²⁴² In the new global order, the whole world was a potential laboratory.

Investing in Interdisciplinarity

Neel was particularly skilled at securing financing from US-based and international funding organizations. The first seasons of Xavante research benefitted from support from the World Health Organization (WHO), the US Atomic Energy Commission (AEC), the US Public Health Service and the National Research Councils of both Germany and Brazil.²⁴³ It was also an outcome of the strong support of the Rockefeller Foundation, which had nurtured the nascent field of genetics in Brazil over the previous decades, and made Salzano’s post-doctoral studies possible.²⁴⁴ The interdisciplinarity of

²⁴⁰ Neel, “Between Two Worlds,” 10.

²⁴¹ Ibid.

²⁴² Ibid.

²⁴³ Neel et al., “Studies on the Xavante Indians,” 52. It is worth noting that during this period, the US prioritized technical aid, assistance, and cooperation in Latin America more broadly as a political strategy to ward off poverty and communism. See Marcos Cueto, *Cold War, Deadly Fevers*.

²⁴⁴ Thomas F. Glick, “The Rockefeller Foundation and the Emergence of Genetics in Brazil, 1943–1960,” in *Missionaries of Science: The Rockefeller Foundation and Latin America*, ed. Marcos Cueto (Bloomington: Indiana University Press, 1994), 149–64. See also Vanderlei Sebastião de Souza, Rodrigo Ciconet Dornelles, Carlos E. A. Coimbra Jr., and Ricardo Ventura Santos, “História da genética no Brasil: um olhar a partir do Museu da Genética da Universidade Federal do Rio Grande do Sul,” *História, Ciências, Saúde-Manguinhos* 20, no. 2 (2013): 675–94. Accessed 29 July 2013. doi:10.1590/S0104-59702013000200018; Vanderlei Sebastião de Souza and Ricardo Ventura Santos, “The Emergence of

the project was part of the draw. As Cohen-Cole has suggested, during the mid-twentieth century “...the researchers who cast themselves as interdisciplinary were vastly more successful in drawing outside patronage and support from university administrators than their disciplinary counterparts.”²⁴⁵

Other historical analysis has posited that the period from the 1940s to the 1960s was marked by an increasing emphasis on the practical applications of scientific research.²⁴⁶ While the Xavante Pilot Study was intended to help illuminate prehistoric human population structure, in fact the practical applications of the work were initially more methodological than content driven.²⁴⁷ The researchers were testing out the feasibility of the interdisciplinary research model for future work with other Indigenous populations. This was the expected contribution of the first field season.

Even before venturing into Central Brazil, Neel and Salzano thought that their study would serve as a model for future work. Since 1959, Neel had been working closely with the WHO to develop the agency’s program in human genetics.²⁴⁸ R. Lowry Dobson was one of his primary interlocutors at the agency, and was a staunch supporter

Human Population Genetics and Narratives about the Formation of the Brazilian Nation (1950–1960),” *Studies in History and Philosophy of Biological and Biomedical Sciences* 47, Part A (September 2014): 97–107, accessed 20 March 2015, doi:10.1016/j.shpsc.2014.05.010.

²⁴⁵ Cohen-Cole, *The Open Mind*, 5.

²⁴⁶ Paul Forman, “Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940–1960,” *Historical Studies in the Physical Sciences* 18 (1987): 149–229. Forman documents the rise in academic dependence on military funding. This shift was motivated both by top-down government priorities as Forman suggests, and by scientists who nurtured the race for technological superiority to their own benefit as Daniel Kevles has shown in “Cold War and Hot Physics: Science, Security, and the American State, 1945–56,” *Historical Studies in the Physical and Biological Sciences* 20, no. 2 (1990): 239–264.

²⁴⁷ Neel et al., “Studies on the Xavante Indians,” 52.

²⁴⁸ James V. Neel, RA Fraser Roberts, William Schull, and Alan Stevenson, draft report: “Possible Roles of the World Health Organization in Research in Human Genetics,” 11–13, folder: WHO Genetics Primitive, Series I: Correspondence, Grants 12, Neel Papers, APS. Meeting held at University of Michigan from 28 April 1959 to 30 April 1959.

of Neel's proposals. In the lead-up to their first field season, Neel wrote excitedly to Salzano and Junqueira saying, "This little expedition of ours may be assuming more importance that we had initially realized." He went on to explain that he had heard from Dobson, "...that the WHO is possibly prepared to make the study of the surviving very primitive groups a dominant theme of their research programs for the next ten or twenty years... They regard our little pilot study as a possible model for how preliminary cooperative survey efforts can be performed."²⁴⁹ Prior to setting foot in Mato Grosso, the geneticists knew their pilot study would serve as the foundation for a WHO Scientific Group meeting and hoped its influence might extend even further. Much as Dobson had indicated in early 1962, the agency would position the study of so-called primitive groups as the basis for one of two major efforts to promote human genetics research.²⁵⁰ So what were the ultimate practical applications of such a program?

As de Chadarevian has shown, the origin of the WHO's involvement in human genetics research was located in concerns about the effects of increasing anthropogenic—human created—radiation on human heredity. But the programs that emerged were not limited to understanding radiation risk.²⁵¹ Neel's proposal to the WHO was intended to inform fundamental understandings of human populations. By establishing "baselines" to examine changes in population structure, the study of the Xavante and other groups like them would illuminate the problems of how "civilized" life might be distorting the

²⁴⁹ Neel to Salzano, 20 March 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS. Underlining in original.

²⁵⁰ But as de Chadarevian points out, support was not unanimous among WHO officials. Some opposed costly support to human genetics research over other public health priorities. See de Chadarevian, "Human population studies and the World Health Organization," 378.

²⁵¹ *Ibid.*, 372.

natural progression of human evolution.²⁵² But in order to understand this baseline, the geneticists argued, the biological and social documentation of the groups under study had to be comprehensive and swift.

Urgency played a prominent role in Neel and others' discourses on the prospects for genetic study of Indigenous populations.²⁵³ Many scientists and policy makers assumed that Indigenous peoples' environments, cultures, and bodies were not only at immanent risk but would be unable to survive the onslaught of development. Few scholars collaborating with the WHO doubted an outcome of extinction, whether through disease, assimilation, or biological mixing that would undermine the isolation and value of the populations to inform genetic theory.

The impending "vanishing" of the groups of interest also provided the rationale for Neel and his colleagues' emphasis on comprehensive documentation. As Radin has pointed out, the model of research was a salvage project.²⁵⁴ Pioneered with the Xavante and later extended into a WHO technical manual and a template for the Human Adaptability arm of the International Biological Program, the vast collection of social and biological information was a kind of scientific insurance for the future. Radin has argued that scientists collected biological samples in part for unknown future use, for analysis by techniques that had not yet been developed. Institutions such as the WHO helped establish protocols that would make these samples an enduring resource.²⁵⁵ Part of what would make them intelligible in the long run, according to the rhetoric of the proposed

²⁵² Neel et al., "Studies on the Xavante Indians," 127.

²⁵³ Radin, "Latent Life," 495–496. See also de Chadarevian, "Human Population Studies at the WHO," 374.

²⁵⁴ Radin, "Latent Life," 485–486.

²⁵⁵ Radin, "Unfolding Epidemiological Stories."

approach, was the linking of cultural and biometric information to the stored samples. Information about marriage patterns, reproduction, and violence, for example, complimented anthropometric measurements and the results of laboratory analysis. Different kinds of experts had to be involved so that the most accurate information could be saved for the sake of posterity. The interdisciplinary team was a tool of salvage, but was to be harnessed primarily for the interests of the genetics agenda.

Implementing Interdisciplinarity

It was in this wider context of valorization of interdisciplinary work and emphasis on the study of “traditional” societies at a moment of fears about the future, the geneticists placed a special importance on finding “the key person” for their project. Once they had determined Keiter would join them as physical anthropologist, Salzano and his Brazilian colleague, hematologist Pedro Clovis Junqueira, were tasked with identifying an appropriate socio-cultural anthropologist.²⁵⁶ This “fifth man,” as Neel called him, would help the team make sense of the cultural traits that determined reproductive practices, health, and other factors of genetic interest. Salzano and Junqueira struggled to find someone with the appropriate training. With few graduate programs in anthropology or social sciences in Brazil at the time, their initial inquiries for an expert well acquainted with a tribe of Mato Grosso failed. Salzano lamented the difficulties in a letter to Neel, and raised the possibility of including a government employee, missionary,

²⁵⁶ Salzano to Neel, 10 January 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS.

or other lay expert in their team if no appropriate Brazilian scholar could be found.²⁵⁷

A few weeks later after attending a Rockefeller Foundation funded human genetics conference in Rio, Salzano wrote that colleagues from the *Museu Nacional* in Rio de Janeiro had recommended a young social anthropologist. David Maybury-Lewis was now a newly appointed assistant professor of anthropology in the Department of Social Relations at Harvard.²⁵⁸ Salzano wrote enthusiastically to Neel about Maybury-Lewis' field experience (described in Chapter 1) and doctoral work at Oxford. Salzano wrote to Maybury-Lewis immediately, with Neel following up with a letter of his own as soon as he received Salzano's note.

Neel's letter was somewhat cautious in tone.²⁵⁹ Declining to explicitly invite Maybury-Lewis to join the expedition in his introductory message, instead Neel solicited copies of any publications Maybury-Lewis might have, and proposed to foot the bill for an in-person meeting in Ann Arbor or Cambridge. His first letter gave no indication of how worried the geneticists had been about finding someone to accompany them into the field. Maybury-Lewis replied enthusiastically: "May I say straight away that I am delighted to hear of the research you plan to undertake, and would be happy to help you in any way I can."²⁶⁰ Writing that the existing literature on the Xavante was by and large "worthless," Maybury-Lewis noted that he also planned to conduct fieldwork in Mato Grosso in July. Neel's response again understated how much he and the Brazilian

²⁵⁷ Salzano to Neel, 8 March 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS.

²⁵⁸ Salzano to Neel, 11 April 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS.

²⁵⁹ Unfortunately, there is no copy of Salzano's initial letter to Maybury-Lewis in the Neel Papers, nor does Salzano have a copy in his personal papers.

²⁶⁰ Maybury-Lewis to Neel, 26 April 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

members of the team were hoping Maybury-Lewis would become an integral member of their team. “As you might imagine,” he wrote, “I was struck by the coincidence that you were planning on being in Brazil this summer. Your generous offer of help is greatly appreciated. I am most anxious to outline in general our plans to you for a number of reasons. I am sure you can be most helpful to us; there is a good chance that we in turn might be helpful to you.”²⁶¹ Maybury-Lewis accepted the invitation to Ann Arbor.²⁶²

Neel and Maybury-Lewis spent two days together in Ann Arbor in May of 1962, discussing the details of Xavante social structure, the challenges of fieldwork in Central Brazil, and the potential of a collaboration. Their conversations helped to develop the priorities for the upcoming field trip. Writing to his Brazilian colleagues emphatically after the meeting, Neel updated them on a new vision for their field season: “We now visualize as the first objective an effort to construct an entire village pedigree, using as a point of departure the extensive ‘kinship pedigree’ Maybury-Lewis now has.”²⁶³ The anthropologists’ knowledge would serve the genetic purposes of the study.

For Neel, the meeting with Maybury-Lewis crystalized his view of just how crucial the anthropologist would be. The geneticist understood kinship systems as one of the most direct ways culture might influence the biology of population. And yet Xavante

²⁶¹ Neel to Maybury-Lewis, 30 April 1962. Part of what Neel had to offer the social anthropologist was support in securing funding. Although Maybury-Lewis secured his own funding for his summer fieldwork, Neel offered to help even as late as June, inquiring, “Where do you stand on your own application for research funds? Is there any need to try to give the Washington tree a last-minute shake?” Neel to Maybury-Lewis, 8 June 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

²⁶² Maybury-Lewis to Neel, 28 April 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS. It seems Maybury-Lewis may not have quite been aware of how much the geneticists were depending on him, how well he fit into their preconceived work plan, or how much his existence influenced the project. His correspondence with Neel hints at the fact that he may have conceived of his own fieldwork as mostly independent of his interactions with the biomedical team.

²⁶³ Neel to Junqueira and Salzano 17 May 1962, Junqueira Correspondence, Box 39, Neel Papers, APS. Underlining in original.

kinship, he wrote, “seen through our genetic eyes is extremely complex, confusing, and non-biological. I am convinced that to attempt to obtain biological pedigrees without a rather intimate knowledge of that kinship system would be an invitation to disaster.”²⁶⁴

Neel believed Maybury-Lewis’ participation in the project would allow the geneticists to understand the kinship system and in turn measure and theorize its possible influence on the introduction and maintenance of genetic diversity within a population. Sociality across disciplinary lines would allow the geneticists to understand the interaction of culture and nature, leading to more robust scientific findings.

Maybury-Lewis was also crucial for helping with a number of practical matters. It was finding the young anthropologist that prompted the geneticists to study the Xavante. It was on his recommendation that they selected the first village to visit. Neel chose the community at Wedezé (São Domingos), explaining to his colleagues that it “looks in terms of its untouchedness by far preferable to the other five.”²⁶⁵ Situated at the edge of a landing strip, Wedezé also offered the possibility to arrive and depart by air, ensuring the safe transport of perishable blood samples, and it was the village Maybury-Lewis knew best.²⁶⁶

Finally, the geneticists gleaned important insight from Maybury-Lewis’ previous difficulties conducting research as a foreigner in Brazil. The social anthropologist had

²⁶⁴ Ibid.

²⁶⁵ Ibid. It is important to note that both Maybury-Lewis and the geneticists had stakes in the society or population under study being understood as “untouched” as possible. The valorization of the study of “unacculturated” Indigenous peoples was prevalent in anthropology as was the emphasis on “isolation” for genetics. Maybury-Lewis observed great changes in the village of São Domingos from his first visit in 1958 to his trip with the geneticists in 1962, lamenting the condition of the community upon his second visit. The geneticists, however, observed masculine vitality, health, and authenticity as described in Chapter 3.

²⁶⁶ Ibid.

suffered with his research materials impounded for months in customs. Severe delays in obtaining research permission also postponed and limited his time in the field for his doctoral research.²⁶⁷ Thoroughly warned by the young man's experience, Neel called on his contacts at the WHO to provide special documentation to facilitate the liberation of their luggage, reagents, and medical equipment upon arrival in Rio.²⁶⁸

Much as Neel and Salzano understood their 1962 fieldwork as a pilot study, Maybury-Lewis understood his own doctoral research as the first step in a large-scale comparative study of Jê-speaking groups. His plan to train a series of graduate students to complete extensive fieldwork with Kayapó, Kanela, and Apinayé communities, seemed to Salzano to anticipate a perfect research trajectory. The Brazilian geneticist wrote to Neel saying, "If our pilot study could be followed by a long-term enterprise and we could obtain the cooperation of Harvard University and other Brazilian social anthropologists I am sure this would establish one of the most powerful research teams ever organized in Brazil. The importance of this development for human genetics in my country need not to be stressed."²⁶⁹ The scientists went to the field with high hopes both for their first experience of interdisciplinary work and future projects. Understanding the expedition as a pilot study for their own work, for longer term collaborations with Harvard anthropologists, and as a model for the upcoming WHO meeting, they were also cognizant of some of the challenges that awaited them.

²⁶⁷ Maybury-Lewis, *Savage and the Innocent*, 156–160. On Maybury-Lewis' struggle with the regulatory system, see Luís Donisete Benzi Grupioni, *Coleções e expedições vigiadas: os etnólogos no Conselho de Fiscalização das Expedições Artísticas e Científicas no Brasil* (São Paulo: Editora Hucitec ANPOCS, 1998), 102-108.

²⁶⁸ Neel to Salzano and Junqueira, 30 April 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS; James Neel to R. Lowry Dobson, 17 May 1962, File G3-445-3, WHO Archives, Geneva.

²⁶⁹ Salzano to Neel, 9 May 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS.

From the Field to the Page

It was a hot, dry afternoon when the six men arrived in Wedezé. Delivered to the airstrip in mid-July by the *Força Aérea Brasileira* (the Brazilian Air Force) thanks to Junqueira's government contacts, the group disembarked during the dry season. Maybury-Lewis went straight to stay in the village, while his colleagues installed themselves at the government post a kilometer away under the watch of Ismael Leitão. The expansive government agent was the same one who had been in charge of the post while Maybury-Lewis completed his fieldwork, but the anthropologist and the SPI agent did not get along.²⁷⁰ However, Leitão quickly ingratiated himself with the geneticists through his wife's cooking and his own offers to translate.

The ten days of fieldwork were intense. Each member of the group was charged with specific tasks. Salzano took demographic histories with Leitão's interpretation. Keiter collected anthropometric measurements and photographs. Neel performed physical exams. Simões assisted, performing vision tests and coordinating the coming and going

²⁷⁰ Maybury-Lewis, *Akwẽ-Shavante Society*, xxii–xxiii. Maybury-Lewis described Leitão's distaste regarding his arrival in politic terms in a footnote saying "The Indian agent at São Domingos made it clear on this occasion that he did not wish to receive me at his post. This did not interfere with my plan of research, but I note it here in order to give as complete a picture as possible of the prevailing conditions. He was the same agent who had been at São Domingos during my previous research. When we left the Shavante in 1958 relations between us had been cool, but I for my part did not feel as strongly about him as he obviously did about me." He went on to discuss briefly the general resentment of SPI workers for anthropologists whose fieldwork might encroach on the authority and expertise of the government agents. In his autobiography Neel remembered the tension between the two men at the moment that they picked Leitão up in Goiania en route writing, "Ismael took one look at David and declared the plane was not large enough for the two of them. I never really fathomed the enmity, but suspect he felt challenged when an anthropologist (with different viewpoints) arrived to study the people prolonged contact with whom was his chief claim to fame. The situation demanded the full range of Pedro's [Junqueira's] diplomacy, but at length all was well—the seating arrangement on the plane was designed to maximize the distance between David and Ismael," *Physician to the Gene Pool*, 125–126.

of Xavante subjects as they showed up to be examined family by family.²⁷¹ In the village, Maybury-Lewis worked on reviewing his existing genealogies, updating his information and documenting who had left and who had joined the village during the four years of his absence. Junqueira, the hematologist, stayed for the first night and would return from Brasilia to join the team only in the final days when blood samples were to be drawn.²⁷²

The time in the field was a moment to build – and strain – personal relationships in a manner uncommon in the laboratory or seminar room, but beyond the tension between the social anthropologist and the government agent most sources suggest the rest of the team got along remarkably well.²⁷³ Despite a steep learning curve regarding logistics, at the close of their time in the field the scientists declared the excursion a success.

Each individual's role was well defined before beginning the fieldwork, and in large part during the day-to-day activity, each kept to his assigned role. An examination of the responsibilities of each researcher in the field shows that the interdisciplinarity of their project wasn't so much in the execution of tasks, but in the compilation, synthesis, and analysis of the collected data. Thus each expert, qualified and trained within his own discipline, would be responsible for creating data of high quality that would be legible and respected by others from his field. These data would then inform the conclusions, primarily as conceptualized and articulated by the geneticists.

²⁷¹ Salzano, interview, 11 July 2012.

²⁷² Ibid.

²⁷³ Maybury-Lewis and Neel, for example, exchanged photographs from the field in addition to questions and responses about the data in their post-fieldwork correspondence. Neel wrote, "Enclosed please find a picture which may serve as a general reminder that we haven't always been so harried by these damn academic details," and Maybury-Lewis responded that the "excellent photograph... makes me feel quite nostalgic for central Brazil." Neel to Maybury-Lewis, 22 October 1962 and Maybury-Lewis to Neel, 26 October 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

Even prior to entering the field, the geneticists expected Maybury-Lewis' preparation of the village pedigree to be his greatest contribution. The vast genealogical chart that the team constructed was an attempt to include every known member of the village, living or dead. (See Figure 2.) It was a blend of anthropological, Indigenous, genetic, and official government knowledge.²⁷⁴ Maybury-Lewis worked from his original kinship pedigrees, which he had constructed for his doctoral studies in the process of describing Xavante exogamous moieties, social groupings that determined who could marry whom within the group. During their fieldwork, Francisco Salzano was responsible for creating a parallel system of documentation, working with Ismael Leitão as an interpreter. Each Xavante to participate was asked to specify the members of their families, with Salzano documenting their responses with the help of Leitão.

The two sets of information then had to be cleaned up, compared, and combined into a definitive version of the Xavante genealogy. Following the fieldwork, Salzano and Maybury-Lewis worked for a furious two days in Porto Alegre to compare their documentation of the social and biological ties in São Domingos. They reported back to Neel saying, "we have re-checked, with mostly concordant results, our independently collected pedigrees."²⁷⁵ This intensive work together gave Salzano an opportunity to see how Maybury-Lewis had built his genealogies. Once their data had been aligned, the scientists parted ways to write up their sections of a single paper that would report the findings of all aspects of the study.

²⁷⁴ Lindee, *Moments of Truth*, 62. Lindee described the pedigree saying it "seamlessly blends folk, emotional, social and technical knowledge, compacting multiple perspectives into a single image and text... a bedrock tool of human molecular genetics."

²⁷⁵ Salzano to Neel, 10 September 1962, Salzano Correspondence (1 of 10), Box 66, Neel Papers, APS.

The next set of challenges involved bringing together the data, interpretations, and work styles of the collaborators. Neel took charge of coordinating the preparation of the manuscript. In September he sent out a detailed outline along with instructions to each of the collaborators regarding the sections that they were charged with writing. To Maybury-Lewis he wrote, “I would venture to suggest that in preparing the materials you emphasize the biological aspects of it.”²⁷⁶ He went on to ask that the social anthropologist include sections on how the clan system proscribed consanguineous marriages, the prevalence of “Extra-marital Relations,” the exchange of marital partners between villages, and finally, a description of Xavante nutrition to contextualize the later biochemical analysis. Maybury-Lewis responded, saying, “I will do my best to talk about the biological aspects of kinship (even though it goes against the grain!)”²⁷⁷

The German physical anthropologist, Keiter, still in Porto Alegre on an extended stay in Salzano’s lab, received directions regarding the preparation of the anthropometric and dermatoglyphic data. Both Neel and Salzano were somewhat disappointed, four months later after receiving the first version of Keiter’s contribution. The physical anthropologist had barely described the examinations; he had focused almost exclusively on the question of parentage exclusions, showcasing his new methodology to systematically, qualitatively compare morphological traits.²⁷⁸ In private correspondence, Salzano and Neel briefly despaired, unconvinced by his approach, and agreed that what was needed was a straightforward description of the anthropometric characteristics of the

²⁷⁶ Neel to Maybury-Lewis, 14 September 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

²⁷⁷ Maybury-Lewis to Neel, 16 October 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

²⁷⁸ Neel to Salzano, 8 February 1963, Salzano Correspondence (2 of 10), Box 66, Neel Papers, APS.

population studied.²⁷⁹ After tactful suggestions from Neel, Keiter prepared a new version more closely in keeping with the geneticists' vision. The physical anthropologist's work was molded to fit the conception of the genetics research. Neel and Salzano would not take a risk including a new approach they found lacking in rigor in such an important publication.

In the mean time Junqueira, the hematologist, spent a period at the University of Michigan working through blood typing studies with Neel. They had tested samples in both Rio and Ann Arbor, but encountered difficulties in replicating results for a few of the tests and samples. This was likely due both to the state of the samples and the reagents, and to some issues of technique. The Ann Arbor work was an attempt to improve consistency between the two teams. But beyond their direct collaboration in the United States, Junqueira was often unresponsive to correspondence.²⁸⁰ This presented a significant barrier, which Salzano was occasionally responsible for overcoming by arranging an in-person visit to Rio. Junqueira was not responsible for composing any of the final paper, but did give detailed feedback on the first full version.

The timelines for putting together a manuscript of such magnitude proved challenging. Neel was particularly frustrated, penning a letter to Junqueira as early as January to complain, "Writing this material up has become much more of a job than originally anticipated. Our colleagues are all a little slow in getting their material in.

²⁷⁹ Salzano to Neel, 15 February 1963, Salzano Correspondence (2 of 10), Box 66, Neel Papers, APS.

²⁸⁰ Neel, with the help of his secretary, was extremely organized and seems to have kept the vast majority of his correspondence (as well as receipts, napkins from lunches, etc.). While Neel had seventeen letters on file sent to Junqueira, he had only three in reply from the hematologist.

However, I still hope we may have it ready by spring.”²⁸¹ Neel initially had hoped to have a draft of the paper finished by the end of 1962, but Keiter’s revised portion of the manuscript did not arrive until March 1963.²⁸² Maybury-Lewis suffered a series of family emergencies and illnesses that prompted apologetic letters. After both of his wife’s parents had died in swift succession in Denmark, and he had been hospitalized in the college infirmary, Maybury-Lewis sent his contribution in mid-June, writing, “Here it is at long last.”²⁸³ Maybury-Lewis’ portion of the paper arrived six months after Neel’s proposed goal.²⁸⁴ In contrast to their coauthors, Neel and Salzano’s work together proceeded at breakneck speed. They usually replied to one another immediately, reviewing, correcting, and negotiating the content and wording of the paper by post. Less than a month after Maybury-Lewis’ contribution arrived, their paper was already under review.²⁸⁵

Publishing and Planning

The manuscript was unusual due to the group of co-authors that it brought

²⁸¹ Neel to Junqueira, 3 January 1963, Junqueira Correspondence, Box 39, Neel Papers, APS.

²⁸² Ibid.

²⁸³ Maybury-Lewis to Neel 17 June 63. Regarding his in-laws’ deaths, see Maybury-Lewis to Neel, 8 February 1963, and regarding Maybury-Lewis’ illness, Louise M Sherman to Neel, 9 April 63, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

²⁸⁴ Maybury-Lewis to Neel, 22 October 1962; Maybury-Lewis to Neel, 8 February 1963, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

²⁸⁵ In the end, despite early suggestions to the WHO and the Brazilian Human Genetics Commission that the Brazilian team members would likely be the first authors and primary shapers of any publications, Neel was responsible for a great deal of the drafting of the manuscript. See: Neel to the Human Genetics Commission of Brazil, 27 December 1961, File G3-445-3, WHO Archives. Upon receiving extensive comments on the first full draft from Salzano, Neel explained, “... as you might imagine, I wrote this long paper with many interruptions, often late at night, and we went straight from the handwritten copy to the mimeographed form, so there is much need for smoothing.” Neel to Salzano, 31 May 1963, Salzano Correspondence (2 of 10), Box 66, Neel Papers, APS.

together and in its form and content. Originally over 110 pages, the peer review and editorial process whittled the paper down to a hefty 88. *The American Journal of Human Genetics*, which published the study with extra funds from the WHO, dedicated almost an entire issue to the paper. With ten pages dedicated to the territory, history, linguistics, and social institutions, the bio-medical data were situated in a much more detailed description of social context than previous human genetics publications. Thirteen pages on the physical anthropology of the group included a black and white plate showing anthropometric photographs of the Xavante chief Apowẽ and one of his sons, as well as numerous tables listing average values for morphological features, intensity indices for finger and toe prints, and the like. Eleven pages of text and tables about genetic variation in blood type was followed by a twelve-page description of demographics of the village and three and a half pages on the comparison of morphological traits and genetic traits to determine “mating pattern.” Two large sections describing the findings of physical and biochemical examinations of health followed, documenting burden of disease and extensive antibody reactions to common infections over the course of twenty pages. Finally, six and a half pages provided a small space for discussion and for a summary of the paper as a whole.²⁸⁶ Despite the frustrations and delays, once the paper proofs were in, Neel was satisfied, saying, “Although I am not without prejudice, I am happy with the outcome.”²⁸⁷

The findings and results of the paper laid the foundation for the fission-fusion

²⁸⁶ Neel et al., “Studies on the Xavante Indians,” 52–140.

²⁸⁷ Neel to Maybury-Lewis, 14 November 1963, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

theory of microevolution and would constitute a major contribution to the nascent field of human genetics, are described at length in Chapter 3. The social anthropologists' insight into political fission and mobility between Xavante communities combined with the physical anthropologists' measurements allowed the geneticists to articulate an influential theory.

Before the paper was even in print, Neel and Salzano were on to planning the next set of studies, which would focus on two additional Xavante villages. Maybury-Lewis, Keiter, Junqueira, and Simões as research assistant would all continue to collaborate for the 1964 Xavante fieldwork and ten resulting papers published in 1967 and 1968.

This next set of studies, however, was different in a few key ways. First, the social anthropologist did not participate in the fieldwork. The biomedical team collaborated with Maybury-Lewis, but only by correspondence. They compared their data with the genealogies he had constructed in 1958, and benefitted greatly from his insights about the inter-village migration.²⁸⁸ Keiter's involvement was also cut short by his untimely death in an airplane crash in 1967, and Junqueira's failure to arrive in the field to pick Salzano and Simões up from an extra field trip to the Xavante of São Marcos marked an end to Salzano's willingness to collaborate with the Rio hematologist.²⁸⁹ The 1967 and 1968

²⁸⁸ Maybury-Lewis to Neel, 7 January 1966, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS. Maybury-Lewis offered the geneticists a more nuanced understanding of the splits and fusions that took place in the communities under study. Reviewing a manuscript for one of the 1967 papers, Maybury-Lewis wrote, "More took place at São Domingos between 1958 and 1962 than the two schisms you mention. They gained population from Capitariquara in 1959 (a group led by Sebastião). Then they lost population again the following year when Sebastião left. The gain and the loss probably balanced roughly but we can be sure that they only balanced very roughly! Then came the secession in 1961 about which you know." The anthropologist's contextualization would be essential in the geneticists' development of their fission-fusion model to explain the maintenance of genetic variability within pre-historic populations.

²⁸⁹ After waiting for 24 hours in the village with no sign of Junqueira's plane, Salzano and Simões instead made the long journey from São Marcos to the nearby city of Barra do Garças in their truck, worried that

publications also took a very different form from the initial project. Now a series of papers distributed between the participating authors, each was tailored to address a narrowly defined topic (and a more clearly delineated audience). The work from these subsequent studies fit smoothly into classic human genetics and biomedical publishing models.

The Uneven Benefits of Interdisciplinarity

Following publication of their joint paper, each of the researchers felt the reverberations of their collaboration as they circulated through their departments and professional meetings, faced with feedback from colleagues across the disciplines. While they had worked together closely, they were under no illusions that they shared the same depth of knowledge about each area of the study. Maybury-Lewis reported back to his colleagues, “An entertaining side effect of my cooperation with you on this is that I am not on occasion approached by physical anthropologists who bear down on me with the gleam of battle in their eye and hope to engage me in argument on the procedures that ‘we’ used in applying the various tests to the population of São Domingos!”²⁹⁰ Likewise, Keiter and Neel both wrote of their great anticipation of Maybury-Lewis’ long awaited book. Neel even requested a copy of the galleys at his own expense, writing, “Really looking forward to the appearance of your monograph—I am forever being asked

the perishable blood samples would not survive the heat. In fact, the samples arrived in poor condition, rendering their study difficult. Salzano, interview, 11 July 2012.

²⁹⁰ Maybury-Lewis to Neel, 16 November 1964, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

questions I can't answer."²⁹¹

The scientists acknowledged their enduring differences in perspective and knowledge base, and yet to differing levels they also believed that their collaboration had enriched their scholarship. From the point of view of Salzano and Neel, the “close-quarters interaction” posited in the research design did lead to an “exciting intellectual interaction and synthesis.”²⁹² They saw their project as a deep integration of knowledge where socio-cultural and physical anthropology could serve as a corrective or supplement to genetic and biomedical studies.

For Salzano, the fieldwork was a boon to his productivity. It provided him visibility on the international stage, and the formation of his own future research agenda. The extensive interaction with Maybury-Lewis gave him a template for what questions to ask socio-cultural anthropologists in future collaborations, and provided him with a level of training in how kinship genealogies compared to genetic ones. As early as 1963 Salzano was promoting the potential for collaborations, announcing at a round table of the *Associação Brasileira de Antropologia*, “the tendency in Brazil is to change, and establish a deeper relationship between Genetics and Anthropology... Following this pilot research with the Xavante, other projects should follow in which we will vitally need the help of anthropologists.”²⁹³ Just as Salzano had responded so optimistically to the possibility of an ongoing association with Harvard and their socio-cultural research in Central Brazil, the possibility of working closely with Brazilian anthropologists seemed

²⁹¹ Neel to Maybury-Lewis, 7 December 1964, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

²⁹² Neel, *Physician to the Gene Pool*, 121.

²⁹³ Francisco M. Salzano, “Oportunidades atuais de colaboração entre antropologistas e geneticistas do Brasil,” *Revista Do Museu Paulista* 14 (1963): 513–14.

promising.

Likewise, for Neel the Xavante Pilot Study and the subsequent WHO and Pan American Health Organization meetings on “Population Genetics of Primitive Groups” helped to establish his scholarly authority in a new area of research that would constitute a major focus for the rest of his career.²⁹⁴ As the model was incorporated into the Human Adaptability arm of the International Biological Program, Neel won additional prestige and funding.

During their execution of the pilot study, the geneticists gained practical and intellectual benefit from the help of Maybury-Lewis. This was important not only to the contextualization of their studies, but also in gaining professional legitimacy. Theirs was a foray into territory more traditionally under the purview of socio-cultural and physical anthropologists. The geneticists were well aware that they might be considered interlopers, and so took measures to emphasize (at least discursively) the role of the social anthropologists. In the introduction to their group paper, for example, they emphasized Maybury-Lewis’ previous experience with the Xavante, writing “During 1958, one of the authors (D. M. -L.) conducted the first detailed investigations of the group, directing his studies primarily toward kinship and political structure. There was

²⁹⁴ World Health Organization, “Research in population genetics of primitive groups: report of a WHO Scientific Group.” World Health Organization Technical Report Series No. 279 (1964); WHO Scientific Group on Human Genetics Research, “Research on Human Population Genetics.” World Health Organization Technical Report Series No. 387 (1968); James V Neel, “Multidisciplinary Studies on Primitive Populations in Latin America,” Advisory Committee on Medical Research, Washington, DC: Pan American Health Organization, 9 March 1964; James V Neel, “The American Indian in the International Biological Program,” Advisory Committee on Medical Research, Washington, DC: Pan American Health Organization, 13 May 1968. Radin discusses the WHO meeting at length in “Latent Life,” including critiques that came from those unconvinced by the methodology.

thus the necessary background of experience and communication with the tribe.”²⁹⁵

Likewise, in the lead-up to the WHO Scientific Group meeting on Population Genetics of Primitive peoples, Neel wanted to be cautious about stepping on the toes of those with more experience. “I would continue to urge,” Neel wrote to Dobson, “that this be regarded as a very preliminary meeting...” and went on to say, “Please do not emphasize unduly our small effort of the past summer. Many of the participants in the conference are old hands at this...”²⁹⁶

So with anthropologists already boasting long-term experience working with Indigenous people, how did they stand to benefit in collaborating with the geneticists? Neel and Maybury-Lewis discussed this, with Neel writing to his Brazilian colleagues regarding what they could offer, “in view of the great assistance which we will be receiving from him.” Neel went on, “I am wondering to what extent we could assist him in obtaining necessary transportation... It would seem to be a legitimate diversion of some of the WHO funds should this prove necessary.”²⁹⁷

Joint publications and being associated with an interdisciplinary project could also constitute a benefit, although these factors were insufficient to ease Maybury-Lewis’ tumultuous tenure review process. When a permanent position for an anthropologist opened in the Department of Social Relations in the 1964–65 academic year, Maybury-Lewis came up against A. Kimball Romney, a Stanford professor and cognitive anthropologist. The Department of Social Relations was an interdisciplinary department

²⁹⁵ Neel et al., “Studies on the Xavante Indians,” 53.

²⁹⁶ Neel to R. Lowry Dobson, 12 September 1962. Folder: WHO Genetics Primitive, Series I: Correspondence, Grants 11. Neel Papers, APS.

²⁹⁷ Neel to Salzano and Junqueira, 17 May 1962, Junqueira Correspondence, Box 39, Neel Papers, APS.

by design, combining anthropology with sociology and psychology. Central involvement in a cross-disciplinary research agenda, then, ought to have helped his case, but some interdisciplinarity were more highly valued than others. In the review process, Maybury-Lewis drew the support of the majority of anthropologists of his department, but Romney had the endorsement of the psychologists. In the end both candidates were recommended to the final review, but the final jury, which Maybury-Lewis perceived as heavily stacked against him, confirmed Romney.²⁹⁸

Maybury-Lewis returned for a year of limbo at Harvard, while his supporters worked to get him tenure. The 1965-66 tenure review process would now include more explicit evidence of Maybury-Lewis' interdisciplinarity. Anthropologist Douglas Oliver asked Neel for a letter of recommendation, writing, "It would be most helpful to his case if we could include in his dossier a letter from you touching on his work in connection with your joint research in Brazil. Like most University Administrations these days, I suppose, presidents and deans are looking for professors who have shown a willingness and a capacity to go beyond the narrow boundaries of their disciplines and work with specialists in other fields..."²⁹⁹ Neel immediately responded with a letter of support, which Oliver assured him would "greatly strengthen the case the department is making on [Maybury-Lewis'] behalf."³⁰⁰ The second attempt at tenure was successful, but moved Maybury-Lewis from the Social Relations Department to Anthropology. It was

²⁹⁸ Maybury-Lewis to Roberto Cardoso de Oliveira, 9 July 1965, Pasta 47, Papers of Roberto Cardoso de Oliveira, Arquivo Edgard Leuenroth, Universidade Estadual de Campinas, Campinas, SP.

²⁹⁹ Douglas Oliver to Neel, 9 November 1965, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

³⁰⁰ Oliver to Neel, 23 November 1965, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

during this tumultuous period in Maybury-Lewis' career that he disengaged from his collaborations with the geneticists. Although he wrote to Neel saying he was still interested in co-authoring work, he later excused himself, saying that the demands on his time were too many and too great for him to continue the collaboration.³⁰¹

For the case of the Xavante Pilot Study, the practicalities of the research process and distinct disciplinary styles and rhythms meant that planning and publication were challenging. The model was perhaps even more difficult to recreate for future research. Perhaps part of the challenge of maintaining the engagement and pace of participation for each member of the group was due to the differential benefit that the collaboration offered to those from different fields and national contexts.

Challenges and Successes Replicating the Model

Shortly after returning from their first field season, and long before the data were analyzed and the first paper composed, the geneticists were already thinking about the next Indigenous group to study. They wrote to Maybury-Lewis, asking him for recommendations. As the anthropologist had foreshadowed in his early letters to Neel, he had begun training graduate students for the comparative study of Jê-speaking groups, what would become the Harvard-Museu Nacional Central Brazil Project (1962-1968). His first student, Terrence Turner, was already in Brazil, working in Kayapó communities to the north of the Xavante, and Maybury-Lewis recommended Turner and

³⁰¹ Maybury-Lewis to Neel, 12 April 66; Maybury-Lewis to Neel, 14 June 1966, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

the Kayapó as a good option.³⁰²

Neel struggled to track down Turner and convince him that collaboration would be worthwhile. Repeatedly writing to Maybury-Lewis to ask for Turner's contact information, and extolling the mutual benefit he envisioned in collaboration, Neel made the case to Maybury-Lewis for the benefit they might offer his doctoral student.³⁰³ At the time, however, Turner was in the field, conducting the ethnographic research for his dissertation and not very available. After a year of attempts to get in touch, Neel lamented to Maybury-Lewis, "I have gone as far as I consider proper in attempts to persuade Turner to join us, should the Cayapo be our final choice."³⁰⁴

In contrast, Salzano had relatively more success coordinating with Turner. Although archival documents are unclear on exactly when, sometime between the 1963 and 1966 Turner made a trip to Porto Alegre, where he held meetings with Salzano and students in the genetics program. Turner agreed to help Salzano in extending the model of the pilot study to research the Kayapó, but he was unable or unwilling to accompany the team into the field. Rather, he provided support similar to that which Maybury-Lewis had offered through correspondence with Salzano.

In anticipation of Salzano's first Kayapó field excursion, Turner sent a detailed list of the inhabitants of the village of Porori, including their names, their household, and

³⁰² Maybury-Lewis to Neel, 16 October 1962, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

³⁰³ Neel to Maybury-Lewis, 19 February 1963, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

³⁰⁴ Neel to Maybury-Lewis, 7 November 1963, Maybury-Lewis Correspondence (1 of 2), Box 49, Neel Papers, APS.

identification numbers he used for each person.³⁰⁵ Specifying his own Kayapó name, Turner suggested, “I think maybe it would help in the beginning of your research if you mention that it was me who wrote all of the names, etc. that you are using, because any other way they are going to think it’s very strange that you know all of their names without having visited the village before.”³⁰⁶ Turner asked for Salzano’s help in clarifying two or three small inconsistencies he had found in his records. He also promised to keep working on the genealogies for two other villages where he had spent time.

The correspondence between Turner and Salzano was warm, and gives the impression that the North American anthropologist might have been happy to collaborate more closely had he had more disposable time and income. In one 1967 letter, after lamenting the difficulty in securing funds for follow-up fieldwork, Turner wrote, “If I make it down I would very much like to come to Porto Alegre again. I have always remember my visit there as one of the best times I ever had in Brazil... I have taken a job as assistant professor in the anthropology department at Cornell University ...I am enjoying it here. But, the restaurants and bars of Ithaca, N.Y. are not up to Porto Alegre standards!”³⁰⁷ Part of Salzano’s broader success with interdisciplinary work was the fact that most of his colleagues found him to be a very cordial, professional, and efficient collaborator.

³⁰⁵ As is true for the Xavante, Kayapó individuals receive a number of different names over the course of their lives, making it challenging for scientists with limited or sporadic time in the field to keep track of changes over time.

³⁰⁶ Terence Turner to Salzano, 31 May 1966, Personal Papers of Francisco M. Salzano, *Universidade Federal do Rio Grande do Sul*.

³⁰⁷ Turner to Salzano, 17 November 1967, Salzano Papers.

However, even in cases of mutual esteem, the socio-cultural anthropologists and the geneticists struggled to maintain sustained collaborations. The geneticists' model, both for their own work and enshrined in the WHO protocol, mandated collecting data on many different populations for comparative study. Their approach required moving from population to population at a rate that most socio-cultural anthropologists would find impossible. While Turner would go on to spend at least 30 more years working writing about Kayapó culture and politics, Salzano's attentions were already turning to other groups, such as the Terena and the Yanomami.³⁰⁸ Likewise, socio-cultural anthropologists were beginning to understand their relationships with the communities they studied in new terms, emphasizing political engagement, activism, and sustained commitments to the groups they studied. Salzano described his differences with Turner in 2014 saying that Turner had distanced himself from more purely academic pursuits in favor of advocacy work.³⁰⁹ Along with many from his field, Turner would become a critic of sociobiology, and specifically the work of Neel and anthropologist Napoleon Chagnon among the Yanomami. Tensions would rise between those advocating biological and socio-cultural approaches to understanding Indigenous populations.³¹⁰

³⁰⁸ In fact, while still conducting his work on the Kayapó, Salzano entered into correspondence with Roberto Cardoso de Oliveira, a socio-cultural anthropologist at the Museu Nacional, and a close colleague of both Maybury-Lewis and Turner. Cardoso de Oliveira and Salzano's collaboration was almost identical in form and methodology to Salzano's work with Turner, but resulted in a joint publication: F. M. Salzano and R. Cardoso de Oliveira, "Genetic Aspects of the Demography of Brazilian Terena Indians," *Social Biology* 17, no. 3 (1970): 217–23.

³⁰⁹ Francisco Mauro Salzano, interview with Rosanna Dent, 3 July 2014, Porto Alegre.

³¹⁰ Robert Borofsky et al., *Yanomami: The Fierce Controversy and What We Can Learn from It* (Berkeley: University of California Press, 2005).

Conclusion

The small, six-person group of experts for the Xavante Pilot Study was carefully composed. Interdisciplinary and transnational, in its construction it crossed many different kinds of boundaries. And yet, it was the disciplinary boundaries that the scientists chose to recognize and celebrate. While the social and natural scientists may not have understood all of the methodologies and academic priorities of their colleagues, they praised the process of working together, lauding its potential to enrich their thinking. But the diverse approaches, fundamentally, were harnessed in the interest of a genetic and biomedical assessment of the Xavante. Fusing anthropological approaches into the human genetics agenda built the credibility of the geneticists' research model. With the endorsement of the WHO and the IBP, this model would be promoted to researchers throughout the biomedical sciences with interest in human variation. By identifying and celebrating interdisciplinarity as the primary descriptor for their fieldwork, Neel and Salzano attempted to set their approach apart from previous research. Interdisciplinarity was both intellectually compelling and fundamentally convenient in the Cold War context. It was marketable. But in the process of promoting the "interdisciplinary" frame, other boundary crossings were naturalized or invisibilized. The Xavante Pilot Study bridged national boundaries and the Cold War First World–Third World divide. It engaged with individuals and academic fields with different ethical regimes and political commitments, and incorporated knowledge across the lay–professional divide in enrolling government employees and their knowledge. It also drew on knowledge from the Xavante, who were themselves trying to profit from the interaction, whether through access to trade goods or medical attention.

The many other boundary-crossings necessary may have contributed to the researchers' struggle to repeat the experience. As the collaboration-by-correspondence that Salzano developed with Terry Turner and Roberto Cardoso de Oliveira shows, even when it was politically and socially possible to combine genetic and socio-cultural anthropology approaches, logistics, works-styles, and increasingly politics and disciplinary norms of researcher-subject relations stood in the way. While Maybury-Lewis expressed interest in continuing to work with Neel, eventually he desisted for reasons that are not entirely clear.³¹¹

For the geneticists, and especially for Salzano, the question of how culture and biology interact in shaping human evolution became a central interest for his long (and ongoing) career. Without looking beyond the confines of one discipline, Salzano explained to me in a 2014 interview, “in the case of the human species, one will not come to a conclusion that is close to reality. That is to say, the human species is conditioned by many factors that are studied by different disciplines. So the focus of a single discipline is very restricted.”³¹² In the years that followed the Xavante Pilot Study, he would continue to correspond and converse with socio-cultural anthropologists, attending meetings of the *Associação Brasileira de Antropologia* and serving on their scientific advisory board for decades. But despite shaping much of his career increasingly with time he also understood interdisciplinarity to be aspirational. Salzano explained, “More and more I am convinced that interdisciplinarity is more of an objective than a reality... Even though

³¹¹ It may have been that the interdisciplinary work did not offer enough professional benefit to be worth the energy dedicated, or that the pace and style of publication did not fit within anthropological scholarly models. It is possible that collaborations with biological sciences were already beginning to be seen with some suspicion within socio-cultural anthropology.

³¹² Salzano, interview, 3 July 2014.

you try to be interdisciplinary, you are restricted by your personal training. So communication, in general communication between people is complicated. It's even more difficult between people with different academic backgrounds."³¹³ But despite the limitations that one form of academic training can produce, discourses and practices of interdisciplinarity were and continue to be promoted as particularly essential for the genetic study of Indigenous people.

The idea that studies of “the human” necessitate multiple approaches is a resurgent theme, from the founding of four-fields anthropology to recent calls to integrate social and biological anthropology.³¹⁴ The Xavante Pilot Study bridged a wide variety of divides. From the differing frameworks of sponsoring institutions, to work in three languages, to the crossing of national borders that separated the “First World” from the “Third World,” the project was transnational in every sense. During their fieldwork the researchers would draw on both elite academic knowledge and lay knowledge, as government agents helped interpret Indigenous subjects. Finally, the researchers would mediate knowledge production directly with Xavante actors, who both helped construct the distinction between “*Índio*” and “*civilizado*” and crossed the concomitant cultural boundary as they engaged their scientific visitors. The sociality of Neel and Salzano’s research agenda was complex.

What the scientists may not have realized during their planning and execution of the Xavante Pilot Study, was that beyond the broad, transnational influence that they

³¹³ Salzano, interview, 3 July 2014.

³¹⁴ Tim Ingold and Gisli Palsson, eds, *Biosocial Becomings: Integrating Social and Biological Anthropology* (New York: Cambridge University Press, 2013).

were hoping to have, there was a less expected, more open-ended local reality to the data they collected. Their attempts at comprehensiveness were seldom attempted by other research groups in lowland Amazonia. For the Xavante, the methodology would also have enduring results. Based on the extensive documentation of the 1962 field season, future researchers—Nancy Flowers, together with Ricardo Ventura Santos and Carlos E. A. Coimbra Jr. from the *Escola Nacional de Saúde Pública*—would build a research program in Xavante territories in the 1990s. The interdisciplinarity that through the WHO and the IBP-HA would shape the study of Indigenous people would also make Xavante communities a particularly compelling resource for interdisciplinary study. Much as the depth of ethnographic data from Maybury-Lewis' early studies would spur interest from social anthropologists, the project of the human geneticists set the stage for a diversifying agenda of inquiries into human health, nutrition, and ecology. And while the Xavante had few means to compel the geneticists to return in 1962, by the 1990s, they had new forms of engaging researchers that would help foster enthusiasm and long-term engagement.

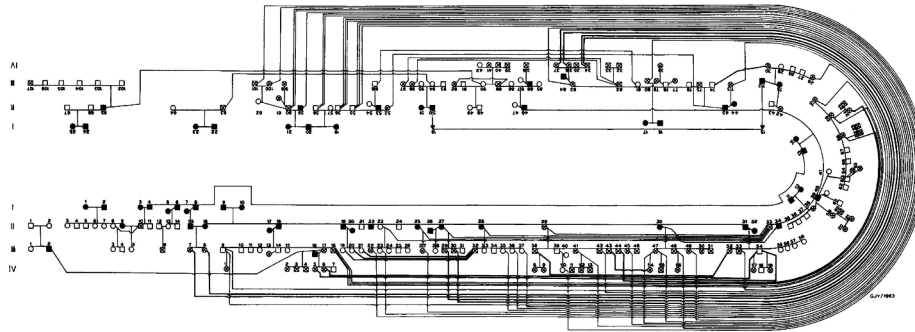


FIG. 2. The village pedigree, combining not only the village examined but its recent offshoot and/or from whom blood specimens were obtained are indicated by an X in the symbol. Marriages known to be consanguineous are indicated by a double-thickness connecting line. (see text). Individuals indicated by solid symbols are deceased. Individuals who were examined

Figure 2: The village genealogy as published in the American Journal of Human Genetics. From: Neel et al. "Studies on the Xavante," foldout between page 92 and 93.

Copyright American Journal of Human Genetics.

Chapter Three

Subject 01: Cold War Masculinity and the Exemplary Indigenous

Introduction: Subject 01

On a July morning in 1962, with the sun already beating strongly by ten o'clock, laboratory technician and field assistant Girley Simões marked a number one on the first square of card stock. Tying a knot and looping it through a pre-punched hole, he passed the string around Apowẽ's neck and fastened the other side. The number hung against the *cacique*'s bare chest, a few inches below his white *tsõrebzu*, the cotton cord necklace worn by Xavante men. With this simple gesture, the famous Xavante leader was designated subject "01" of a pilot study that would structure years of research to come. He was the first person formally included in the interdisciplinary study designed by geneticists James V. Neel and Francisco M. Salzano. Deceased in 1978, he is still studied today.

Over the course of ten days of fieldwork, Apowẽ was transformed from an exceptional leader and national figure into subject "01." In the following weeks and years of genetic and biomedical research, he became an extraordinary research subject. The biological and cultural profile that scientists created of him would continue to drive research even fifty years later. This chapter examines Apowẽ's unusual research subjectivity. It argues that the synergism of his self-fashioning and geneticists' preconceived notions of gender roles fundamentally shaped the theoretical interventions

resulting from their fieldwork in Xavante territories. Apowẽ's performance of masculinity provided the key point of reference to crystalize the scientists' model for pre-history that emphasized the agency of exceptionally masculine men in propelling human evolution.³¹⁵

The scientists who studied Apowẽ saw him paradoxically as both unique and representative, and as both remarkable and typical. With his personal history of aggression, his numerous wives and offspring, and his polarizing leadership role in his village, geneticists understood Apowẽ to embody a "natural" masculinity characterized by sexual prowess, violence, and ruthlessness. The leader came to stand in first for his community, then his people, and finally for the charismatic male leaders of human evolutionary history. The already-iconic *Índio* became an iconic genetic subject, cited by name into the twenty-first century.

Scientists' perceptions were fundamentally influenced by political and social context. It was the fame that the Xavante in general, and Apowẽ specifically, had garnered for ferocity and strength that attracted the attention of scholars. By the fieldwork of 1962, Brazilian expansionism was wreaking havoc, fueling the violent confrontation between Xavante communities as well as with encroaching settlers—the same violence that seemed so "natural." Nationally in Brazil and internationally, especially in the United States, concerns about male virility, strength, and authority were commonly articulated in

³¹⁵ As Milam and Nye have observed, there has been extensive interest in the history of gender in the sciences, but with limited attention to the workings of masculinity. This chapter adds to this newly emerging literature. See: Erika Lorraine Milam and Robert A. Nye, "An Introduction to Scientific Masculinities," *Osiris* 30, no. 1 (2015): 1–14, doi:10.1086/682953. See also the other contributions to the 2015 *Osiris* on Scientific Masculinities.

terms of the communist threat and prioritized as a key issue in the defense “the West.”³¹⁶

In this context, Apowẽ became a particularly compelling subject because the public image he cultivated prior to the scientists’ visit fit with their notions of primordial masculinity.³¹⁷

Indigenous people have long been essential sources of material for biomedical and genetic investigation. Tissue samples, vials of blood, and demographic data have fueled investigations into debilitating neurological disorders, pre-historic human migration, and human immune responses. This research has won Nobel Prizes and international recognition. It has also drawn the attention of critical postcolonial historiography, which has productively analyzed how power and privilege have been levied and contested in the context of biomedical research.³¹⁸ At the forefront of the turn

³¹⁶ Carolyn Herbst Lewis, *Prescription for Heterosexuality: Sexual Citizenship in the Cold War Era* (Chapel Hill: University of North Carolina Press, 2013): 2–6, 78–79. Also, on the issue of masculinity during Brazil’s military dictatorship, see James Green and Renan Quinalha, eds. *Ditadura e homossexualidades: repressão, resistência e a busca da verdade* (São Carlos: Editora da Universidade Federal de São Carlos, 2015), which discusses fears about homosexuality and increased surveillance and persecution during the 1964–1985 period. On policing of masculinity in the armed resistance movement, see James N. Green, “‘Who Is the Macho Who Wants to Kill Me?’ Male Homosexuality, Revolutionary Masculinity, and the Brazilian Armed Struggle of the 1960s and 1970s,” *Hispanic American Historical Review* 92, no. 3 (2012): 437–69.

³¹⁷ The research on Apowẽ fits remarkably well with another almost contemporaneous research agenda, sponsored by the Guggenheim foundation. These scientists’ investments in homosocial relations, and their inquiries into aggression and male leadership were informed by the context of the women’s movement, as Erika Milam shows in Erika Lorraine Milam, “Men in Groups: Anthropology and Aggression, 1965–84” *Osiris* 30, no. 1 (2015): 66–88, doi:10.1086/682966.

³¹⁸ Early examples that drew heavily on Foucauldian approaches include David Arnold, *Colonizing the Body: State Medicine and Epidemic Disease in Nineteenth-Century India* (Berkeley: University of California Press, 1993) and Megan Vaughan, *Curing Their Ills: Colonial Power and African Illness* (Stanford: Stanford University Press, 1991). Later work brought more attention to the variations and spaces for the exercise of agency within colonial health and research settings, including: Nancy Rose Hunt, *A Colonial Lexicon: Of Birth Ritual, Medicalization, and Mobility in the Congo* (Durham: Duke University Press, 1999); Julie Livingston, *Improvising Medicine: An African Oncology Ward in an Emerging Cancer Epidemic* (Durham: Duke University Press, 2012). For work focused on the US colonial context, see: Warwick Anderson, *Colonial Pathologies: American Tropical Medicine, Race, and Hygiene in the Philippines* (Durham: Duke University Press, 2006); Laura Briggs, *Reproducing Empire: Race, Sex, Science, and U.S. Imperialism in Puerto Rico* (Berkeley: University of California Press, 2002).

towards materiality in the history of science and medicine, much of this literature has focused on the procuring, storage, circulation, and study of Indigenous samples.³¹⁹ Samples have constituted part of a diverse, puzzling, and at times troubling ethics of practice in global research. Some scholarship has proposed that this research is inherently exploitative due to the impossibility of true informed consent, or the imbrication of colonial or settler-colonial agendas with scientific priorities.³²⁰ Other work makes clear that under certain conditions, Indigenous groups embrace and mobilize biomedical research to their own political and social ends.³²¹

In the process of turning people into biomedical data, however, the individual is often assumed to be lost, disappeared into the aggregate of unnamed human subjects. This is due both to now standard practices to anonymize research subjects, and to the often short or limited presence of researchers in Indigenous communities where data collection rarely includes longhand narrative field notes.³²² This chapter suggests,

³¹⁹ Anderson, *Collector of Lost Souls*; Radin “Latent Life”; Emma Kowal, “Orphan DNA: Indigenous Samples, Ethical Biovalue and Postcolonial Science,” *Social Studies of Science* 43, no. 4 (2013): 577–97; Emma Kowal, Jenny Reardon and Joanna Radin, “Indigenous Body Parts, Mutating Temporalities, and the Half-Lives of Postcolonial Technoscience,” *Social Studies of Science* 43, no. 4 (2013): 465–83; M. Susan Lindee, “Voices of the Dead: James Neel’s Amerindian Studies,” in *Lost Paradises and the Ethics of Research and Publication*, ed. Francisco M Salzano and A. Magdalena Hurtado (Oxford: Oxford University Press, 2003), 27–48; Joanna Radin, “Life on Ice: Frozen Blood and Biological Variation in a Genomic Age, 1950–2010,” PhD, University of Pennsylvania, 2012; Ricardo Ventura Santos, “Indigenous Peoples, Postcolonial Contexts and Genomic Research in the Late 20th Century—A View from Amazonia (1960–2000),” *Critique of Anthropology* 22, no. 1 (2002): 81–104.

³²⁰ Mark Munsterhjelm, “Beyond the Line: Violence and the Objectification of the Karitiana Indigenous People as Extreme Other in Forensic Genetics,” *International Journal for the Semiotics of Law* (2014): 1–28; Kim TallBear, “Narratives of Race and Indigeneity in the Genographic Project,” *Journal of Law, Medicine and Ethics* 35 (2007): 412.

³²¹ Michael Kent, “The Importance of Being Uros: Indigenous Identity Politics in the Genomic Age,” *Social Studies of Science* 43, no. 4 (2013): 534–56; Kim TallBear, *Native American DNA: Tribal Belonging and the False Promise of Genetic Science* (Minneapolis: Univ Of Minnesota Press, 2013).

³²² A key exception is Anderson, *Collector of Lost Souls*. Anderson explores Gajdusek’s repeated forays into colonial New Guinea and his interactions with the Fore in his quest to understand the devastating neurological disease of Kuru; Nancy D. Campbell and Laura Stark, “Making up ‘Vulnerable’ People:

however, that some Indigenous subjects had far greater impact on the knowledge produced than simply becoming a series of numbers in a database. Much like thousands of other Indigenous subjects studied by human geneticists and biological anthropologists in the second half of the twentieth century, Apowē's blood, photographs, fingerprints, and anthropometric measurements were collected and circulated, back to the laboratories of Porto Alegre, Rio de Janeiro, and Ann Arbor. As they analyzed the collected materials, scientists reproduced the persona Apowē had crafted for himself, reconstituting him into a model for the selection, maintenance, and propagation of human genes.³²³

The Xavante as a Compelling Case

In their preparations for the first field expedition, conceptualized as a pilot study, Neel and Salzano had searched extensively for a socio-cultural anthropologist who could complement their expertise, as discussed in Chapter 2. They chose to conduct their first study in a Xavante village thanks to hearing of David Maybury-Lewis' work, and his willingness to join the expedition cemented the choice.³²⁴ The model of interdisciplinary research that they developed, became widely influential, and was the primary driver—according to their own accounts—of their first field season. Here I turn to focus on the context and content of that fieldwork. I examine how the geneticists' notions of the

Human Subjects and the Subjective Experience of Medical Experiment," *Social History of Medicine* 28, no. 4 (2015): 825–48, doi:10.1093/shm/hkv031.

³²³ Rebecca Lemov has explored the fascinating history of Don C. Talayesva, a Hopi man who "achieved the status of the most intensively documented non-literate person in the world" according to Dorothy Eggan, one of the anthropologists who worked with him. Rebecca Lemov, "The 341 Dreams of the Sun Chief, or How Dreams and Lives Turn Into Data," presented at the Max Planck Institute for the History of Science, 15 October 2014.

³²⁴ Francisco M. Salzano to James V. Neel, 8 March 1962, Neel to Salzano, 20 March 1962, and Salzano to Neel, 11 April 1962, Neel Papers, APS.

people they were visiting would influence and be influenced by what they perceived in the field. How did Apowẽ come to be an icon of human evolution?

As mentioned briefly in Chapters 1 and 2, in the years leading up to the arrival of the first researchers, the Xavante had been widely represented in the national news media in Brazil. During the *Estado Novo*, populist dictator Getúlio Vargas promoted westward expansion and developmentalism within Brazil as a mechanism to unify the country. Government publicity for the nationalist “March to the West” romanticized uncontacted Indigenous peoples of Central Brazil as a reserve of strength, resourcefulness, and authenticity.³²⁵ Hostile to the outsiders invading their land, however, the Xavante made headlines with their fierce resistance to “pacification.”

The “March to the West” led to extensive encroachment of Xavante territory and catapulted the Xavante to fame for their violent responses. Locally, the Xavante were infamous; they had successfully kept invaders out of large tracts of Mato Grosso. Along the Rio das Mortes, in the 1930s and 1940s Xavante bands, reportedly led by Apowẽ, killed both Salesian missionaries and employees of the government’s SPI who sought to establish contact.³²⁶ This failure to succumb to “pacification” became increasingly problematic for the government as Xavante hostility threatened the progress of the *Expedição Roncador-Xingu*. The expedition, which Seth Garfield has described as the “centerpiece of the March to the West,” began in 1943 and crossed Central Brazil

³²⁵ For an overview of discourses regarding Indigenous peoples under the *Estado Novo*, see Garfield, *Indigenous Struggle*, 23–44. Also see, Guzmán *Native and National in Brazil*, 124–30.

³²⁶ On Salesian attempts to pacify the Xavante in 1933, see *ibid.*, 53; On the SPI’s Pimentel Barbosa and assistants’ murder in 1941, *ibid.*, 55.

building roads and opening up airstrips.³²⁷ The subject of extensive media coverage, the risk of failure in the face of Xavante resistance represented a serious concern for the government.³²⁸

The *Expedição* proceeded cautiously, and eventually in 1946 one group of Xavante finally chose to establish permanent contact with the government officials that courted them. Led by Apowê, the group exchanged gifts with the *warazú* expedition members. This contact at Wedezé unleashed a wave of popular coverage, from travel accounts to documentary film.³²⁹ The media storm was a story of the success of government outreach, and the progress of unifying dispersed regions of the country to render the hinterlands economically productive. Widely represented in the national media, Garfield argues, “The Xavante had become the first indigenous group mass marketed by the media.”³³⁰ They were celebrated as fierce and untamable, but once “tamed” state actors and aligned media simultaneously positioned them as representative of the strong, natural heritage of Brazil and in need of the tutelage of a benevolent State for the sake of their betterment.³³¹

The masculine appeal of the Xavante and those explores who dared contact them even found coverage in US based magazines. *Time Magazine* ran a story reporting on the success of the pacifying mission, titled—probably with little sense of the violence of conquest—“Love Conquers,” explaining that as the Xavante stood in the way of “Brazil’s

³²⁷ Ibid., 45.

³²⁸ Ibid., 57.

³²⁹ de Souza, *Entre os Xavante do Roncador*; de Souza, *Os Xavante e a civilização*; Fonseca, *Frente a frente com os Xavantes*; Garfield, *Indigenous Struggle*, 59.

³³⁰ Garfield, *Indigenous Struggle*, 59.

³³¹ Ibid., 23–44.

great dream—The March to the West,” the “Airmen of the Brazilian Central Foundation, a grandiose colonizing scheme, dropped on them pots, pans and even pictures of Hollywood pinup girls. That only frightened the Indians.”³³² In the end, patience, love, and gifts of machetes and bright cloth convinced the Xavante to enter into peaceable contact, the magazine reported, even if pinup girls had failed. A 1952 issue of *Male* magazine reported on the Xavante with a byline that read, “When you wander into unexplored territory, your life may depend on how you react to native taboos. A wrong move could mean death.” Describing the Xavante as a “stone-age” tribe, the adult men were collectively referred to as “the war leaders.” Despite two pages of photographs, the only image of a Xavante woman—holding a child—was captioned “Indian mother distrusts camera. Glares at us. The tribe’s women rarely speak.”³³³ The other images showed men shooting arrows, trekking, and dancing. The tribe was framed to appeal to the manly men interested in the adventure stories and pictures of half clad women that occupied the advertisements on either side of the article. There is no evidence that the scientists saw these articles, but they speak to the hyper-masculine reputation that preceded the Xavante.

Well before Maybury-Lewis arrived in Wedezé in 1958, he was aware of the violent characterizations that the Xavante had earned through their interactions with missionaries and local *fazendeiros*. In fact, in the lead up to his departure for the field, their reputation for ruthlessness kept him awake at night: “I had nightmares in which they shouted (but always with Sherente voices), ‘Don’t come here. Keep out. If you come

³³² “Love Conquers,” *Time* (2 September 1946): 35.

³³³ “Lost Tribe of Brazil,” *Male* (May 1952): 26–28.

here, we will kill you.’ Worse still, I had nightmares in which they all shouted at me and I could not understand a word of what they said.”³³⁴

Apowẽ was featured prominently enough in the literature that Maybury-Lewis reported having known of the chief’s repute even before arriving in the field. The anthropologists described his first meeting with Apowẽ in *The Savage and the Innocent* saying, “I watched his aquiline features and the greying shoulder-length hair and could not help feeling that the man would not have looked out of place as a doge of Venice. Mentally I told myself to stop romanticizing. I was simply reacting to his reputation, I thought, for this was Apewen, perhaps the best-known Shavante in Brazil. He was thought to have led the band that massacred Pimentel Barbosa and his companions of the Indian Protection Service.”³³⁵ And Maybury-Lewis did not restrict his emphasis on Apowẽ’s reputation to his semi-popular work. In *Akwẽ-Shavante Society* he qualified the leader as “an old Shavante renowned among his compatriots and also among the local Brazilians as a ‘strong chief’.”³³⁶ This emphasis on Apowẽ’s leadership in the deadly attacks on Salesians and SPI employees of the 1940s and early 1950s would persist throughout scientific publications that cited the leader by name.

Once included in the genetic study of Neel, Salzano, and colleagues, Apowẽ’s polarizing political persona and his numerous offspring would be combined with his history of violence to turn Apowẽ from a ‘strong chief’ into an icon of selection. But before he could be converted into a genetic legend, the leader, visionary, tyrant, and

³³⁴ Maybury-Lewis, *Savage and the Innocent*, 153. As noted in Chapter 1, this reputation for man-to-man warfare motivated Maybury-Lewis’ decision to travel to the field with his family.

³³⁵ *Ibid.*, 168.

³³⁶ Maybury-Lewis, *Akwẽ-Shavante Society*, 37.

grandfather needed to become a simple subject of genetic investigation.

Study Inclusions: From Extraordinary Leader to Ordinary Subject

On the first day of their expedition, the team of six *warazú* men—the researchers—went to the village to meet Apowẽ and the rest of the community just before sundown to explain their research and offer gifts.³³⁷ Since Maybury-Lewis had arrived with the other outsiders, the Xavante likely interpreted the team of scientists in light of their familiar companion.³³⁸ Aided by Leitão, the scientists explained their research in time for the village’s evening elder men’s council meeting, the *warã*.³³⁹ The men’s council decided that they would accept the project, but that the men would be examined first: Neel wrote, “...we began our examinations, beginning, at their insistence, with the males (since the Shavante were not yet sure of our intentions toward their women.)”³⁴⁰

In fact, they began their study with Apowẽ. Despite his fame, it is unlikely that the scientists thought twice as they hung the numbered card around the old man’s neck. (See figure 3). Prior to their fieldwork in Wedezé, Salzano and field technician Simões had already conducted a number of genetic studies in Kaingang villages in the south of Brazil.³⁴¹ There they had developed of inviting the *cacique* to be the first participant for

³³⁷ Francisco M. Salzano, *Caderno de Campo #1*, Personal Papers of Francisco M. Salzano, UFRGS, Porto Alegre (hereafter Salzano Papers).

³³⁸ Maybury-Lewis, *Akwẽ-Shavante Society*; Maybury-Lewis, *Savage and the Innocent*.

³³⁹ As I discuss at more length in Chapter 5 over time the Xavante adjusted and repurposed existing institutions to help them engage with and manage researchers productively. The *warã* is the first example of this adaptation.

³⁴⁰ Neel, *Physician to the Gene Pool*, 126.

³⁴¹ Salzano reflected on this July 1964 fieldwork as a simple continuation of the work to which he and Simões were accustomed (Salzano, interview, 11 July 2012). For Simões, however, it was a completely different experience. Girley V. Simões, interview with Rosanna Dent, 10 December 2013, Porto Alegre.

each study. This practice, they held, helped build the trust of other village residents, while demonstrating respectful precedence for the leadership. Once a chief had shown his own amenability to the research protocol, it was hoped that other community members would follow suit.³⁴²

On the morning of the first day of the study, with a number of different stations set up at the SPI Post a kilometer's walk from the village, the research began with this simple act of giving each individual a number. Over the next hour or so, Apowẽ was ushered through a battery of questions, tests, and measurements in the schoolhouse. Beginning with Salzano, with Leitão interpreting, Apowẽ recounted the demographic data of his family, which the geneticist noted onto a pre-prepared form.³⁴³ He listed the names of his five wives, his brothers and sisters, his 23 surviving children, their sex and age. Salzano also asked about his children who had died, where each person in the family had been born, and who from the family—if anyone—had left the village.³⁴⁴ As the interview drew on and on, Salzano's colleagues paled with the thought that each interview might take as long as the first.³⁴⁵

After the demographic interview, the German physical anthropologist Keiter waited with a scale, tape measure and a set of calipers to take a series of eleven measurements including nine of Apowẽ's facial features and head. In the background the Xavante leader heard his eldest son Warodi answering the same set of questions on the other side of the room. The anthropometrists' calipers were far faster than the extensive

³⁴² Salzano described this decision to focus first on the *cacique* as an intuitive decision. Francisco Mauro Salzano, interview with Rosanna Dent, 17 August 2015, Porto Alegre.

³⁴³ Salzano, interview, 11 July 2012; Also: Neel et al., "Studies of the Xavante," 90.

³⁴⁴ Neel et al., "Studies of the Xavante," 90.

³⁴⁵ Salzano, interview, 17 August 2015.

questionnaire. Measurements complete, Keiter motioned the chief outside where he took five standardized photographs, each with a slightly different position of the head. Simões then led the *cacique* to stand at a line in the red clay soil twenty feet from the side of the post building. Pointing at a chart in black and white hung up on the wall of the schoolhouse, Simões and Neel mimed for Apowẽ to raise or lower his arm to show the orientation of each symbol.³⁴⁶ The results of the vision test recorded, the final visit would be to the clinic, where Neel performed a comprehensive medical examination, noting everything from the shape of the leader's liver to the presence or absence of irregularities on his irises. Visitors were cause for much curiosity in the village, and so it is likely a large group of onlookers followed the progress of the researchers through each step of the protocol.

Almost two weeks later, on the last two days of the scientists' stay in the village, the final procedure involved taking a saliva sample, dermatoglyphic impressions of each hand and each foot, and blood samples from the individuals who had undergone the battery of other exams. Filling two vacutainers (small, sterile, vacuum-filled tubes containing anti-coagulant) for each subject, the next day the perishable blood samples would be whisked to Rio with the scientists and their equipment and promptly frozen. They would provide the material basis for the analysis of eighteen different genetic markers in the first round of studies, and would be revisited after decades at -70 degrees Celsius when new DNA based analysis techniques became available.³⁴⁷

³⁴⁶ Simões, interview, 10 December 2013.

³⁴⁷ Neel et al., "Studies of the Xavante," 78; T. Hünemeier et al., "Cultural Diversification Promotes Rapid Phenotypic Evolution in Xavante Indians," *Proceedings of the National Academy of Sciences* 109, no. 1

How might Apowẽ have experienced the process of becoming a subject of the puzzling scientific activity underway at the post? Some aspects of the study would have been familiar. During his 1958 field work, Maybury-Lewis had spent a good deal of time collecting genealogical data in order to try to figure out Xavante social structure; Salzano's questions were much along the same lines. Neel's medical examination was certainly more extensive than the kinds of check-ups that SPI medical teams would have offered, but would have been somewhat familiar. It is hard to know what exactly the Xavante thought about the extensive measurements and the blood and vision tests. But the experience made a deep enough impression on Sidówi as a young man that fifty years later in his old age he could still mime the way the scientists had taken the measurements.³⁴⁸

Scientific Motivations: The Search for Mechanisms of Human Evolution

Salzano and Neel developed their studies of Indigenous groups to address a pressing scientific question; they hoped Indigenous populations could provide insight into the underlying selection mechanisms driving human evolution. According to Neel's estimation, human geneticists had made great progress in thinking about "the origin and persistence of genetic differences between and within populations" during the 1920s and 1930s with the development of statistical approaches to population modeling. "But then,"

(2011): 73–77, doi:10.1073/pnas.1118967109; Pontus Skoglund, Swapan Mallick, Maria Cátira Bortolini, Niru Chennagiri, Tábita Hünemeier, Maria Luiza Petzl-Erler, Francisco Mauro Salzano, Nick Patterson, David Reich, "Genetic evidence for two founding populations of the Americas," *Nature*, 525 (2015): 104–108; Maria Cátira Bortolini, personal communication with the author, Porto Alegre, 18 August 2015.

³⁴⁸ Tsuptó Buprewên Wa'iri Xavante, Barbosa Sidówi Wai'azase Xavante, Luiz Hipru Xavante, and Agostinho Seseu Xavante, interview with Rosanna Dent, 4 June 2014, Água Boa, MT. Sidówi was the individual who mimed.

he wrote “as attention turned to the study of actual populations, much of that impetus was lost, despite the manner in which concern over our increasing exposure to radiation and other mutagenic agents has underscored our relative ignorance regarding population genetics.”³⁴⁹ For Neel, the comparative study of populations he referred to as primitive and civilized would provide the material with which population geneticists could start to make sense of how selection worked in humans.

Neel explored the potential of this approach in a 1958 article entitled “Natural Selection in Primitive and Civilized Human Populations.” Published shortly after Salzano completed his postdoctoral studies with Neel in Ann Arbor, the programmatic piece laid out the scientific incentives that would motivate their future collaboration. Neel described the problem facing the field writing, “The principle of natural selection as a guiding factor in human evolution is today universally accepted. However...our knowledge of the actual workings of natural selection in human populations is almost nil.”³⁵⁰ Neel went on to outline existing areas of research that could potentially address selection. The key area of interest for his work would be the question of differential individual reproduction.

Neel’s thinking about selection, fitness, and reproduction, aligned well with other population geneticists; those working in this field considered reproductive success as the primary measurement of fitness, and thus the most important factor in determining selection.³⁵¹ Evolution as driven by selection could only occur when populations were

³⁴⁹ James V. Neel, “Between Two Worlds,” 9.

³⁵⁰ James V. Neel, “Natural Selection in Primitive and Civilized Human Populations,” 43.

³⁵¹ As Erika Milam has shown, this exclusive focus on genetic contribution to the next generation at the expense of other questions of health and vitality separated geneticists from their colleagues in organismal biology. See Erika Milam, *Looking for a Few Good Males: Female Choice in Evolutionary Biology* (Baltimore: Johns Hopkins University Press), kindle locations 2713–2734.

growing or shrinking significantly, or when some individuals had many more children than others. The cause for this “differential reproductive success” and the resulting selection, Neel continued, “may be either on biological or cultural grounds. It will often be difficult to distinguish between the two types.”³⁵²

In his 1958 article, Neel focused on the question of individuals and differential fertility *within* a group. He understood this to be a factor where culture might influence biology. Citing literature on the cultural determinants of fertility *between* groups, he instead stressed the importance of examining these factors in relation to individuals, because “...within any group, there are still great ranges in fertility. A comprehensive understanding of the occurrence and the cause of individual fertility differences in various societies is certainly one of the outstanding genetic objectives of our day.”³⁵³ Of course, individual fertility differences could be determined by a variety of factors. Child survival, polygamy, infanticide, extramarital births, and fertility control were all of great interest to determine the relative contributions of individuals to the gene pool of a population. As such, these were some of the key parameters to be built into the methodology of the Xavante Pilot Study. Plans to sample as comprehensively as possible, including as many members of each village as possible, would allow the researchers to determine within-group variation and the unequal contributions of *individuals* to subsequent generations.

As others have elegantly discussed, the turn to focus on Indigenous groups, those dubbed primitive by scientists hailing from many disciplines, was expected to help

³⁵² Neel, “Natural Selection in Primitive and Civilized Human Populations,” 783.

³⁵³ *Ibid.*, 789.

explain deep human history.³⁵⁴ As Neel would write in the introduction to the first publication based on their research at Wedezé, “The time factor in evolution being what it is, there can be little doubt that many—most—of the genetic attributes of civilized man have been determined by the selective pressures and breeding structures of these primitive communities.”³⁵⁵ However, Neel’s purpose of understanding hunting-gathering groups was not necessarily to make sense of them for their own sake. Again and again throughout his writing, he argued for the “primitive” as a means to make sense of the “civilized” present and human future. He continued, “If we would understand modern man, we must study such of these primitive groups as still remain in a way in which they have rarely if ever been investigated to date.”³⁵⁶ This was a position that he had outlined before his first co-ventures in South America, “The need is great for a variety of parallel studies on selective factors in advanced and primitive societies. Such studies are at the same level of importance as investigations on spontaneous mutation rates, the nature of the gene, skeletal remains, or the effects of irradiation on human populations, in our efforts to understand man’s past and to predict his future.”³⁵⁷ The study of the Xavante and particularly the figure of Apowẽ would become a means to study the past and create a vision of a masculine natural state that should inform the management of the human gene pool in the future.

Cold War Context: Masculinity Between the Bombs

³⁵⁴ Radin, *Life on Ice*; Santos, “Indigenous Peoples, Postcolonial Contexts, and Genomic Research”; Santos, Lindee, and Souza, “Varieties of the Primitive,” 723–35.

³⁵⁵ Neel et al., “Studies on the Xavante Indians,” 52.

³⁵⁶ *Ibid.*

³⁵⁷ Neel, “Natural Selection in Primitive and Civilized Human Populations,” 783.

Neel's focus on questions of reproductive control has to be situated in its Cold War moment. Sandwiched between WWII and Paul Ehrlich's *The Population Bomb*, Neel articulated strong concerns about the direction of human evolution, and whether as a species humanity was deviating from the path of natural betterment.³⁵⁸ His writings did not uniformly replicate earlier discourses of the eugenics movement; however, they shared some important motivations.³⁵⁹ Neel's concerns about the future grew out of a deep pessimism about what he saw as the misguided present.³⁶⁰ Part of the answer to addressing this pessimistic present was through the careful understanding and application of knowledge about human population structure. As Neel and Salzano would write in their 1967 overview of the Xavante research, "The future of efforts to utilize genetic knowledge for the good of man lies far less in the spectaculars of applying transduction, transformation, or cloning techniques to man than in the acquisition of the kinds of understanding of genetic population dynamics which will permit man to develop the society most consistent with both his present genetic endowment and his continuing

³⁵⁸ Paul R. Ehrlich's 1968 best-seller popularized worries from the 1950s and 1960s about the "population explosion." It predicted mass starvation and advocated aggressive population control measures. See Paul R. Ehrlich, *The Population Bomb*, (London: Macmillan, 1968).

³⁵⁹ Much scholarship has shown that as the field of human genetics was emerging in the post-war period, scientists went to pains to explicitly distance themselves from the atrocities of Nazi Germany. They offered discourses of abrupt disengagement from eugenics emphasizing the objectivity of studies of heredity now carried out in fruit flies, or mediated through the impartial quantification of blood groups. However, many of the techniques, methods, and analytic tools for analyzing human difference carried over from the pre-war science of human classification. A rich literature explores the continuities and disjunctions of the transition from eugenics to human genetics. A classic study is Kevles, *In the Name of Eugenics*. Another key study is Stern, *Eugenic Nation*. On the gradual increase in the importance of thinking genetically about human disease, see Lindee, *Moments of Truth*.

³⁶⁰ Neel's intimate knowledge of the suffering caused by the atomic bomb through his extensive experience working with the Atomic Bomb Casualty Commission in Japan only served to accentuate his worries about the future of humanity. See M. Susan Lindee, *Suffering Made Real: American Science and the Survivors at Hiroshima* (Chicago: University Of Chicago Press, 1997).

evolution.”³⁶¹ Neel found both biological and social promise in the bodies and lives of his Indigenous research subjects. Apowẽ, specifically, provided compelling material for study within the context of these concerns, both because of his social position as chief and his unique individual reproductive success.

Politics and Polygyny: Apowẽ in the Initial Xavante Publication

Before the scientific team ventured into the field, Neel had outlined interests that meant Apowẽ’s life and genes would contribute to the study’s major findings. Among other things, the geneticists and their collaborators went into the field to find individual fertility differentials. They wanted to see which cultural determinants defined an individual’s reproductive success. Apowẽ’s profile brought together strength, violence, leadership, and perhaps most importantly, what they would call a “very disproportionate” contribution to subsequent generations.³⁶² His masculine prowess first as a fierce leader and second as a fertile progenitor came to be of great interest in determining his individual impact on the genetic profile of his community. In turn, it allowed the scientists to think about new models for microevolution.

Much as the scientists’ expedition replicated existing imaginaries of masculine daring and challenge for the sake of scientific exploration, the Xavante men they encountered lived up to the group’s reputation: “The general impression of the men was of exuberant health and vitality. They were erect in carriage, deep-chested, and very well

³⁶¹ James V. Neel and Francisco M. Salzano, “Further Studies on the Xavante Indians: X. Some Hypotheses and Generalizations Resulting from These Studies,” *American Journal of Human Genetics* 19, no. 4 (1967): 569.

³⁶² Neel et al., “Studies on the Xavante Indians,” 131.

muscled, with a notable absence of adiposity.”³⁶³ The scientists arrived to find a ritual underway, noting in their field notes that the men danced on the first evening they visited the village.³⁶⁴ The rhythmic stomp and chant of the dancing made a great impression on the visitors, who noted not only the visual and acoustic impact, but marveled at how the men maintained their energy to sing throughout the night.³⁶⁵

Other cultural practices made similarly strong impressions on the scientists: “In the examinations of the Shavante males, we were initially puzzled by a universally present callus on the right shoulder. Sudden insight came the day we witnessed our first *buruti* race... After the race we found one of the “batons” to weigh 75 kilograms and the other 85! All the adult males participate!”³⁶⁶ The spectacle of teams of men sprinting with the great palm logs and passing them off to one another only served to confirm the scientists’ sense that they had found a population of great masculine vitality.

For a group that showed such strength and “bellicosity,” the male leader could only be expected to be a force to be reckoned with. Building on Maybury-Lewis’ assessment and Apowẽ’s existing reputation, the researchers wrote that as “the senior man of the dominant faction in the village,” Apowẽ was a particularly “strong” chief.³⁶⁷ Over the course of their 88-page treatise, the researchers described Apowẽ’s history of violent conflict three times. Based on Apowẽ and on Maybury-Lewis’ experiences in

³⁶³ Ibid., 110. The scientists’ perceptions of the women were quite different: “By contrast, the women, although in apparent good health and nutrition when young, gave an impression of early aging, an impression more than sustained by the results of the physical examinations. Indeed, one of the most striking impressions of this study was of the different medical worlds of men and women,” (ibid.).

³⁶⁴ Salzano, *Caderno de Campo #1*, Salzano Papers.

³⁶⁵ Neel, *Physician to the Gene Pool*, 150.

³⁶⁶ Ibid. The geneticists failed to note that Xavante women also log race, often carrying logs as heavy as 50kg. They also perform the vast majority of the heavy lifting in day-to-day life.

³⁶⁷ Neel et al., “Studies on the Xavante Indians,” 60.

other villages, they made multiple generalized references to violence and the chieftaincy. First they noted Apowê's involvement in the 1941 slaughter of government functionaries, and then went on to twice describe the results of a "purge" of an opposing faction within the village. Their prose evoked a brutal and daring political ploy to consolidate his leadership in 1953, "On that occasion eight men were killed in their sleep. Their kinsmen and factionaries fled, resulting in a loss to the village of about 30 to 40 people... The action seriously weakened Apewe's village, but as a calculated risk, it appears that Apewe gambled and won. He is now incontestably chief of his community and is regarded by all Xavante as one of the strongest chiefs in the tribe."³⁶⁸

Subsequently in his 1967 *Akwê-Shavante Society*, Maybury-Lewis would report that the strength of Apowê's chieftaincy was due to his place "at the head of a faction which is numerically strong, undivided, and which infiltrates his community at all levels."³⁶⁹ Maybury-Lewis continued, "He certainly secured the chieftaincy by being the shrewdest leader of the most ruthless faction in competition for it. Both before and after he had established himself as chief of his group he was responsible for the killing or expulsion of those who stood in his way."³⁷⁰ As the geneticists inquired into Apowê's polarizing role in the political life of his community, the interaction between Xavante kinship structures and political conflict emerged as key place to make sense of the differential individual reproduction for which they were searching.

The geneticists were particularly interested in exogamous moieties, which

³⁶⁸ Neel et al., "Studies on the Xavante Indians," 60.

³⁶⁹ Maybury-Lewis, *Akwê-Shavante Society*, 193.

³⁷⁰ Ibid.

determined both who could marry whom and tended to predict political factions. These political allegiances and fissures were of great interest to the geneticists, since their genealogical nature implied not only social, but also biological relationship. Trying to appreciate the way social structure might determine genetic population structure, the moiety system promised to help the scientists meet the first objective of their Xavante pilot study, “to identify those cultural elements with particularly biological implications.”³⁷¹

Political cunning, ruthlessness, and carefully calculated risks—particularly Apowẽ’s—led to village splits along biologically important moiety lines. This would become one of their most important findings but was not readily apparent at first. Trying to make sense of the data only a few weeks after returning to the United States, Neel wrote to Maybury-Lewis: “You know how disappointed we were to find that there had been a village schism in the last three years, but it is perhaps important to make some brief description of this...”³⁷² The drop in the number of inhabitants from about 220 in 1958 when Maybury-Lewis conducted the bulk of his fieldwork to only 120 in 1962 risked compromising the sample size of their study. This weakened their ability to draw statistically significant conclusions. And yet as they explored their data, the schism that Apowẽ had caused also provided the most compelling mechanism to generate genetic change over time.

By the time the final 1964 paper went to press, the researchers were able to cast

³⁷¹ Neel et al., “Studies on the Xavante Indians,” 53. Research by anthropologist James Welch would present a different analysis of factionalism and village splits, challenging Maybury-Lewis’ assertion the moiety belonging most determined allegiance. See Welch, “Age and Social Identity,” 324-326.

³⁷² Neel to Maybury-Lewis, 14 September 1962, Maybury-Lewis Correspondence (1 of 2), Neel Papers, APS.

what had initially seemed to be a setback as a resource for understanding evolutionary history. Describing what they first called a “schism,” and would later come to call a “fission,” the researchers wrote, “The opportunity to record to some measure how this important sampling event in the lives of such populations occurs offsets to some degree our disappointment at finding a smaller village than anticipated.”³⁷³ By considering the people in the pedigree who were unable to be examined, and thus assumed to be living in the splinter village, they deduced that groups of brothers, predominantly of Apowẽ’s opposing moiety, left the village together. Thus individuals who were closely biologically related tended to form new villages in a highly “non-random” pattern.

Furthermore, the scientists suggested that these kinds of kinship splits could likely be considered a general pattern: “We regard it as fortunate that we were able to provide preliminary documentation of the biological lines along which a village split occurs, since this is a process which must have occurred frequently in the history of man.”³⁷⁴ And so factionalism, which would constitute a major area of research interest in subsequent work by both social anthropologist Maybury-Lewis and geneticists Neel and Salzano, came to represent “non-random sampling” as human groups formed new communities.

While the first key finding about Apowẽ was his unabashed use of force in his political dealings, the second compelling characteristic was his prolific fertility. At first glance, Apowẽ may not have seemed an ebullient a masculine subject. Greying and already in his fifties or sixties, he was old by Xavante standards in 1962. And yet, as if to perfectly complete the imaginary of a virile leader, Apowẽ had more wives and children

³⁷³ Neel et al., “Studies on the Xavante Indians,” 90.

³⁷⁴ *Ibid.*, 127.

than any other member of the village. Neel had outlined polygamy as a possible source for individual fertility differentials in his 1958 thought piece.³⁷⁵ Apowẽ was an exceptional subject as described in the 1964 publication, "...the reproductive history of the chief, Apewe, is striking. As befits the chief, he had had more wives (five) than any other member of the tribe."³⁷⁶ Apowẽ's twenty-three surviving children impressed the researchers. But they struggled tremendously to understand the relationships between his wives.

They exchanged dozens of letters trying to establish whether Apowẽ's wives were full sisters, half sisters, or cousins. Maybury-Lewis lamented his inability to provide more clarity, as two of Apowẽ's five wives were not living in the same household as the *cacique* at the time of the 1962 visit. He wrote to Salzano apologizing for not having the pertinent information regarding a wife that he had known during his 1958 field work saying, "I am sorry that I can't be more helpful; but, as you know, I was not really aware of the biological, as opposed to the sociological, implications of this before I had the good fortune to work with you and Neel."³⁷⁷ Polygyny provided the second parameter of exceptionality—it allowed Apowẽ to sire far more offspring than any other man of his village, and, they scientists would soon learn, of any other Xavante village.

From Apowẽ and Wedezé to the Generalizable Xavante

The preparation of the 1964 Xavante manuscript was a laborious process that

³⁷⁵ Neel, "Natural Selection in Primitive and Civilized Human Populations," 787.

³⁷⁶ Neel et al., "Studies on the Xavante Indians," 94.

³⁷⁷ Maybury-Lewis to Salzano, 11 January 1963, Salzano Correspondence (2 of 10), Neel Papers, APS.

began in the fall of 1962 and dragged on until the eve of the researchers' second collective foray into Central Brazil.³⁷⁸ Their initial findings were finally published in March of 1964 just before they embarked for the Xavante villages of São Marcos and Simões Lopes. Over the course of the preparation of the unusually lengthy first paper, the researchers had to negotiate which claims to make based on their preliminary pilot study.

As they worked through the life story of their first subject and his potential to inform questions of human microevolution, they faced determining whether he was the exception or the rule. "Of particular interest," they wrote, "should it be found to be a general phenomenon is the disproportionate contribution of the village chief (and possibly certain other outstanding members of the village, such as the heads of clans) to the next generation."³⁷⁹ Some of the very aspects that made Apowẽ a compelling focus in the search for drivers of genetic differentiation also raised the possibility that he was an anomaly: "However, we must recognize the possibility that the relatively prolonged and dominant nature of Apewe's chieftaincy has discouraged immigration to the village but encouraged emigration, as suggested from the snatches of history available."³⁸⁰

Including numerous clauses qualifying the generalizable nature of the pilot study did not prevent Neel from elaborating on the potential he saw in Apowẽ's chieftaincy to inform understandings of human selection. As the main identified source of fertility differential, the disproportionate reproduction of a strong leader not only represented a possible source for genetic change, the researchers' prose suggested it also represented

³⁷⁸ See Chapter 2 for a detailed discussion of the process.

³⁷⁹ Neel et al., "Studies on the Xavante Indians," 100. Emphasis added.

³⁸⁰ Ibid., 93.

the natural course for evolution. “The evidence suggest that fertility differentials have far more genetic significance in the Xavantes than is true for civilized man today,” Neel and his colleagues wrote. They went on, “The position of chief or head of clan is not inherited but won on the basis of a combination of attributes (prowess in hunting and war, oratory, skill in wrestling, etc.). The greater fertility of these leaders (assuming this to be a rather general pattern) must have genetic implications. Indeed it may be that the single most dysgenic event in the history of mankind was departure from a pattern of polygamy based on leadership, ability, and initiative.”³⁸¹ Part of what made Apowẽ so compelling was the possibility that he could pass on his exceptional qualities to his many offspring, and in so doing improve his community’s gene pool.

Over the course of the 1964 fieldwork, the team of scientists maintained their interest in polygynous leadership as a major factor shaping the genetic pool. In their 1967 publications they again emphasized that a man’s reproductive privilege as chief was linked to highly valued masculine traits, and was an earned social status: “In general, leaders will be accomplished orators, good hunters and warriors, well versed in the tribal lore. In these small communities, one’s performance under widely varying conditions is well known; it seems reasonable to postulate that the leaders will tend to have intellects and physiques which in that culture are superior.”³⁸² Masculine traits were a driving force for genetic change.

The second season of fieldwork also helped clarify the way the scientific team

³⁸¹ Ibid., 127.

³⁸² Neel and Salzano, “Further Studies on the Xavante Indians: X Some Hypotheses and Generalizations,” 563.

would articulate the interface between politics and genetic change. While Apowẽ and his community focused the attention of the researchers on the importance of political fissures and the founding of new villages following violent confrontation, the other villages they visited drew their attention to the tendency for smaller groups to aggregate. In this second round of interdisciplinary fieldwork collaboration, people from these the other villages allowed the scientists to focus on the fluidity of movement within the greater Xavante population. They researchers explained saying that, “The picture which now emerges is of constant, continuing realignment among groups within the population, of such a degree that...over a period of several generations there should be so much exchange between ‘villages’ that the breeding unit approximates the entire tribe.”³⁸³ Politics and kinship were still central to their understanding of how certain genetic changes or polymorphisms might come to be established within the population. In fact the combination of polygyny and factional politics posed a new mechanism to address the problem of stabilizing selection, which in turn would constitute the theoretical basis for much of their future research on population structure.

Atomic Metaphors: A Genetic Chain Reaction

One afternoon during an advanced genetics seminar at the *Universidade Federal do Rio Grande do Sul*, shortly after their second joint field trip, Salzano and Neel were discussing the population dynamics of the Xavante villages. Drawing a diagram on the

³⁸³ Francisco M. Salzano, James V. Neel, and David Maybury-Lewis, “Further Studies on the Xavante Indians. 1. Demographic Data on Two Additional Villages: Genetic Structure of the Tribe,” *American Journal of Human Genetics* 19, no. 4 (1967): 469. See also Neel, “Between Two Worlds,” 12.

chalkboard to represent the various splits and aggregates that they had documented, Salzano suggested to the group that what they were witnessing was a fission-fusion model. Neel's eyes lit up. He loved the new term, and Salzano's suggested name stuck.³⁸⁴

As mentioned previously, in the 1960s, population geneticists were struggling with the question of how human evolution actually occurred. There was consensus that mutation led to new variants of genes, referred to as polymorphisms, but they were unsure as to how these new polymorphisms could persist for long enough to become widespread. Regardless of whether a polymorphism was beneficial, if too rare in a group, statistically it would be unlikely to persist. In a large gene pool with random mating, each successive generation would have an increasingly small probability of inheriting the new allele (or variant of a gene).

The fission-fusion hypothesis, however, suggested a mechanism for these polymorphisms to become stabilized within a population. Since Xavante communities typically split along kinship lines due to the confluence of political competition and the moiety system of patrilineal descent, it was possible for a new village to include many closely related individuals. This increased the probability of a polymorphism becoming prevalent in the new community. In combination with the disproportionate genetic contribution of certain powerful male figures, the geneticists argued, this meant it was quite possible to attain the necessary frequency for new alleles to be maintained.

Salzano and Neel described these findings in provocative atomic metaphors, writing, "...the pattern of fission-fusion would seem to provide the basis for what might

³⁸⁴ Salzano, interview, 17 August 2015.

be termed a genetic chain reaction, as in successive villages the critical frequency is exceeded by the addition of groups of people from villages in which the polymorphism is already established.”³⁸⁵ The fissions provided the possibility for the stabilization of new gene frequencies. Meanwhile the wider practices of mobility between different communities – the fusions – ensured that over longer time scales selection would lead to the expansion of beneficial genetic changes. The model fit with key contemporary thinking about the conditions necessary for rapid and effective evolution.³⁸⁶ Using the Xavante to stand in for pre-historic humans, the geneticists offered an explanation of how human variability could have developed over time.

But for the case of the Xavante, the Cold War and the local political and economic moment offered more than just metaphors. The geneticists set out on their second joint field excursion only days after the Military-Civilian Coup of 1964. As the military assumed control of the country’s institutions, the political instability in the country’s urban centers threatened to make the geneticists’ work impossible. The slower, but even more pernicious economic-political trend was also underway in Central Brazil. The state-led process of opening up the interior placed unquestionable pressure on Indigenous communities. Even as Neel, Salzano, and their colleagues described masculine vitality and health, Xavante communities were under extreme strain.

The geneticists were careful to specify that they considered the group not

³⁸⁵ Neel and Salzano, “Further Studies on the Xavante Indians: X Some Hypotheses and Generalizations,” 557.

³⁸⁶ Ibid. Specifically, Neel and Salzano offered support for Sewall Wright’s work with this theory.

“untouched.”³⁸⁷ They also recognized that the local context and historical moment might have influenced the fission-fusion pattern that they observed. “*We do not know to what extent this is a recent phenomenon, in response to the increasing contacts with neo-Brazilians,*” they wrote, “Further work on tribes even less disturbed in their social structure is obviously necessary. *In general, we believe that recent events have not created but only increased the internal mobility of the Xavantes.* If this is correct, then it is clear that one may derive a very biased picture of the tribal dynamics of this (and presumably any other) Indian tribe during the course of a brief contact.”³⁸⁸ But despite these careful disclaimers, the overall representation of the population studied tacks to the side of optimism regarding how well observations of a living group could stand for primordial humanity.

In large part the prevalence of this tone throughout the series of publications was due to Neel’s stylistic flair. While Salzano and some of their other collaborators drafted cautious phrasings, Neel was more liberal in his speculations and links to questions about human futures. As other scholars have suggested, Neel’s quest to find “tribes even less disturbed in their social structure” in his subsequent field research would lead him and his collaborators deep into the Brazilian Amazon, and then on to the Venezuelan side of Brazil’s northern border.³⁸⁹

But the geneticists’ estimations of the health of the communities they studied seems to have been strongly influenced by the impression of strength and vitality that

³⁸⁷ Salzano, Neel, and Maybury-Lewis, “Further Studies on the Xavante Indians. 1. Demographic Data,” 464.

³⁸⁸ *Ibid.*, 469. Emphasis in original.

³⁸⁹ Santos, Lindee, and Souza, “Varieties of the Primitive,” 726.

they took from the Xavante men. While they identified very high prevalence of antibodies to a wide variety of pathogens, they interpreted this trend to suggest, “that a high level of challenge of immunological competence ... has been a feature of human existence for a long time.”³⁹⁰ Rather than interpreting the high antibody count as a relatively recent phenomenon linked to increased disease burden and high likelihood of mortality for those with weakened immune systems, the findings puzzled them. The antibody data “...only intensifies the mystery of the relative absence of aged in this population,” they wrote.³⁹¹

Later studies would suggest that both the high antibody-counts and high population attrition rates that the geneticists’ research documented were results of the destructive process of increased contact with Brazilian society. The pressures of developmentalism accelerated the disintegration and reintegration of Xavante villages that the scientists observed. Competition over material goods led to violent conflict within and between Xavante communities, and epidemics of disease accounted for the high infant mortality rate and relative absence of elders that the 1962 studies documented.³⁹²

Conclusion: The Enduring Legacy of Apowẽ and Fission-Fusion

The Xavante studies and the resulting fission-fusion hypothesis had a lasting

³⁹⁰ Neel and Salzano, “Further Studies on the Xavante Indians: X Some Hypotheses and Generalizations,” 568.

³⁹¹ *Ibid.*, 569.

³⁹² Coimbra et al., *The Xavante in Transition*, 82, 130; Maybury-Lewis, *Savage and the Innocent*, 177; Garfield, *Indigenous Struggle*, 45–65.

influence both on the careers of the geneticists who planned and led this work, and on the broader field of human population genetics. In the short term, the fieldwork experience established a model for future research. The data collected provided a foundation for diachronic health and demography research as well as quantitative comparisons with other human populations. In the long term and at the theoretical level, this initial work defined the primary area of scholarship to which Salzano and his future students would contribute. Many of the same motivations that shaped the Xavante research agenda would animate the collaborations that Salzano and Neel carried out over the course of the next decade, which took them to visit dozens of Indigenous communities throughout Amazonia. Whether working with the Kayapó, the Ticuna, or the Yanomami, Salzano-Neel expeditions continued to inquire into polygyny and socio-political organization as factors underlying genetic microevolution and the maintenance of human variation.

Data from the Xavante fieldwork would be mobilized repeatedly, both in the study of the Xavante, and in comparative work on other Indigenous populations. As I discuss in Chapter 5, the physical examinations, genealogies, and biomedical data from Wedezé became a baseline for later studies on the health and demography of *Terra Indígena Pimentel Barbosa*.³⁹³ For comparisons with other groups, it was the data from anthropometric measurements, one of the oldest techniques employed by the group, which were widely employed for future studies.³⁹⁴ Initial findings had characterized the

³⁹³ E.g. Nancy M. Flowers, “Forager Farmers”; Coimbra et al., *The Xavante in Transition*. See Chapter 5 for more details.

³⁹⁴ A. J. Marcellino, F. J. da Rocha, and F. M. Salzano, “Size and shape differences among six South American Indian tribes,” *Annals of Human Biology*, 5 (1978): 69–74; A. E. Stark, F. M. Salzano, and F. J. da Rocha, “Marital correlation for anthropometric characteristics in Brazilian Indians,” *Annals of Human*

Xavante as morphologically differentiated. After building a database of measurements from ten other ethnic groups, Salzano and his colleagues Fernando da Rocha and Walter Neves corroborated the distinctiveness of the Xavante in the mid-1980s through principle-component analysis.³⁹⁵ These anthropometric measurements and standardized photos that accompanied also provided the data necessary for genomic analysis of genetically based microdifferentiation of skull morphology as recently as 2012.³⁹⁶

Both the fission-fusion hypothesis and Apowê's prominence as an exemplary subject weathered the transition from genetic analysis of proteins as expressed in the blood to the direct study of DNA. One recent study documented the importance of cultural differences leading to rapid evolution both at the genetic and morphological level. The 2012 paper published by Salzano and collaborators argued that, "Sexual selection could be the culture-generated force that would explain the results and cause of such divergence. For example, the reproductive success of some of the Xavante chiefs is well documented on the ethnographic missions of the 1970s. When familiar data were collected on the São Domingo village, 25% of the inhabitants were sons of the Xavante chief Apoena, who had five wives and a vast array of alliances."³⁹⁷ Based on the both the anthropometric measurements and frozen blood samples of the 1962 field research, new DNA-based research corroborated Salzano and Neel's early findings of the

Biology 17 (1990):417–22; James V. Neel and R. H. Ward. "Genetic Structure of a Tribal Population, Yanomama Indians .6. Analysis by F-Statistics (Including a comparison with Makiritare and Xavante)," *Genetics* 72, no. 4 (1972): 639-666.

³⁹⁵ Walter A. Neves, Francisco M. Salzano, and Fernando J. Da Rocha, "Principal-Components Analysis of Brazilian Indian Anthropometric Data," *American Journal of Physical Anthropology* 67, no. 1 (1985): 13–17.

³⁹⁶ Hünemeier et al., "Cultural Diversification Promotes Rapid Phenotypic Evolution in Xavante Indians."

³⁹⁷ *Ibid.*, 76.

morphological differentiation of the Xavante. Apowẽ persisted into the twenty-first century as a compelling example of how culture could translate to human genetic evolution.

While Apowẽ's participation in the research was not the only factor leading to the geneticists' development of the fission-fusion concept, his reputation, life-story, and political position served to focus their attention on an exceptional case. The Cold War context was permeated by violence. Imaginaries of global warfare saturated the popular culture of the scientists who set out to study and make sense of Apowẽ and his community. Economic and political instability in Brazil cultured the conditions for military rule. On the frontier of western expansionism *fazendeiros* hungry for land and waves of disease perpetrated both physical and structural violence on Xavante communities. It was in this context that geneticists set their sights on the political violence of Xavante society and the fierceness of the Xavante warrior to make sense of human evolution. Their interests, and particularly those articulated by James Neel, found their match in the reputation and political action of Apowẽ, "...perhaps the best-known Shavante in Brazil."

Figure 3a

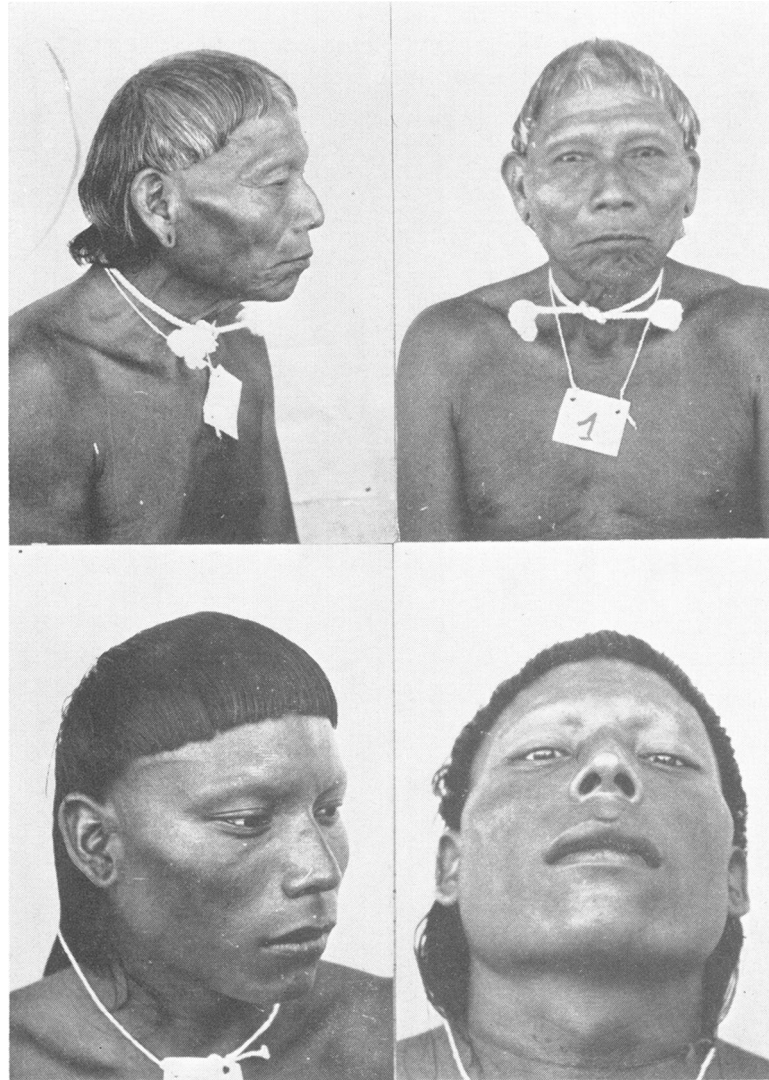


FIG. 1. Two male Xavantes, the chief and one of his sons, illustrating four of the five types of standard photograph obtained of each subject.

Figure 3: This series of images illustrated four of the five positions for anthropometric photographs, showing Apowē and his son Barodi. From: Neel et al., "Studies on the Xavante," 66. Copyright American Journal of Human Genetics.

Chapter Four

Militantes: Studying the Indigenous under Military Rule

Introduction

On the first of February of 1982, socio-linguistic anthropologist Laura R. Graham appeared at a regional FUNAI office to contest allegations of inciting unrest in Xavante territory. A United States citizen, she was in Brazil for a year to complete research for her master's thesis from the University of Texas-Austin under the supervision of anthropologists at the *Universidade de São Paulo*. But shortly after arriving in T.I. Pimentel Barbosa, her research authorization was temporarily suspended and she was expelled from the territory. She arrived at the *Ajudância Autônoma de Barra do Garças* (Autonomous Adjutancy of Barra do Garças, AJABAG) to respond to a local FUNAI agent's accusations.³⁹⁸ Among other issues, the FUNAI employees accused Graham of inciting the Xavante to evict a neighboring *fazendiero*, of provoking unrest and discord between the Xavante community and the local FUNAI post, of threatening to malign the government administration on the international stage, and of convincing the villagers that she, too, was Xavante.

The central offices of FUNAI in Brasília collected a dossier of information on the case over five weeks. They also forwarded copies to the state surveillance system, the *Assessoria de Segurança e Informações* (ASI, Security and Information Committee), an

³⁹⁸ Laura Graham to Ajudância Autônoma de Barra do Garças (Autonomous Adjutancy of Barra do Garças, hereafter AJABAG), "Declaração," 1 February 1982, Doc: PSS_0395, Fundo: Assessoria de Segurança e Informações da Fundação Nacional do Índio (hereafter ASI/FUNAI), Arquivo Nacional, Brasília.

arm of the military regime that reached into nearly every public institution in the country. The records included short wave radio messages, letters, and statements from various FUNAI employees, as well as from the anthropologist herself. “According to the point of view of the elders and the leaders of the village,” Graham stated in her declaration to the authorities, “there was no problem with my presence in the village, and they were very confused. I also believe there is no dispute between factions because of me, no politics among the Indians. I do not think it is fair to end my research due to the disrespect of the *chefe do posto* of T.I. Pimentel Barbosa towards me.”³⁹⁹ Her dossier also included the eventual decision, on 1 March 1982, by FUNAI’s president in Brasília, that Graham could return to the village to continue her work.

Taking place in the final years of the military dictatorship in Brazil, this case of the suspension and reinstatement of Graham’s state authorization to research in Indigenous territory raises a number of questions about the relationship between social science research, FUNAI administrators, and the broader politics and policies of state administration of Indigenous affairs. First, the accusations against her highlight the tense dynamic between local, national, and international actors. Foreign anthropologists were potentially subversive forces, surveilled for their activity in territories that were simultaneously Native and federal. And yet indictments from local government agents were not always authoritative; they were mediated by oversight centralized in the national offices of government institutions. Secondly, Graham’s case shows that a variety of different interests were at play as FUNAI debated the benefits and risks of allowing

³⁹⁹ Ibid.

researchers, foreign or national, to work in Indigenous territory. Since the institution of military rule in 1964, the government had accelerated aggressive development policies that brought settlers into conflict with the Indigenous peoples whose lands they sought to claim. However, in a very literal sense, this conflict was an economic liability, and the government required information to make legible and governable the peoples targeted for colonization. Facing the threat of international denunciations of state policy, which became increasingly frequent from the end of the 1960s on, a growing network of NGOs and many academics pressured the Brazilian government to substantiate its claims to transparency and protection of Indigenous people by allowing researchers and journalists into the field. Finally, the third tension this chapter explores is between the Indigenous leaders, Brazilian anthropologists, and international scholars who all engaged the Brazilian state, both collaborating with and critiquing indigenist policies and practices. In the process, I argue, they reformed discourses and practices of anthropology.

Through the cases of researchers who have worked in Xavante territory, this chapter examines anthropological engagement under military rule. The military dictatorship was a period of contradictions. Although anthropology had a long history in Brazil prior to the military-civilian coup of 1964, the educational reform and expansion implemented by the military government led to the institutionalization of doctoral programs, the growth of undergraduate education, and increases in research funding. Many anthropologists were employed as expert social scientists by the state or international NGOs funding development projects. Simultaneously, in the face of the devastating impact of developmentalism on many of the communities they studied, anthropologists formed networks of opposition to publicize widespread abuses and urge

action against the policies and projects of the military regime. As they nurtured collaboration with international colleagues and engaged an emerging sector of human rights NGOs, Brazilian anthropologists focused international criticism on the Brazilian state. The contradictions of the period left an indelible mark on the professionalization and self-imaginaries of ethnologists who worked in Brazil, including—and in part led by—the *xavantólogos*. It was during this time that many anthropologists began to refer to themselves as *militantes*, “militants” or “activists.” They articulated a sense of purpose that was both personally and professionally tied to the imperative of advocacy.⁴⁰⁰

Focusing on the period from 1968 to 1988, this chapter offers a broad view of the development of Brazilian anthropology under military rule through the lens of the work of the *xavantólogos*. As previous chapters have shown, the academic production of scholars who researched in Xavante territories informed and reflected large-scale trends for intellectual developments such as structuralism, human ecology, and human genetics. The same can be said for forms of social engagement.

When, in 1972, Pia and David Maybury-Lewis founded the international NGO Cultural Survival, it was in response to the struggles facing the Xavante communities they had visited, as well as other Indigenous communities where Maybury-Lewis’ graduate students were busily conducting fieldwork. The same year, while finalizing her

⁴⁰⁰ In this sense, the Brazilian case offers an interesting complement to the growing literature on the human sciences in the postwar and Cold War periods. The historiography of anthropology in the United States has strongly emphasized the relationship of anthropological work to intelligence interests. See Price, *Cold War Anthropology*; Wax, ed. *Anthropology at the Dawn of the Cold War*. For an overview of this period in the US setting including attention to the growth of the field and changes in government funding, see Susan Lindee and Joanna Radin, “Patrons of the Human Experience: A History of the Wenner-Gren Foundation for Anthropological Research, 1941–2016,” *Current Anthropology* 57, no. S14 (2016): S218–301, doi:10.1086/687926.

undergraduate degree at the *Universidade de São Paulo* (USP), Aracy Lopes da Silva conducted her first season of fieldwork in Xavante T.I.s São Marcos and Sangradouro. She would continue on to doctoral research, but also combine her academic pursuits with a temporary position as a consultant to the government on a development project, and the creation of two Brazilian NGOs dedicated to advocacy. Finally, to close the chapter, I return to Laura Graham's fieldwork, which began in 1981 during the *abertura* (opening), or the loosening of the military regime's repression. Graham's case provides insight into the engagement of both her Brazilian and Xavante hosts in advocating for her. It also emphasizes the heterogeneity of perspectives within government institutions regarding researchers who wished to venture into Indigenous territory.

The historiography of the military dictatorship in Brazil is rapidly growing, fueled by the availability of new archives and the publication of the comprehensive investigation of its abuses conducted by the *Comissão Nacional da Verdade* (National Truth Commission).⁴⁰¹ Among other questions, historians have been centrally concerned with the relationship between civil society and military rule. The regime has often been

⁴⁰¹ The *Comissão Nacional da Verdade* (CNV) finalized its task with the publication of a three-volume report in December 2014. See: Brasil, "Relatório da Comissão Nacional da Verdade," (Brasília: CNV, 2014), available at <http://www.cnv.gov.br>. On the opening of the archive of the Assessoria de Segurança e Informações (ASI, Security and Information Committee) see Vivian Ishaq and Pablo E. Franco, "Os acervos dos órgãos federais de segurança e informações do regime militar no Arquivo Nacional," *Acervo* 21, no. 2 (2008): 29–42. In addition to the final report of the CNV, a number of new historical accounts of the military regime were published in 2014 for the fiftieth anniversary of the military-civilian coup. See: Rodrigo Patto Sá Motta, *As universidades e o regime militar: cultura política brasileira e modernização autoritária* (Rio de Janeiro: Zahar, 2014); M. Napolitano, *1964 – História do regime militar brasileiro* (São Paulo: Contexto, 2014); Daniel Aarão Reis, *Ditadura e democracia no Brasil* (Rio de Janeiro: Zahar, 2014). Recent historiography from the United States includes the Gilbert M. Joseph and Daniela Spenser, eds., *In from the Cold: Latin America's New Encounter with the Cold War* (Durham: Duke University Press, 2007); Victoria Langland, *Speaking of Flowers: Student Movements and the Making and Remembering of 1968 in Military Brazil* (Durham: Duke University Press, 2013). On the role of US academia in opposing Brazil's military regime, see James N. Green, *We Cannot Remain Silent: Opposition to the Brazilian Military Dictatorship in the United States* (Durham: Duke University Press, 2010).

characterized as a bureaucratic-authoritarian regime that, in the words of Jerry Dávila, “ostensibly gave autonomy to technocrats to restore political and economic order so that private enterprise could again invest safely and productively.”⁴⁰² Much of this scholarship attends to the links between bourgeois civil society and business interests that were at the heart of the support that the regime received, support that allowed continued military rule for twenty-one years.⁴⁰³ This chapter builds on these approaches to consider how anthropologists and their colleagues and interlocutors from abroad—most of them part of the social elite—engaged with and challenged the military regime on issues relating to Indigenous rights and the governance of Native territory. In doing so, I follow historian Rodrigo Patto Sá Motta to argue that without discounting the abuses and excesses of the military regime, conciliatory politics played an important role in the implementation of reforms, whether to the educational system or the administration of Indigenous territory.⁴⁰⁴ This argument reveals the inherent heterogeneity and contradictions within authoritarian rule in Brazil: anthropologists were both pragmatic and *militante*; they worked with the government for self-interested purposes, including to obtain legal authorization for their work; they consulted and provided information to FUNAI in practical attempts to advocate for their interlocutors within the space allowed by the

⁴⁰² Jerry Dávila, *Dictatorship in South America* (West Sussex, UK: Wiley-Blackwell, 2013), 7.

⁴⁰³ For a discussion of the wide literature that looks at business interests, civil society, and support for the Brazilian military coup, see Marcelo Ridenti, “The Debate over Military (or Civilian-Military?) Dictatorship in Brazil in Historiographical Context,” *Bulletin of Latin American Research* (June 2016): 1–10, doi:10.1111/blar.12519. One of the earliest works to propose this attention to the social and class issues underlying support for the military regime was René Armand Dreifuss, *1964: a conquista do Estado* (Petrópolis: Vozes, 1981).

⁴⁰⁴ Motta, *As universidades e o regime militar*.

system; and they organized against the regime, collaborating directly with their informants and drawing international attention to the abuses they witnessed.

This chapter does not endeavor to characterize the success or failings of anthropological activism, or evaluate the value of different forms of engagement that emerged at the international and national levels. Other work has examined, to great effect, the complications and intricacies of claims made by Indigenous people and advocates who employ legal notions of human rights or cultural rights in their work to attain Indigenous rights,⁴⁰⁵ advocacy projects gone awry,⁴⁰⁶ or the difficult relationship between anthropology and NGOs or development projects.⁴⁰⁷ Rather, this chapter seeks to show on the one hand, how discourses of activism and articulations of ethical imperatives emerged during this period in response to the threats to Indigenous existence deeply marking the field of anthropology, and on the other how the institutionalization of the social sciences under military regime was imbricated in the messy politics of state expansionism.

⁴⁰⁵ Engle, *The Elusive Promise of Indigenous Development*; Ned Blackhawk, "Julian Steward and the Politics of Representation: A Critique of Anthropologist Julian Steward's Ethnographic Portrayals of the American Indians of the Great Basin," *American Indian Culture and Research Journal* 21 no. 2 (1997): 61–81. On the unintended consequence of doing "good" in humanitarian work, see David Kennedy, *Dark Sides of Virtue: Reassessing International Humanitarianism* (Princeton: Princeton University Press, 2004). Miriam Ticktin's study on humanitarian aid and migration in France makes clear how regimes intended to help can lead to unexpected devaluing of certain lives: Miriam I. Ticktin, *Casualties of Care: Immigration and the Politics of Humanitarianism in France* (Berkeley: University of California Press, 2011).

⁴⁰⁶ Blackhawk, "Julian Steward and the Politics of Representation."

⁴⁰⁷ William Fisher, "Doing Good? The Politics and Anti-Politics of NGO Practices" *Annual Review of Anthropology* 26 (1997): 439–464; Timothy Mitchell, *Rule of Experts: Egypt, Techno-Politics, Modernity* (Berkeley: University of California Press, 2002), 209–243. For an analysis of the shared roots of development paradigms and the field of anthropology itself, see James Ferguson, "Anthropology and Its Evil Twin: 'Development' in the Constitution of a Discipline," in *International Development and the Social Sciences*, ed. Frederick Cooper and Randall Packard (Berkeley: University of California Press, 1997), 150–175. For a response that argues that development anthropology has its own particular moral discourse, see David D. Gow, "Anthropology and development: Evil twin or moral narrative?" *Human Organization* 61 no. 4 (2002), 299–313.

Indigenous Peoples and Anthropologists under Military Rule

On 27 October 1975, General Ismarth de Araújo Oliveira, the president of FUNAI, opened a three-day meeting with a group of prominent anthropologists and *indigenistas*. The *Conselho Indigenista da FUNAI* (Indigenist Council) was made up of a panel of experts including, for the first time in a number of years, academic ethnologists from federal universities. The group also included government employees with extensive experience working with Indigenous groups. The general presided over the meeting.

As the head of the agency charged with protecting the interests of Indigenous peoples, Ismarth de Araújo Oliveira re-convened an expert advisory panel that had met intermittently since FUNAI replaced the SPI in December of 1967. The grainy audio recording of this meeting is peppered with the general's grandiose statements regarding the prioritization of Indigenous welfare. According to him, it was through collaboration with academic researchers that the government organization would better be able to meet the needs of the communities it was meant to serve: "We are not infallible. And when I say 'we' I am referring to the administration of FUNAI," Ismarth de Araújo Oliveira pronounced in his closing comments for the meeting. "FUNAI isolated itself in the past, wanting to complete this immense task alone. This isolation, to which FUNAI subjected itself, represents a waste of time in relation to these Indigenous communities. ... But alone, with a relatively small budget, and now that FUNAI's budget has been adjusted again, it is practically impossible to reach all the Indigenous communities in the country. I consider this first experience of a group of anthropologists participating in a meeting of

the *Conselho Indigenista* of FUNAI to be perfectly valid.”⁴⁰⁸ The cure to FUNAI’s isolation, as Ismarth de Araújo Oliveira had repeatedly stated in agreement with the anthropologists in attendance, was the consolidation of existing knowledge, new collaborations with academic ethnologists, and the training of more scholars dedicated to the study of Indigenous groups. He believed these links between anthropology and the military regime to be essential for effective governance. The academics present at the meeting, however, had many reasons to suspect the motivations of the federal agency in turning to them for help.

When President João Goulart was deposed in 1964, industrialists and large landowners supported the military coup due to concerns about the economic situation and Goulart’s left-leaning politics.⁴⁰⁹ The new military regime prioritized economic reform and national security. In addition to efforts to modernize education and thus increase the technical capacity of Brazilian labor pools in urban areas, the administration pursued economic goals aimed at expanding access to external markets, streamlining extractive practices, and investing in rural infrastructure to facilitate these outwardly focused policies. Furthermore, the new regime considered settling and developing the vast interior of the country, and especially securing border regions, critical for national security.⁴¹⁰

⁴⁰⁸ Conselho Indigenista, “Sessão 1 do Conselho Indigenista,” 27–29 October 1975, AVESON 222, compact disc, SEDOC-MI/FUNAI.

⁴⁰⁹ Thomas Skidmore’s early history of the Brazilian military dictatorship provides an overview of the coup and the development of military rule with particular attention to political and economic implications: Thomas E. Skidmore, *The Politics of Military Rule in Brazil, 1964–1985* (Oxford: Oxford University Press, 1990).

⁴¹⁰ On the intersection of national security and imaginaries of Indigenous people, see Tracy Devine Guzmán, “Our Indians in Our America: Anti-Imperialist Imperialism and the Construction of Brazilian Modernity,” *Latin American Research Review* 45, no. 3 (2010): 35–62.

The Amazon region thus became a major target for “modernization” of agricultural practices, building on the discursive and institutional foundations Presidents Getúlio Vargas and Eurico Gaspar Dutra laid in the 1940s.⁴¹¹ These policies continued a long history of westward expansionism, but with increased scale and speed.⁴¹²

The economic policies of the military regime had major impacts on Indigenous people. In 1966, for example, an extensive fiscal-incentive program implemented by the newly established *Superintendência do Desenvolvimento da Amazônia* (Superintendency for the Development of the Amazon, SUDAM) offered major tax incentives for agricultural investment.⁴¹³ These policies applied to the area known as *Amazônia Legal*, a politically defined conglomerate of nine states that reached substantially beyond the ecological borders of the Amazon basin. The area included the northern portion of Mato Grosso; in the four years after its establishment, SUDAM would approve sixty-six cattle ranches in the counties of Luciara and Barra do Garças, where Xavante territory was located.⁴¹⁴ Furthering the government-sponsored project of colonization of the interior of the country, in 1970, President Emílio Médici instituted the *Plano de Integração*

⁴¹¹ Garfield, *Indigenous Struggle*, 137–140.

⁴¹² There is a rich literature critiquing the developmentalist program, much of which grew out of Arturo Escobar’s *Encountering Development: The Making and Unmaking of the Third World* (Princeton: Princeton University Press, 1995). Escobar described the development paradigm as a new form of coloniality, perpetuating and exacerbating the very poverty and exploitation development was ostensibly seeking to correct. More recent scholarship has challenged earlier portrayals of developmentalism to include more diversified interests and actors, including attention to how local populations, NGOs, government agencies, and industry all intersect in the context of development projects. For two particularly interesting ethnographic inquiries, see Greene, *Customizing Indigeneity* and Kiran Asher, *Black and Green: Afro-Colombians, Development, and Nature in the Pacific Lowlands* (Durham: Duke University Press, 2009).

⁴¹³ For an overview of the environmental impact of developmentalism under the policies of the military dictatorship, see Susanna Hecht and Alexander Cockburn, *The Fate of the Forest: Developers, Destroyers and Defenders of the Amazon* (Chicago: University Of Chicago Press, 2010[1990]), 104–141.

⁴¹⁴ Seth Garfield, “Where the Earth Touches the Sky: The Xavante Indians’ Struggle for Land in Brazil, 1951–1979,” *Hispanic American Historical Review* 80, no. 3 (2000): 543, doi:10.1215/00182168-80-3-537.

Nacional (National Integration Plan), which made provisions to fund a series of major infrastructure projects. Among the most dangerous to Indigenous populations were the construction of the two major highways crossing the Amazon region. These megaprojects intended to relocate five million people from the drought-plagued Northeast throughout the region.⁴¹⁵ These road-building projects had dire effects. As Alcida Ramos has noted, diseases decimated Indigenous communities “pacified” through these projects, with many communities losing between one fifth and half of their members to infectious disease within one to two years of construction.⁴¹⁶

While the “Brazilian miracle” of an average growth of 11.1% from 1968 to 1973 fueled industry excitement over the dictatorship-led investments,⁴¹⁷ the government-led projects also sparked resistance from Indigenous, religious, and academic opponents.⁴¹⁸ As scholarship on the Brazilian Military Dictatorship has often emphasized, the Brazilian regime went to some lengths of maintain premises of legality to its rule—what Thomas Skidmore called “legal acrobatics” for legitimacy.⁴¹⁹ Especially during the early years of military rule, the government was concerned with maintaining the premise that the regime was temporary and for the purposes of cleaning house. Even when the

⁴¹⁵ Garfield, *Indigenous Struggle*, 156–157; On the Trans-Amazon Highway, see: Velho, *Capitalismo Autoritário e Campesinato*, 209–223; Shelton H. Davis, *Victims of the Miracle: Development and the Indians of Brazil* (Cambridge: Cambridge University Press, 1977), 62–76.

⁴¹⁶ Alcida Rita Ramos, *Indigenism: Ethnic Politics In Brazil* (Madison: University of Wisconsin Press, 1998), 201–204. Different Xavante communities also suffered terrible death rates due to infectious disease during this period. See Garfield, *Indigenous Struggle*, 118. Also refer to Chapter 1 for discussions of Flowers’ findings on infant mortality, and Chapter 3 regarding antibodies showing evidence of prior infection with infectious disease in Pimentel Barbosa.

⁴¹⁷ Brasil, “Relatório da Comissão Nacional da Verdade,” 3.

⁴¹⁸ The Conselho Indigenista Missionario (Indigenist Missionary Council, CIMI) was established in 1972 and quickly became one of the most important local organizations for documenting and speaking out against abuses. For more on CIMI, see Garfield, *Indigenous Struggle*, 178–179.

⁴¹⁹ Thomas E. Skidmore, *Brazil: Five Centuries of Change* (New York: Oxford University Press, 2009), 156.

administration passed laws unilaterally, it still made cosmetic provisions for the end of the regime and return to democracy.⁴²⁰

Reform in the government agency dedicated to Indigenous affairs seemed, to the legalistic military administrators, a possible site to intervene and demonstrate transparency and reform. In 1967, administrators commissioned an investigation into reported abuses of the SPI in Indigenous territory. The government attorney assigned to the case, Jader de Figueiredo Correia, issued a chilling report, thousands of pages long, as a result of the investigation in March of 1968. The *Relatório Figueiredo*, as it came to be known, included reports of SPI employees participating in massacres, poisonings, rape, and enslavement of Native people.⁴²¹ It documented widespread corruption in the SPI, and resulted in 134 employees being charged with crimes and dozens fired.⁴²² As Garfield has pointed out, the military's decision to make the findings of the report public is puzzling. While the government bargained that the report would prove "a perfect morality play to legitimize authoritarian rule in spotlighting corrosion of the public sector under populists,"⁴²³ and an opportunity to prove the humanitarian and transparent nature of military rule, in fact the strategy backfired. Both on the national and international stage, journalists, social scientists, and politicians accused the military regime of

⁴²⁰ Skidmore, *Politics of Military Rule*, 56–57. Dávila also presents the courts' close collaboration and legalistic nature of the dictatorship in Brazil as something that set the regime apart from those in Argentina and Chile. See Dávila, *Dictatorship in South America*, 5.

⁴²¹ Davis, *Victims of the Miracle*, 10–13. The *Relatório Figueiredo* was somewhat mysteriously lost shortly after its presentation to the public, and so from the late 1960s until 2012 historians relied on journalistic accounts of the report. In 2012, staff at the *Museu do Índio* uncovered a copy of twenty-nine of the thirty volumes that made up the 7000 page report. The documents had been sent from FUNAI central offices in an unprocessed series of boxes of documents. All recovered volumes are now available:

https://archive.org/details/RelatorioFigueiredo_01.

⁴²² Davis, *Victims of the Miracle*, 11.

⁴²³ Garfield, *Indigenous Struggle*, 143.

atrocities against Native groups.⁴²⁴ While national dissent was stifled under increasing censure, a series of publications sharply criticized the government.⁴²⁵ During the years directly following the *Relatório Figueiredo* and the tightening of military rule, a host of international NGOs were established to advocate for Indigenous rights, and the political situation in Brazil was a key contributor to this international concern.

One of the most widely cited accounts of developmentalist ravages in Indigenous communities in Brazil was Sheldon Davis' *Victims of the Miracle*. The young North American anthropologist had recently completed fieldwork in Guatemala, and was lecturing at the *Museu Nacional* in Rio when colleagues unable to publically denounce the regime enlisted his help. Davis reflected on his involvement, which began in 1970, writing, "The general political climate in Brazil at this time made it difficult for my Brazilian colleagues to take any organized action against Indian policy along the new Amazon roads. My own position as a foreign anthropologist, however, was different."⁴²⁶ In addition to studying how economic development policies unleashed a "massive amount of disease, death, and human suffering" on Indigenous groups, Davis organized and advocated for international pressure on the Brazilian government to address these abuses. FUNAI and the surveillance apparatus were put in a kind of double bind: they could not completely disallow foreign anthropologists and journalists without raising

⁴²⁴ Ibid. See also, Jonathan W. Warren, *Racial Revolutions: Antiracism and Indian Resurgence in Brazil* (Durham: Duke University Press, 2001), 83–85.

⁴²⁵ Robin Hanbury-Tenison, *A Question of Survival for the Indians of Brazil* (London: Angus and Robertson, 1973); Edwin Brooks, René Fuerst, John Hemming, and Francis Huxley, *Tribes of the Amazon Basin in Brazil: Report by the Aborigines Protection Society* (London: C. Knight, 1973); Robert J. A. Goodland and Howard Samuel Irwin, *Amazon Jungle: Green Hell to Red Desert?: An Ecological Discussion of the Environmental Impact of the Highway Construction Program in the Amazon Basin* (Amsterdam: Elsevier Scientific Publishing, 1975).

⁴²⁶ Davis, *Victims of the Miracle*, xiv.

accusations of hiding abuses, but they also risked criticism in allowing access. As the military government shifted towards a gradual easing of repression, FUNAI sought to engage with academic anthropologists to assuage critics and aid the government in the considerable challenge of governing at the frontier.

It was in this context that in 1974, coterminous with the presidency of Ernesto Geisel, General Ismarth de Araújo Oliveira assumed the presidency of FUNAI. He is widely perceived by scholars to have been a moderate voice within the dictatorship. Under his direction FUNAI accelerated its program to “integrate” Indigenous groups through development projects and programs to make *aldeias* (villages) into sites of agricultural production. However, the same period saw a large increase in the demarcation of *Terras Indígenas*. Alcida Ramos has referred to his leadership from 1975–1976 at FUNAI as “a somewhat ‘enlightened’ phase.”⁴²⁷ Daniel Gross highlighted his partially successful attempts to speed the process of demarcation.⁴²⁸ In a 2014 interview, anthropologist Regina Müller noted a certain “openness” during Ismarth de Araújo Oliveira’s tenure, as well as the success that USP professor Lux Vidal had in engaging FUNAI and pressuring for support of projects implemented by her former students; this tense but relatively cordial arrangement lasted only until the change of leadership in March of 1979.⁴²⁹

Under military rule, processes of land demarcation that had previously languished, stalled by local political resistance, were centralized under the exclusive

⁴²⁷ Ramos, *Indigenism*, 214.

⁴²⁸ Daniel R. Gross, “The Indians and the Brazilian Frontier,” *Journal of International Affairs* 36, no. 1 (1982): 8.

⁴²⁹ Müller, interview.

purview of the federal government. As a result, perhaps counter-intuitively and even as land invasions drastically increased with infrastructure projects, FUNAI was far more effective than the SPI at demarcating land.⁴³⁰ It was impossible to complete these tasks, however, without basic knowledge about the communities under study. As of 1975 FUNAI still struggled even to acquire basic data regarding the number of inhabitants and the location of Indigenous villages.

Given this context, it seems more plausible that General Ismarth de Araújo Oliveira's assessment that the government needed to collaborate with ethnologists was genuine. During the *Conselho* meeting, one participant remarked that, "there is a major lack of information and documentation of legal aspects regarding indigenous land" and that anthropologists should be required to provide information to FUNAI in the form of field reports and final publications.⁴³¹ The president responded that, "the organization of this documentation will be one of the greatest weapons that FUNAI has for the defense of Indigenous land."⁴³² Despite policies that consistently endangered Indigenous welfare, the regime was heterogeneous. Many public employees and at times even FUNAI's military administration took their charge to protect indigenous welfare seriously.⁴³³

⁴³⁰ Garfield, "Where the Earth Touches the Sky," 543–545. On the dual legacy of the military regime, see also Stephan Schwartzman, Ana Valéria Araújo, and Paulo Pankararú, "Brazil: The Legal Battle over Indigenous Land Rights," *NACLA: Report on the Americas* 29, no. 5 (1996): 37–38.

⁴³¹ Conselho Indigenista, "Sessão 1 do Conselho Indigenista," AVESON 222 F lado B, compact disk, SEDOC-MI/FUNAI.

⁴³² *Ibid.*

⁴³³ One particularly compelling example is that of Cláudio dos Santos Romero, an SPI and FUNAI employee who was particularly successful at demarcating Xavante land, and who continues to be very popular with both anthropologists and Xavante community members. On Romero's work and Xavante reactions to it in the 1970s, see Garfield, *Indigenous Struggle*, 171–172. Romero, in his reflections on the work of anthropologists from the academy, promoted the importance of research, but also dismissed anthropologists who he saw as more interested in acquiring data than supporting Xavante resistance. Cláudio dos Santos Romero, interview with Rosanna Dent, 19 May 2014, Brasília.

Likewise, despite their resistance to government policies, anthropologists who engaged the bureaucratic system believed that they had the potential to influence government decisions.

A small example of change over time with respect to FUNAI's administration can be seen with the example of the Swiss ethnologist-journalist René Fuerst. Fuerst had co-authored a highly unfavorable report regarding the conditions faced by Native peoples in 1973. When he applied for permission to travel to *Terra Indígena* again in 1974, the president of FUNAI General Bandeira de Mello described Fuerst's work in a meeting of the *Conselho Indigenista* saying, "it surprises with treacherous statements, completely devoid of fundament, with which Mr. Fuerst makes evident once again his un-disguisable attempt to 'degrade not only FUNAI, but our nation'."⁴³⁴ Fuerst's request for research permission for was denied. Three years later, when he reapplied, one council member was quick to vote to deny authorization on the basis of the dossier of information collected by the dictatorship's surveillance system. Anthropologist Julio Cezar Melatti, professor at the *Universidade de Brasília*, offered mediating words on behalf of Fuerst. "Prior to any decision by the Council," he suggested, "it would be most interesting to consult Itamaraty [the Ministry of Foreign Relations] regarding the veracity of the charges."⁴³⁵ When the diplomatic corps responded in support of Fuerst's application, he was granted permission, although the areas he was allowed to visit were limited.

⁴³⁴ Conselho Indigenista, "Ata da 59a Sessão," 9 February 1974, Doc: 0_DTI_0027, Fundo: ASI-FUNAI, Arquivo Nacional.

⁴³⁵ Ibid.

Anthropologists helped temper the programs and policies of the government, and in turn their participation allowed for the appearance of democratic systems even within a non-democratic regime. But even in the context of their engagement with the state, anthropologists developed strong discourses of activism and solidarity with Indigenous peoples. As they worked with government institutions and government funding, they nevertheless articulated their positions as independent from and critical of the regime.

At the 1975 meeting of the reconvened *Conselho Indigenista da FUNAI*, professor Lux Vidal emphasized scholars' independence from the government's agenda. She attributed the strong program in ethnology at USP to the commitment of her colleagues and students in the face of changeable official positions: "Even, let's say, when the relationship with FUNAI was..." she paused, "neutral. Indigenous peoples' problems always interested us. For example, years ago when I was in the Maranbá region with Doctor João Paulo Botelho Vieira Filho, in the micro region of Maranbá, we asked FUNAI for permission to enter Gavião territory to vaccinate. The response was negative. We went, and we vaccinated the Indians... This is to say that we never, even when relationship was bad, we never deviated from what we believed we should do."⁴³⁶ For Vidal, part of what drew students to the study of Indigenous peoples at USP was the moral compass of the anthropologists who resisted state imperatives.

In fact, discourses about political action would become central to imaginaries of the field. Brazilian anthropologists, as I discuss below, increasingly articulated a scientific and political identity that privileged the comingling of theoretical and practical

⁴³⁶ Conselho Indigenista, "Sessão 1 do Conselho Indigenista," AVESON 222 F lado B.

concerns. Advocacy work in conjunction with and opposition to government institutions would become a norm. Part of what made this possible was the extension of higher education made possible under the restrictive policies of military rule.

Brazilian anthropologists have widely considered whether there is a specifically *Brazilian* anthropology. Prominent ethnologists, particularly those with some international circulation who have represented Brazilian anthropology abroad, have offered various characterizations, but in all of them the political nature of their intellectual position is clear.⁴³⁷ As Mariza Peirano highlights, from the 1930s on, the cultivation of the social sciences and the emergence of anthropology as a distinct line of inquiry in Brazil has been centrally linked to nation-building projects.⁴³⁸ Based on this analysis, Peirano suggest that, “the definition of an ‘intellectual’ in Brazil includes a commitment to political problems in terms of the ideology of nationhood.”⁴³⁹ For

⁴³⁷ *Etnólogo*, or “ethnologist” in English, has a different connotation in Brazil than in some other national traditions. Although technically it refers broadly to the study of cultures, in Brazil it is used specifically to describe anthropologists who study Indigenous groups. See Eduardo Viveiros de Castro, “O campo na selva, visto da praia,” *Revista Estudos Históricos* 5, no. 10 (1992): 170.

⁴³⁸ Peirano’s earliest work on this topic was her doctoral dissertation, completed as a student of David Maybury-Lewis’ at Harvard. Her subsequent work builds on this analysis in comparative perspective, examining anthropology as a field in Brazil and India, positioning the United States as a point of comparison. Part of the foundation needed to make these comparisons, according to Peirano, is first establishing how anthropology came to be a social science in Brazil, and its differentiation from sociology as the discipline concerned with studying “the other” within Brazil in contrast to sociologists’ study of Brazil’s relation to other nation-states. See Mariza G. S. Peirano, “The Anthropology of Anthropology: The Brazilian Case,” (PhD diss., Harvard University, 1991[1981]), <http://marizapeirano.com.br/teses.htm>, accessed 14 April 2014; Mariza G. S. Peirano, *Uma antropologia no plural: três experiências contemporâneas* (Brasília: Editora UnB, 1991), 16, 85–104; and Mariza G. S. Peirano, “A antropologia como ciência social no Brasil,” *Etnográfica* 4, no. 2 (2000): 219–32.

⁴³⁹ Peirano, “Anthropology of Anthropology,” 135. Maybury-Lewis was not a supporter of simple monikers being used to determine “schools” of thought, and imparted that same perspective to Peirano. She recounts his approach and a letter he wrote to her on 25 January 1978 saying, “Note that I still insist on speaking of anthropology in Brazil rather than Brazilian anthropology. I continue to be dubious about national styles of anthropology—in the sense of a sort of *Volksgeist*—and would prefer to see you concentrate on national traditions — in the sense of the ongoing activity in a particular place. I do not find American or British anthropology terribly useful either, by the way. One of the remarkable things about [anthropology in Brazil] furthermore appears to be its eclecticism. So I would not try to force it into categorical pigeonholes

ethnology, specifically, this is intimately related to the “*questão indígena*” (Indigenous question) and the prominent position of Indigenous groups in the national imaginary. But national ideologies are not stable, wholly coherent, or insulated from transnational ideas and movements. During the 1960s and 1970s, political actors from diverse sectors of society offered competing visions for Brazil. As anthropologists responded to the military regime, the merging of theoretical concerns and practical advocacy became central to the field’s identity.

Studying Indigenous peoples was foundational to Brazilian anthropology, both because of the national imaginary of Native heritage and the long history of Native people as privileged sources of knowledge. Even as ethnographic work on urban and peasant communities has accounted for a larger portion of anthropological investigation in Brazil, many scholars still concur with Antonio Carlos de Souza Lima’s contention that “the origins and growth of anthropology in Brazil are synonymous with the study of its indigenous peoples. It is also synonymous with efforts to expose ethical issues and help defend against actions that compromise the rights of indigenous peoples.”⁴⁴⁰

Even those who poke fun at the idealization of ethnology as the bedrock of anthropology also articulate the naturalized role of the ethnologist as political actor on behalf of the group studied. In a 1992 commentary, Eduardo Viveiros de Castro offered tongue-in-cheek commentary saying, “[Ethnologists] are seen by their colleagues as practicing a bizarre trade, a bit antiquated, symbolically important but too technical, at its

for the sake of a simple thesis.” See Mariza Peirano, “Lembranças,” *Mana* 14, no. 2 (2008): 570, doi:10.1590/S0104-93132008000200013.

⁴⁴⁰ Antonio de Souza Lima, “Anthropology and Indigenous People in Brazil: Ethical Engagement and Social Intervention,” *Practicing Anthropology* 26, no. 3 (2004): 11, doi:10.17730/praa.26.3.j57t15251538660m.

core, irrelevant. In turn, it is possible that we think of ourselves as the aristocracy of the discipline, direct descendants of the heroic founders—like Brahmins of the religion of anthropology, forged in the crucible of fieldwork alongside authentic primitives, lost in the heart of the jungle.”⁴⁴¹ However, Viveiros de Castro was careful to include a footnote regarding his political action even as he emphasized the perceived irrelevance of classical ethnology. He thought it necessary to complement his description of his intellectual genealogy by saying “I remind you that my career as a ‘classic’ ethnologist did not prevent me from participating to the best of my abilities in the fight for indigenous rights.”⁴⁴² While Viveiros de Castro emphasized that his political and theoretical work were largely independent of one another, other scholars have asserted that the mixing of the two is both inevitable and desirable. In her 1990 piece, “Ethnology Brazilian Style,” Ramos argued that political action constitutes a norm in Brazilian anthropology, and that foreigners’ perplexity at the mixing of theoretical and practical concerns is misplaced.⁴⁴³

Scholars trace this orientation towards action to different origins within the longer history of the field. Ramos ties “political commitment to the defense of the rights of the

⁴⁴¹ Viveiros de Castro, “O campo na selva, visto da praia,” 171.

⁴⁴² Ibid., 195. He provided more detail saying, “For two terms, I was a member of the indigenous commissions of the *Associação Brasileira de Antropologia*, I lobbied during the creation of the new Constitution, I attended innumerable presentations and public acts, I wrote about this or that absurdity perpetrated against *povos indígenas*, I risked analyses for Indigenist affairs, I worked for the demarcation of Araweté territory, etc.” (ibid.). Perhaps Viveiros de Castro had some sense that he would come under criticism for the accounts of lowland South American cosmologies and his political positioning. For debates over Viveiros de Castro’s writings on perspectivism, see Alcida Rita Ramos, “The Politics of Perspectivism,” *Annual Review of Anthropology* 41, no. 1 (2012): 481–94, doi:10.1146/annurev-anthro-092611-145950; Lucas Bessire, *Behold the Black Caiman: A Chronicle of Ayoreo Life* (Chicago: University Of Chicago Press, 2014).

⁴⁴³ Alcida Rita Ramos, “Ethnology Brazilian Style,” *Cultural Anthropology* 5, no. 4 (1990): 453. For an interesting comparison on the relationship between applied and theoretical anthropology in the North American context, see Feldman’s notes on what it means to practice anthropology “close to home” in the Alaskan context: Kerry D. Feldman, “Engaged Anthropology on ‘the Last Frontier,’” *Annals of Anthropological Practice* 37, no. 1 (2013): 113–32, doi:10.1111/napa.12020.

peoples studied,” to the centrality of interethnic relations as theoretical orientation in the field.⁴⁴⁴ Following Melatti, Ramos emphasizes that while foreign anthropologists working in Brazil have tended to study the social organization and cultural institutions of Native groups, Brazilian scholars have been less prone to “bracket out” the consequences of contact.⁴⁴⁵ Temporally, Ramos locates the origin of this political orientation in the 1950s, with the rise of scholarship focusing on contact and interethnic friction, specifically that of Darcy Ribeiro and Roberto Cardoso de Oliveira.

Often considered the foundational figures of modern Brazilian ethnology, the two social scientists came of age during the Vargas era, as the March to the West was at its height.⁴⁴⁶ Preceding his colleague by a number of years, Ribeiro became one of the most prominent voices regarding Indigenous affairs and the state in the mid-twentieth century, writing extensively and compellingly about the plight of Native peoples in the early twentieth century. He advocated the position that acculturation was inevitable and necessary, and would later be fiercely criticized by younger members of the profession for his continued emphasis on an evolutionist vision of integration in the service of the

⁴⁴⁴ Ramos, “Ethnology Brazilian Style,” 453.

⁴⁴⁵ Ibid. See also, Julio Cezar Melatti, “A Antropologia no Brasil: Um Roteiro,” *Série Antropologia* 38 (2007 [1983]): 1–50. Melatti gives a broad overview of the history of anthropology, including subsections on the three “sub-fields” that are usually treated separately (archaeology, linguistics, and physical anthropology).

⁴⁴⁶ Ribeiro was responsible for bringing Cardoso de Oliveira to Rio in 1954 while he was head of research at the *Museu do Índio*. (See Afrânio Garcia Jr., “Fundamentos empíricos da razão antropológica: a criação do PPGAS e a seleção das espécies científicas,” *Mana* 14, no. 2 (2009): 415). Other foundational figures, though not Brazilian, include Curt Nimuendajú, Florestan Fernandes (Cardoso de Oliveira’s doctoral advisor), and Edgar Roquette-Pinto. On Nimuendajú, see Julio Cezar Melatti, “Nimuendajú E Os Jê,” *Série Antropologia* 49 (1985): 1–22; Marta Amoroso, “Nimuendajú às voltas com a história,” *Revista de Antropologia* 44, no. 2 (2001): 173–86. On sociologist Florestan Fernandes, see Mariza G. S. Peirano, *Uma antropologia no plural: três experiências contemporâneas* (Brasília: Editora UnB, 1991), 51–84. On physical anthropologist Edgar Roquette-Pinto, see: Santos, “Guardian Angel on a Nation’s Path”; Ricardo Ventura Santos, “A obra de Euclides da Cunha e os debates sobre mestiçagem no Brasil no início do século XX: ‘Os Sertões’ e a medicina-antropologia do Museu Nacional,” *História, Ciências, Saúde-Manguinhos* 5, no. supl. (1998): 237–54; Souza, “Em busca do Brasil.”

state.⁴⁴⁷ As Ribeiro's approach to acculturation failed as an analytical model, in the 1960s Cardoso de Oliveira's notion of interethnic friction brought a more symmetrical analysis to the vagaries of contact. He anticipated later scholarship that theorized ethnicity as categories produced through interactions of two groups, rather than a preexisting and static quality.⁴⁴⁸ A number of authors have suggested that the fact that Ribeiro and Cardoso de Oliveira both worked for the SPI during the 1940s and 1950s was essential in shaping their visions of the Indigenous panorama.⁴⁴⁹ Their public work was also important to shaping broader perceptions of the field and of the role of anthropologists in relation to the state.

Historian of anthropology Mariza Corrêa has described the period of 1960–1980 as the key moment of institutionalization for the field,⁴⁵⁰ and emphasized the developments that took place during this period by examining Darcy Ribeiro's changing position in the field. As an influential public intellectual, Ribeiro had been tapped by

⁴⁴⁷ Darcy Ribeiro, *Os índios e a civilização: A integração das populações indígenas no Brasil moderno* (Rio de Janeiro: Civilização Brasileira, 1970). For historical analyses of Ribeiro, see: Ramos, "Ethnology Brazilian Style," 462. On Ribeiro's attempt to shape his own legacy, see Luciana Quillet Heymann, "O arquivo utópico de Darcy Ribeiro," *História, Ciências, Saúde-Manguinhos* 19, no. 1 (2012): 261–82. doi:10.1590/S0104-59702012000100014. For a discussion of the heated disagreements between Roberto da Matta and Darcy Ribeiro following Ribeiro's return from exile, see Mariza Corrêa, "A antropologia no Brasil 1960–1980," in *História das ciências sociais no Brasil: Volume 2*, ed. Sergio Miceli (São Paulo: IDESP, 1995), 91–98.

⁴⁴⁸ Mariza Peirano, "Brazil: Otherness in Context," in *A Companion to Latin American Anthropology*, ed. Deborah Poole (Malden, MA: Wiley-Blackwell, 2008), 60; Ramos, "Ethnography Brazilian Style," 463–464. Later scholars including Mexican anthropologist Guillermo Bonfil Batalla and the other participants of the 1971 Barbados Conference would draw on Cardoso de Oliveira's work, which anticipated an important shift in thinking about ethnicity. Mariza Corrêa describes this briefly in "O mato e o asfalto: Campos da antropologia no Brasil," *Sociologia & Antropologia* 1, no. 1 (2011): 212. See: Guillermo Bonfil Batalla, "El concepto de indio en América: Una categoría de la situación colonial," *Anales de Antropología* 9 (1972), 105–124; Fredrik Barth, "Introduction," in *Ethnic Groups and Boundaries: The Social Organization of Culture Difference*, ed. Fredrik Barth (Long Grove, IL: Waveland Press, Inc., 1998[1969]), 9–38.

⁴⁴⁹ Ramos, "Ethnology Brazilian Style," 462; Julio Cezar Melatti, "A antropologia no Brasil: um roteiro," *Série Antropologia* 38 (2007 [1983]): 14.

⁴⁵⁰ Corrêa, "A antropologia no Brasil 1960–1980."

President Juscelino Kubitschek in 1959 to plan the federal university for the new capital. He also worked as chief of staff for president João Goulart, planning agricultural and educational reform.⁴⁵¹ As a result of his political connections, he was one of few anthropologists to be exiled by the military regime following the coup of 1964 that overthrew Goulart. When Ribeiro returned to Brazil in 1976, he lamented what he saw as the irrelevance and lack of independent thought that characterized the younger generations now running anthropology in the academy.⁴⁵² Corrêa described the changes that took place during his exile by writing, “In these twenty years, anthropology was transformed from an almost artisanal undertaking into a profession.”⁴⁵³ This professionalization occurred during the most repressive years of military rule. It resulted in a new version of the discipline that combined theoretical work with advocacy, in which conciliatory and oppositional approaches comingled.

Universities under Military Rule

While Brazilian reformers in the 1930s–1940s emphasized the imperative of education for the sake of creating a cultured elite to help lead the rapidly centralizing nation, by the 1950s, discourse had broadened to emphasize science and technology as

⁴⁵¹ For a fascinating discussion of Ribeiro as an intellectual figure formed in part through his period of exile, see Tracy Devine Guzmán, “Subalternidade hegemônica: Darcy Ribeiro e a virtude da contradição,” *Cadernos de estudos culturais* 1 (2011): 171–92. Guzmán describes Ribeiro saying, “Ribeiro lived, worked, wrote, loved, and died immersed in the sea of his own contradictions” (192).

⁴⁵² Corrêa, “A antropologia no Brasil 1960–1980,” 91–98. Guzmán gives a slightly more sympathetic reading. Interestingly, part of what he objected to was what he saw as a lack of *action* and political involvement. He described this saying, “An English poet can be just a poet. But in a country with its bowels exposed, like Brazil, an intellectual has the obligation to take a position,” as cited in Guzmán “Subalternidade hegemônica,” 180.

⁴⁵³ Corrêa, “A antropologia no Brasil 1960–1980,” 92.

key to development.⁴⁵⁴ The government established new funding institutions, such as the *Conselho Nacional de Pesquisas* (National Research Council, CNPq), which expanded access to research funds across the field, new federal universities were established, and enrollment began to increase. These patterns, which began in the 1950s, accelerated in the years following the military coup.⁴⁵⁵

This acceleration influenced higher education both for better and for worse. The prioritization of investment in education and research during this period meant that universities expanded, and teaching and research at the university level became a viable profession. For example, from 1970 to 1980, the number of students applying for undergraduate educations increased fivefold, and university enrollment increased by almost 200%.⁴⁵⁶ But at the same time, the regime (particularly after the consolidation of the military's hold on power in 1968) imprisoned, fired, or forcibly retired faculty members whose ideological leanings were considered suspect; implemented surveillance systems; and repressed student organizing.

Motta has argued that the profound changes to the university system during the military dictatorship were the result of more than heavy-handed administration by a unified block of military interests; in his account, conciliatory politics played a central

⁴⁵⁴ Simon Schwartzman, *A Space for Science: The Development of the Scientific Community in Brazil* (State College: Penn State University Press, 2005), 202. Here I am focusing primarily on academia and discussions about university-level education. However, many other socio-technical projects had sought to form and reform Brazilian publics. For an exploration of technological innovation and the relationship between industrialists and laborers during the early twentieth century in São Paulo, for example, see Barbara Weinstein, *For Social Peace in Brazil: Industrialists and the Remaking of the Working Class in São Paulo, 1920–1964* (Chapel Hill: University of North Carolina Press, 1997).

⁴⁵⁵ For an overview see Schwartzman, *Space for Science*, 215–236.

⁴⁵⁶ *Ibid.*, 220.

role.⁴⁵⁷ In the 1960s and 1970s, actors across the political spectrum agreed that the education system in Brazil needed improvements, and the leftist government of João Goulart had begun the process of proposing reform in response to a well-organized student movement before the coup. While the Left and Right could agree on the need for change, their visions of progress differed. Motta has also highlighted the fact that in order to successfully implement its modernization program, military administrators depended on intellectual elites, who could draw on their own social positions and connections to exert some influence, however limited. The student movement also featured as one of the most powerful forces of resistance against the regime.⁴⁵⁸ Finally, the participants in military rule were not a unified block, and so the resulting policies of reform involved heterogeneous social and political alliances.

The regime conducted two waves of purges. The first occurred in 1964, immediately after the military assumed power, and imprisoned between twenty thousand and thirty thousand people. Most were released within a number of days; by the one-month anniversary of the coup, approximately three thousand people remained imprisoned.⁴⁵⁹ Although specific numbers regarding the academics and students are unavailable, prominent figures from across the social sciences were among those targeted. The second crackdown came in the wake of widespread student protests in 1968. Following outspoken protest from a member of Congress, a ruling by the Supreme Court demanding the release of 81 student protestors, and the refusal of the Congress to

⁴⁵⁷ Motta, *As universidades e o regime militar*, 12–15.

⁴⁵⁸ *Ibid.*, 154–164; Skidmore, *Politics of Military Rule*, 49–51 and 75–77.

⁴⁵⁹ Motta, *As universidades e o regime militar*, 26.

revoke the immunity of two of its members, President Costa e Silva issued two acts that hardened the grip of the military regime. Institutional Act (AI-5) and Supplementary Act no. 38 suspended Congress, outlawed protest, and led to a new wave of repression. AI-5 had far greater impact on academia than the first wave of purges; approximately seventy professors from the *Universidade de São Paulo* (USP)⁴⁶⁰ and seventy-nine from the *Universidade de Brasília* were forced to retire, with others removed from posts at universities across the country.⁴⁶¹ These purges also affected proportionally far more students than the first.⁴⁶²

AI-5 coincided fairly closely with the institution of education reform, which proceeded unevenly and continued even as repression eased under the presidency of Ernesto Geisel (1974–1979). Between 1968 and 1978, the number of professors employed by federal universities in Brazil grew from fifteen thousand to thirty-eight thousand; student enrollment in federal higher education went from 100,000 to 290,000 in 1979, accompanied by concurrent major growth in private institutions.⁴⁶³ The government built new campuses and restructured educational programming from a continental model of set courses of study to a credit system, largely modeled after the United States, with the aim of allowing greater flexibility and higher graduation rates.⁴⁶⁴ The benefits of these improvements, however, were not evenly distributed, and did not address the critical social issues that reformers under democratic rule had prioritized. The

⁴⁶⁰ Skidmore, *Politics of Military Rule*, 83.

⁴⁶¹ Motta, *As universidades e o regime militar*, 165.

⁴⁶² *Ibid.*, 160. For detailed documentation of surveillance and repression in the universities, see Brasil, “Relatório da Comissão Nacional da Verdade: textos temáticos,” (Brasília: CNV, 2014), 265–298.

⁴⁶³ Motta, *As universidades e o regime militar*, 248.

⁴⁶⁴ On USAID and its important (but often overstated) role in educational reform see *ibid.*, 110–147.

reform was uneven, progressed in fits and starts, and maintained and even contributed to the stratification of elite sectors.⁴⁶⁵

Still, the implications for the field of anthropology were significant, and meant that the alumni of newly established graduate programs could quickly find employment and—depending on the large fluctuations in the Brazilian economy that marked the 1970s—funding for fieldwork. Likewise, the government could hire Brazilian anthropologists to consult on projects, whether concerning questions of land demarcation, education, or—following pressure by national and international advocates and funding agencies—to oversee large-scale development projects.

“Xavantólogos Militantes” on the International Stage

The Maybury-Lewises were caught up in these large-scale dynamics at every level following the period of their research in Xavante territory. Collaborating on the institutionalization of graduate training at the *Museu Nacional*, David Maybury-Lewis waded into the fray of fast-growing academic programs caught between material growth and ideological repression under military rule. Meanwhile, with the strong support and perhaps even insistence of his wife, Pia Maybury-Lewis, the two entered the chorus of voices regarding Indigenous rights. The Maybury-Lewises founded an international NGO to draw attention and funnel resources to aid Indigenous groups like the Xavante. Working from the relative safety of the United States, the Maybury-Lewises were able to critique the military regime with less fear of retribution. It was in these two acts, each

⁴⁶⁵ Ibid., 352–355.

made possible by his earlier fieldwork with the Xavante, that Maybury-Lewis most influenced his discipline.

The Maybury-Lewises' continuing connections to Brazil were essential to both projects of institutionalization. After his initial fieldwork, David Maybury-Lewis had returned to Xavante territory in two summer field seasons to follow up on details for his ethnographic monograph.⁴⁶⁶ Even after his last trip to visit the Xavante in 1964 before the publication of his monographs, he continued to spend a great deal of time in Brazil. From 1962 to 1967 Maybury-Lewis oversaw the field research of six graduate students with funding for the comparative study of Central Brazilian groups from the National Institute of Mental Health in the United States.⁴⁶⁷ As his students set out on what he would call the Harvard-Central Brazil Project, Maybury-Lewis began receiving consistent reports from the field. His primary interest, motivated by his ongoing disagreement with Lévi-Strauss' interpretation of data on Jê-speaking groups, diverged somewhat from the inter-ethnic friction approach that Cardoso de Oliveira was developing. These two strands would come together with the growth of a graduate program at the *Museu Nacional*.

Maybury-Lewis' concern for sustaining his research program was a major motivator for this formalization of collaborations with Brazilian colleagues. As the funding from the National Institute of Mental Health came to a close, in 1966 Maybury-Lewis began to help anthropologists Roberto Cardoso de Oliveira and Luiz de Castro

⁴⁶⁶ Maybury-Lewis, *Akwẽ-Shavante Society*,” xxi–xxv.

⁴⁶⁷ David Maybury-Lewis, ed., *Dialectical Societies: The Ge and Bororo of Central Brazil* (Cambridge, MA: Harvard University Press, 1979), vi.

Faria build a new graduate program at the *Museu Nacional*.⁴⁶⁸ The Ford Foundation was integral to the growth of the new program. Yet prior to accepting funding, Cardoso de Oliveira and Maybury-Lewis were wary of the Foundation's expectations. As Afrânio Garcia highlights in his close reading of their correspondence, the anthropologists were unsure as to what compromises they might have to make in order to receive support. In one letter to Cardoso de Oliveira, Maybury-Lewis noted with distaste how a member of the Foundation challenged the quality of sociological work coming out of São Paulo due to its "Marxist" nature. Maybury-Lewis went on, however, to suggest that, "it seems to me to be very likely that they will give us money." For him, the only question was at what cost. "The problem at this moment," he continued, "is simply whether we will be prepared to accept their conditions and I do hope that you will have an opportunity to explore this matter with your customary subtlety when you next talk to [the program manager]."⁴⁶⁹ Apparently, Maybury-Lewis and Cardoso de Oliveira decided the funding was worth the compromises, and the *Programa de Pós-Graduação em Antropologia Social* (PPGAS-MN, Postgraduate Program in Social Anthropology) opened in 1968 with

⁴⁶⁸ Afrânio Garcia Jr. provides an overview of the creation of the PPGAS of the Museu Nacional, with attention to each of the three key figures—Cardoso de Oliveira, Maybury-Lewis, and Castro Faria. He examines the intellectual legacy of each, as well as commenting on the fraught political moment that juxtaposed repression of critical thought with investment in high quality graduate programs. Cardoso de Oliveira's position was somewhat precarious under the military dictatorship; in addition to the surveillance and repression of his doctoral advisor Florestan Fernandes and the exile of colleague Darcy Ribeiro, his cousin, Henrique Fernando Cardoso, also fled the country to Chile, where he would publish influential scholarship on dependency theory. Garcia suggests that Cardoso de Oliveira's alliance with a prominent British anthropologist and Harvard University likely helped him gain the political clout necessary to advance the implementation of the PPGAS-MN. See Garcia, "Fundamentos empíricos da razão antropológica," 416.

⁴⁶⁹ David Maybury-Lewis to Roberto Cardoso de Oliveira, as quoted in Garcia, "Fundamentos empíricos da razão antropológica," 429.

funding from the Ford Foundation, just five months before the military regime implemented AI-5.⁴⁷⁰

Garcia describes the dissonance of the expansion of violent repression of the *Atto Institucional* and the increased funding and support for academic programs as a “double bind” for the early generations of students in the newly formed program: “The material conditions for doctoral students were without parallel compared to the past. But pity the ‘new heirs’ if they tried to rest on their laurels: the conditions for university exchange and intellectual debate deteriorated every day (persecution of the presses, the closing of collectives and scientific journals, courses and colloquia monitored, the requirement of a ‘testimony of ideology’ for university recruitment, etc).”⁴⁷¹ The PPGAS depended on Ford Foundation funding for four years until it received support from the federal agency FINEP (*Financiamento de Estudos e Projetos*). In the meantime, the program’s consolidation facilitated Maybury-Lewis’ ongoing research in Central Brazil, as well as providing a place for his Brazilian students to teach upon completing doctoral work or their study abroad at Harvard.⁴⁷²

Maybury-Lewis and Cardoso de Oliveira navigated the fraught context of Cold War funding to invest time and effort in institution building that would be highly influential for the field of anthropology in Brazil. In the Brazilian context and in response to his earlier fieldwork, however, David Maybury-Lewis was not satisfied with a purely

⁴⁷⁰ Garcia, “Fundamentos empíricos da razão antropológica,” 432. For a broader perspective on the Ford Foundation and its role in funding Brazilian higher education, see Sergio Miceli, *A desilusão americana: relações acadêmicas entre Brasil e Estados Unidos*, Biblioteca da república (São Paulo: Sumaré, 1990), 17–29 and citations therein.

⁴⁷¹ Garcia, “Fundamentos empíricos da razão antropológica,” 432.

⁴⁷² Roberto da Matta, Roque de Barros Laraia, and Julio Cezar Melatti all spent time with Maybury-Lewis at Harvard before teaching at the *Museu Nacional*.

academic endeavor of institution building. At the height of the developmentalist push into Central Brazil, Maybury-Lewis was well aware of how the push of Brazilian society into Indigenous territory put at risk the very groups that he and his students were studying: He joined the voices of protest, creating an organization to advocate for the people that he had previously only studied. Years later, Pia Maybury-Lewis would report that their discussions about what they might be able to do to help the Xavante began while they were still in the field.⁴⁷³ But it was not until 1972 that the Maybury-Lewis together with Harvard colleagues Orlando Patterson and Evon Vogt founded Cultural Survival. This came shortly after the establishment of the International Working Group on Indigenous Affairs (IWGIA, 1968, Denmark) and Survival International (1969, United Kingdom). These new NGOs focused predominantly on issues concerning Indigenous peoples in Latin America.

Following Cultural Survival's official incorporation David Maybury-Lewis wrote to his colleagues in May 1972 stating that the organization's intension was to "further the interests of small, indigenous societies throughout the world who are threatened with cultural or physical extinction."⁴⁷⁴ As they waited for non-profit status, he requested that recipients of his letter send information and publications concerning the groups that they thought best fit this description, going on to specify, "At this stage we feel that imagination and expertise are needed as much if not more than outright financial

⁴⁷³ Jerome M. Levi, "From Mato Grosso to Millennium: An Introduction to the Anthropology of Dialectical Observations," *Anthropological Quarterly* 82, no. 4 (2009): 881.

⁴⁷⁴ Maybury-Lewis to Neel, s.d. [May 1972], Maybury-Lewis Correspondence (2 of 2), Neel Papers, APS.

assistance.”⁴⁷⁵ The organization’s earliest goals were to collect information and consider different approaches to support the interests of Indigenous groups.

Within a few years, Cultural Survival began to produce publications and educational materials aimed at a broad public. Perhaps the most prominent of these was the newsletter, which began publication in the spring of 1976, and eventually became the more formal, *Cultural Survival Quarterly* in 1981. Maybury-Lewis drew on Cold War rhetoric of freedom and vigilance, in the service of the pluriethnic, multicultural society that he saw as the only viable future for Indigenous groups. He introduced the newsletter and the goal of the organization in its inaugural issue writing: “*CULTURAL SURVIVAL* aims to help small societies have a say in their own future, to become, in effect, successful ethnic minorities. This is not a matter which only concerns a few, out of the way tribal peoples. It is vital for us all to insure that we live in a world based on the practice of mutual tolerance and respect, for these are the only true guarantees of freedom. But the price of freedom is eternal vigilance.”⁴⁷⁶ This emphasis on tolerance and multiculturalism would pervade the anthropologist’s approach for the rest of his career, motivating a number of his future publications.⁴⁷⁷ His description combined the thread of self-determination, “to help small societies have a say in their own future,” with

⁴⁷⁵ Ibid.

⁴⁷⁶ David Maybury-Lewis, “Message from the President of Cultural Survival,” *Cultural Survival Newsletter* 1 no. 1 (1976): 1.

⁴⁷⁷ Maybury-Lewis’ later scholarship, and particularly his work aimed at public audiences focused on the question of ethnicity and the state. See: David Maybury-Lewis, *The Politics of Ethnicity: Indigenous Peoples in Latin American States* (Cambridge, MA: Harvard University David Rockefeller Center for Latin American Studies, 2002); David Maybury-Lewis, *Indigenous Peoples, Ethnic Groups, and the State* (Boston: Pearson, 2001[1997]); Laurie Kain Hart, “Popular Anthropology and the State: David Maybury-Lewis and Pluralism,” *Anthropological Quarterly* 82, no. 4 (2009): 1033–42, doi:10.1353/anq.0.0101.

invocations of extinction—“Without help, they die.”⁴⁷⁸ While other organizations that rallied political support for urgent issues often framed their arguments in terms of the vulnerability and helplessness of the people they sought to help, Cultural Survival attempted to urge action while also recognizing some measure of agency in the people for whom it advocated.⁴⁷⁹ This more measured discourse would also be present in Maybury-Lewis’ later advocacy work.

Early issues of the newsletter focused on public education for an English-speaking audience. In the mid-1970s, Cultural Survival hosted public film screenings and exhibits, mostly held on Harvard’s campus. They advertised and distributed publications from the IWGIA, and began producing products such as a lecture kit containing color slides, an audiocassette, and a list of discussion questions on the topic of “Indigenous People in Search of a Future.” Much of the coverage in the Newsletter focused on issues throughout the Americas, although short articles also profiled problems in other regions of the world.

By 1980, Cultural Survival had partnered with local organizations in five countries in Latin America to support ongoing projects that the organization’s board saw

⁴⁷⁸ On discourses of urgency and impending extinction and how these narratives have animated scientific and anthropological work, see Radin, *Life on Ice*, esp. 86–117; Sadiya Qureshi, “Dying Americans: Race, Extinction and Conservation in the New World,” in *From Plunder to Preservation: Britain and the Heritage of Empire, c. 1800–1940*, ed. Astrid Swenson and Peter Mandler (Oxford: Oxford University Press, 2013), 269–288. Adrianna Link also eloquently addresses the links between urgent anthropology and narratives of extinction, particularly in regards to the intersection of ecological and cultural approaches. See: Link, “Salvaging a Record for Humankind,” esp. 106–148. Both Radin and Link discuss the fact that discourses of extinction were already being critiqued at this time. See Jacob W. Gruber, “Ethnographic Salvage and the Shaping of Anthropology,” *American Anthropologist* 72, no. 6 (1970): 1289–99.

⁴⁷⁹ On Survival International and “the ongoing characterization of indigenous people as politically weak, vulnerable, and almost defenceless,” see Jochen Kemner, “Fourth World Activism in the First World: The Rise and Consolidation of European Solidarity with Indigenous Peoples,” *Journal of Modern European History* 12, no. 2 (2014): 269.

as supporting self-determination. From land demarcation projects to the establishment of a bilingual radio station, Cultural Survival described the efforts it supported as all contributing to “a minority’s adaptive capacity in a multi-ethnic society.” The authors elaborated on their criteria by arguing that, “Rather than attempt to freeze an ethnic group into some romantic traditional or picturesque historical moment, the projects selected will increase a group’s understanding of its situation and help them to make their own adaptation to change.”⁴⁸⁰ Cultural Survival’s approach fit closely with the emphasis that the 1971 Declaration of Barbados placed on cultural rights, as well as fitting with a broader tendency not to challenge the authority or boundaries of the state. As Karen Engle and others have explored, both local and international advocacy organizations that focused on Indigenous rights in Latin America shied away from the invocation of the concept of sovereignty during the 1970s and well into the 1990s.⁴⁸¹ While Native groups in North America organized around concepts of land and pushed for the recognition of treaties as agreements between sovereign entities, much Latin American advocacy emphasized cultural rather than territorial rights. This is clear in the tempered tone Cultural Survival took in a 1980 newsletter when the editors wrote that, “a degree of local autonomy does not imply the development of autonomous and potentially hostile states within states. A nation which recognizes the strength of ethnic group aspirations and accepts their legitimacy will be best served by supporting efforts of self-

⁴⁸⁰ Cultural Survival, “Some Well-Placed Drops in a Large Bucket,” *Cultural Survival Newsletter* 4, no. 2 (1980): 1.

⁴⁸¹ Engle, *Elusive Promise*, 57 and citations therein.

determination.”⁴⁸² In these calculated statements, among others, the Maybury-Lewises and their organization took a markedly more moderate tone than some of their colleagues with similar aims but bolder discourse.

Cultural Survival’s position was not immune from critics within international activist circles, nor was it immune from critiques from anthropologists who thought anthropology should observe without intervening. Furthermore, donors and organizers alike noted the delicate position of an organization based in the United States encouraging progressive policies towards Native rights abroad, given the history and ongoing reality of US abuses toward its own Indigenous peoples. Maybury-Lewis responded to this concern in a letter to James Neel, writing, “Cultural Survival is however, I am happy to say, not in the business of giving other people moral lectures. What we try to do is to educate the public ... that it is possible to do it right and that this alternative strategy is not counter-developmental nor too expensive. In any case we only operate in other countries in connection with or in support of indigenous groups and institutions, preferably in support of the Indians themselves.”⁴⁸³

In order to engage policy makers, Maybury-Lewis and his organization emphasized their high scholarly standards of evidence and tried to create economically palatable proposals. Maybury-Lewis’ focus on precision of language and empirically based assessment brought him—and by extension Cultural Survival—into conflict with colleagues from other international NGOs. One particularly salient example was his

⁴⁸² Cultural Survival, “Some Well-Placed Drops,” 1–2.

⁴⁸³ David Maybury-Lewis to James V. Neel, 17 March 1980, Maybury-Lewis Correspondence (2 of 2), Neel Papers, APS.

collaboration with MIT anthropologist James Howe on an assessment of Indigenous rights violations in Paraguay. This example illustrates the relatively moderate tone that Cultural Survival and Maybury-Lewis adopted in comparison to other NGOs.

In the early 1970s, a number of anthropologists raised the alarm about the fate of the Aché in eastern Paraguay.⁴⁸⁴ The case drew international attention as the Paraguayan anthropologists were persecuted and scholars and activists from abroad accused the government of perpetrating genocide.⁴⁸⁵ The Aché became a rallying point for international attention to abuses of Indigenous rights. In response to the continuing public outcry, by 1978 the Carter administration and USAID commissioned a report.⁴⁸⁶ USAID officials asked Maybury-Lewis and Howe to conduct a field survey to look into the accusations of abuses. Their findings, published by Cultural Survival in 1980, rejected

⁴⁸⁴ Robert K. Hitchcock, Charles Flowerday, and Thomas E. Koperski, "The Ache of Paraguay and Other 'Isolated' Latin American Indigenous Peoples: Genocide or Ethnocide?" in *Genocide of Indigenous Peoples: A Critical Bibliographic Review*, ed. Samuel Totten and Robert K. Hitchcock (New Brunswick: Transaction Publishers, 2011), 173. These accusations were first raised at the Barbados Conference of 1971 by Paraguayan anthropologist Miguel Chase Sardi. He framed the case of the Aché (then often referred to as the Guayaki-Aché) as an example of genocide perpetrated by colonists and their descendants over the 500 years since contact. In following years, anthropologists who worked with IWGIA and Survival International continued to accuse the Paraguayan government of perpetuating genocide against the Guaraní and Aché. See Richard Reed and John Renshaw, "The Aché and Guaraní: Thirty Years after Maybury-Lewis and Howe's Report on Genocide in Paraguay," *Tipiti: Journal of the Society for the Anthropology of Lowland South America* 10, no. 1 (2012): 1–2. Reed and Renshaw cite anthropologists Mark Münzel and Richard Arens, as well as journalistic coverage by Norman Lewis in the 1975 Sunday Times Magazine. See: Richard Arens, *The Forest Indians in Stroessner's Paraguay: Survival or Extinction?* (London: Survival International Report, 1978); Mark Münzel, "The Aché Indians: Genocide in Paraguay," International Working Group on Indigenous Affairs Document No. 11 (Copenhagen: IWGIA, 1973); Mark Münzel, "The Aché: Genocide Continues in Paraguay," International Working Group on Indigenous Affairs Document, No. 17 (Copenhagen: IWGIA, 1974).

⁴⁸⁵ In 1975, the Paraguayan regime denounced Chase Sardi and three other anthropologists as subversives. They were imprisoned, and tortured, widely understood to be retribution for their newly formed organization, *Projeto Marandú*, which counseled Indigenous people about their legal rights. See: Cyril S. Belshaw, "A Call for Help: Miguel Chase Sardi Imprisoned in Paraguay," *Current Anthropology* 17, no. 3 (1976): 541–43; John Renshaw, "Miguel Chase Sardi," *Anthropology Today* 17, no. 5 (2001): 28, doi:10.1111/1467-8322.00083.

⁴⁸⁶ While the United States had supported the dictatorship of General Alfredo Stroessner even through years of increasing repression in the mid-1970s, official US policy towards Paraguay shifted following the election of Jimmy Carter.

reports of genocide, focusing rather on how government economic and development policies created conditions of dire poverty and ill health for Indigenous groups.⁴⁸⁷

These results were controversial within activist circles, leading to a vigorous debate about the meanings and uses of the term genocide. Maybury-Lewis and Howe held that the state did not advance a sustained policy aimed at ethnic elimination, and thus despite disastrous conditions, genocide was not the correct terminology.⁴⁸⁸ Rather, they promoted an integrated approach to thinking about the economic and social forces at play. When critics of their research, including members of Survival International, pushed back, reportedly, Maybury-Lewis responded that Cultural Survival's work met an "academic" standard rather than a "journalistic" one.⁴⁸⁹

While some critics accused Maybury-Lewis' of taking a soft stand on genocidal government practices and exacerbating the already formidable barriers to advocacy work,⁴⁹⁰ others cited this report as a reflection of the high standards Maybury-Lewis prescribed for anthropological advocacy.⁴⁹¹ One of Maybury-Lewis' students, who later

⁴⁸⁷ David Maybury-Lewis and James Howe, *The Indian Peoples of Paraguay: Their Plight and Their Prospects* (Cambridge, MA: Cultural Survival, 1980); Richard Reed, "At the Intersection of Scholarship and Activism," *Anthropological Quarterly* 82, no. 4 (2009): 1065–68.

⁴⁸⁸ Since this period, scholars in Indigenous studies have contested narrow uses of the term "genocide" emphasizing how settler colonialism establishes structures that persistently result in the elimination of Native people and lands. See, for example, Simpson, *Mohawk Inturruptus*; Simpson and Smith, *Theorizing Native Studies*; Wolfe, "Settler Colonialism and the Elimination of the Native."

⁴⁸⁹ Hitchcock, Flowerday, and Koperski, "The Ache of Paraguay and Other 'Isolated' Latin American Indigenous Peoples: Genocide or Ethnocide?" 180.

⁴⁹⁰ Jens Dahl, *IWGIA: A History* (Copenhagen: IWGIA, 2009), 170–171.

⁴⁹¹ On the case of the Aché, later anthropologists, including Kim Hill and Magdalena Hurtado, and even Chase Sardi himself offered evidence that the earliest reports had been overstated. René Harder Horst cites an interview with Chase Sardi in 1995 in which the Paraguayan anthropologist stated that Münzel had exaggerated claims; see Horst, "The Catholic Church, Human Rights Advocacy, and Indigenous Resistance in Paraguay, 1969–1989," *The Catholic Historical Review* 88, no. 4 (2002): 723–44. For an overview of these data and the progression of scholarship on the question of genocide, see Hitchcock, Flowerday, and Koperski, "The Ache of Paraguay and Other 'Isolated' Latin American Indigenous Peoples: Genocide or Ethnocide?" 179–186.

came to self-identify as an “activist anthropologist,” described the competing draw of the more dramatic critiques of NGOS like Survival International in comparison to Maybury-Lewis’ more conservative approach saying, “As a young graduate student, I found myself caught between the two perspectives: the politicized perspective that pushed a spectacular analysis of indigenous affairs and the more measured findings of Maybury-Lewis and Howe.”⁴⁹² But Maybury-Lewis faced critics on both sides – those who thought his analyses were compromised by the fact that he and Howe were employed by USAID to produce the report and those who thought his activism precluded the possibility of responsible, objective practice of anthropology.

The Maybury-Lewis and Cultural Survival have regularly been recognized as an important precursor for later anthropological activism on the international stage, particularly in discussions of human rights based approaches to Indigenous peoples.⁴⁹³ It became a defining feature of David Maybury-Lewis’ career, and as he was memorialized

⁴⁹² Richard Reed, “At the Intersection of Scholarship and Activism,” 1067. See also: Reed and Renshaw, “The Aché and Guaraní,” 3.

⁴⁹³ Richard O. Clemmer, “Anthropology, the Indigenous and Human Rights: Which Billiard Balls Matter Most?” *Anthropological Theory* 14, no. 1 (2014): 93–94, doi:10.1177/1463499614528925; Mark Goodale, “Introduction to ‘Anthropology and Human Rights in a New Key,’” *American Anthropologist* 108 (2006): 7. doi:10.1525/aa.2006.108.1.1; Dorothy L. Hodgson, “Introduction: Comparative Perspectives on the Indigenous Rights Movement in Africa and the Americas,” *American Anthropologist* 104 (2002):1044, doi:10.1525/aa.2002.104.4.1037; Shannon Speed, “At the Crossroads of Human Rights and Anthropology: Toward a Critically Engaged Activist Research,” *American Anthropologist* 108 (2006): 75, doi:10.1525/aa.2006.108.1.66. Nancy Scheper-Hughes and Philippe Bourgois go as far as to cite “Maybury-Lewis’ Cultural Survival movement” as an example of “anthropological ideas and methods used as tools of human liberation in opposition to state projects of mass killing and genocide” (Nancy Scheper-Hughes and Philippe I. Bourgois, *Violence in War and Peace: An Anthology* [Blackwell Publishing, 2003], 8).

after his death in 2007, his activist stance became the most consistently cited aspect of his work.⁴⁹⁴

It is not an overstatement to say that the sense of moral obligation that the Maybury-Lewises felt to take action was formed with their earliest experiences in Xerente and Xavante territory. However, the Xavante people who they had worked with were mostly unaware of these advocacy activities. In Pimentel Barbosa village in 2015, residents remembered the Englishman and his family, both from their early ethnographic work and from Maybury-Lewis' later returns to the village, including the filming of the PBS special, *Millennium*.⁴⁹⁵ But they were unaware that he had founded and run an NGO, initially prompted by the time he spent with them. Large-scale advocacy work, from the ground, in the village, was difficult to perceive.

A "Xavantóloga Militante" in Brazil

The same year that the Maybury-Lewises established Cultural Survival, another anthropologist was just beginning her foray into the field. Aracy Lopes da Silva first ventured into Xavante territory with her colleague and friend Regina Aparecida Polo Müller in 1972 while completing undergraduate degrees in social sciences at the *Universidade de São Paulo*. They arrived in the T.I.s of São Marcos and Sangradouro at

⁴⁹⁴ Stephen Pincock, "David Maybury-Lewis" *The Lancet* 371, no. 9612 (2008): 554, doi:10.1016/S0140-6736(08)60260-3; Jeremy Pearce, "David Maybury-Lewis, Who Studied Native Tribes, Dies at 78," *The New York Times*, 14 December 2007, <http://www.nytimes.com/2007/12/14/us/14mayburylewis.html>; Lorand J. Matory, "David Maybury-Lewis: Anthropologist Keen to Protect the Interests of the Peoples of Central Brazil," *The Guardian*, February 5, 2008.

⁴⁹⁵ The ten part series was complemented by a coffee table book, and featured the Xavante in the first episode. See David Maybury-Lewis, *Millennium: Tribal Wisdom and the Modern World* (New York: Viking, 1992).

the Salesian Missions, planning to enquire into Xavante mythology for their master's theses. They soon found the subject matter outstripped their linguistic abilities, and while they each produced a thesis based on this field work, Lopes da Silva would redirect her attention during her doctoral work to address the question of Xavante naming practices and the institution of formal friendship.⁴⁹⁶ Under the supervision of Lux Vidal, Lopes da Silva began her doctoral research at the *Universidade de São Paulo* in 1974. She went to the field at a moment of repression, but also under General Ismarth de Araújo Oliveira's FUNAI. Lopes da Silva's career trajectory provides insight into how field experiences in Xavante territory reverberated back into the careers and institutions that anthropologists were building. In the context of military rule, Lopes da Silva exemplifies the tensions between conciliatory engagement and *militância*, between trying to make change through official channels and working to bring national and international pressure to bear on Indigenous rights in Brazil.

Lopes da Silva may have been exactly the generation of anthropologists that Ismarth de Araújo Oliveira had in mind when he called for more exchange between academic ethnologists and FUNAI. However, her development as a scholar was also consistent with her advisor Lux Vidal's assertion that anthropologists must privilege the welfare of their interlocutors over government rules or priorities. Like many other scholars of her generation, Lopes da Silva would benefit from increased government support of research, receiving funding for her doctoral work from public institutions

⁴⁹⁶ Aracy Lopes da Silva, "Relatório das atividades de pesquisa no P.I. Paraíso" 19 April 1974, Proc. FUNAI/BSB/2466/73, Fundo: FF-SEP-PQ, Caixa 582, Documento 9826, SEDOC, FUNAI; Müller, "A pintura do corpo e os ornamentos Xavante."

Fundação de Amparo à Pesquisa do Estado de São Paulo (FAPESP) and CNPq.⁴⁹⁷ She would also engage the government as a consultant, preparing a 1975 report on conditions in one Xavante territory, and briefly participating in a government sponsored development project in 1978. Finally, Lopes da Silva became an important figure in the shift towards anthropologists addressing pressing social issues, both through her work in a local NGO, and her pursuit of a scholarly career that would address both theoretical and practical issues for the communities she studied.

Even while she was conducting her master's and doctoral research, Lopes da Silva quickly included consultancies for the government in her fieldwork. In 1975 she spent a month in the Xavante community of Couto Magalhães, hired by FUNAI to document the conditions in the territory.⁴⁹⁸ What she saw distressed her. It was a moment of particular tension between the Xavante inhabitants and the neighboring ranch, *Fazenda Xavantina S.A.*⁴⁹⁹ She wrote to her contacts at FUNAI requesting a special audience with General Ismarth de Araújo Oliveira. She also proposed speeding up the timeline of her report, which she had a three-month contract to prepare: "The situation here is really very difficult and requires urgent measures," she wrote, "For this reason I hope to complete the survey in just one month ... That way, the project will be ready earlier and can be implemented in time to avoid conflicts that are more serious than those that have already occurred."⁵⁰⁰ In her subsequent publications she described her own intervention in fuller

⁴⁹⁷ Lopes da Silva, *Nomes e amigos*, 11.

⁴⁹⁸ Lopes da Silva, *Nomes e amigos*, 16.

⁴⁹⁹ Ibid. For a more in depth discussion of the conflict between Xavante communities in the area and *Fazenda Xavantina*, see Garfield, *Indigenous Struggle*, 152–186.

⁵⁰⁰ Aracy Lopes da Silva to Rubens C. Oliveira, DGPC-FUNAI, 9 December 1975, Fundo: FF-SEP-PQ, Caixa 582, Doc 9826, SEDOC, FUNAI.

detail, writing that she “sought to attend to the most urgent requests of the Xavante, portray the gravity of the tensions regarding land occupancy, and present ethno-historical data that attested to the right of the Indians to the territory that they claimed.”⁵⁰¹

Following this experience, Lopes da Silva would again consult for FUNAI on the “*Plano de Desenvolvimento da Nação Xavante*” (The Development Plan for the Xavante Nation) in 1978. Specifically, she was invited to work on the Xavante Project (as the program was informally known) as an advisor for education portion of the project.⁵⁰² Lopes da Silva was charged with preparing a curriculum for bilingual Xavante monitors who would assist in classrooms, but due to disagreements with the project managers and lack of consensus on the program she was developing, she left the project. While Lopes da Silva specified that during these periods working for FUNAI she was not engaged in research for her scholarship, she included the details of all the time she spent in the field, saying, “I list them all here because my intention is to paint as clear and complete a picture as I can of the experience that my accompanying of the Xavante reality afforded me.”⁵⁰³ As she moved forward with her career, Lopes da Silva increasingly sought to integrate her activism and her scholarship.

While still in her graduate training, Lopes da Silva joined a group of her colleagues in forming one of the earliest Brazilian NGOs dedicated to promoting Indigenous rights. The group founded the *Comissão Pró-Índio de São Paulo* (CPI-São Paulo, the Pro-Indian Commission) in 1978, the same year that other *Comissões Pró-*

⁵⁰¹ Lopes da Silva, *Nomes e amigos*, 16.

⁵⁰² On the Xavante Project and its many failures, see Garfield, *Indigenous Struggle*, 187–211.

⁵⁰³ Lopes da Silva, *Nomes e amigos*, 17. This passage bears a very close resemblance to Maybury-Lewis’ description of his time in the field, particularly regarding his conflict with the *chefe do posto* at Pimentel Barbosa.

Índio were formed in four other states.⁵⁰⁴ While the CPIs tended to focus mostly on government policy, other groups emerged to address legal issues, and yet others focused on carrying out community-based development projects. A few years earlier when the IWGIA and Cultural Survival were formed, local advocacy in Brazil was near impossible due to surveillance and repression by the dictatorship. With the *abertura*, the proliferation of multiple groups served several purposes. As Greg Urban has noted, the diversification of organizations proved an effective strategy in the context of the military dictatorship: it distributed advocacy activities regionally, allowed for specialization to address specific types of challenges, and avoided creating an antagonistic relationship between the federal government and one, large, centralized advocacy organization.⁵⁰⁵

Following her successful defense of her doctoral dissertation, Lopes da Silva spent a year as a visiting professor at Harvard with Maybury-Lewis.⁵⁰⁶ She would return to join the faculty at the *Universidade de São Paulo* in 1981, benefitting from the swift growth of the Department of Anthropology. After her extensive work in social organization, both through her Portuguese-language translation of Maybury-Lewis' *Akwẽ-Shavante Society*, and through her own doctoral research, Lopes da Silva turned to cultivate her interest in the theoretical and practical concerns of education.⁵⁰⁷ In doing so, she sculpted a career that would allow her both to engage with Native communities, and

⁵⁰⁴ Greg Urban, "Developments in the Situation of Brazilian Tribal Populations from 1976 to 1982," *Latin American Research Review* 20, no. 1 (1985), 8.

⁵⁰⁵ *Ibid.*

⁵⁰⁶ Vidal, "Maria Aracy de Pádua Lopes da Silva," 14.

⁵⁰⁷ David Maybury-Lewis, *A sociedade Xavante*, trans. Aracy Lopes da Silva (Rio de Janeiro: Francisco Alves, 1984). For a description of her offer to translate Maybury Lewis' work for her fellow students and the influence of this experience on her vocation as anthropologist, see Vidal, "Maria Aracy de Pádua Lopes da Silva," 13.

to work to improve education in non-Indigenous schools regarding issues that Native peoples faced so as to bolster public support for Indigenous communities. Lopes da Silva dedicated much of the rest of her career to these dual goals.⁵⁰⁸ She participated in public debates, proposed legislation, and, following the passage of the 1988 Constitution, she created a center for the study of Indigenous education at the *Universidade de São Paulo*.⁵⁰⁹ The organization, MARI, would become one of the most important research hubs for the anthropology of education.

Colleagues praised Lopes da Silva for the ease with which she combined her advocacy work and theoretical rigor.⁵¹⁰ Luís Donisete Benzi Grupioni, a colleague who collaborated with her on MARI, described her work in 2015 saying, “While reflection and activism often overlap and merge, one almost always compromising the other, Aracy Lopes da Silva’s academic production and activities in the area of indigenous education reveal that she knew, like few others, how to administrate them, weaving connections and

⁵⁰⁸ Lopes da Silva produced, among other things, a series of influential edited volumes. The earliest volumes helped formulate Brazilian laws regarding Indigenous education in the early period after redemocratization. See: Aracy Lopes da Silva, *A questão indígena na sala de aula: subsídios para professores de 1o. e 2o. graus* (São Paulo: Editora Brasiliense/ Comissão Pró-Índio de São Paulo, 1987) and Aracy Lopes da Silva, and Luís Donisete Benzi Grupioni, and Ana Vera Lopes da Silva Macedo, eds., *A temática indígena na escola: novos subsídios para professores de 1o. e 2o. graus* (São Paulo: MEC/UNESCO/MARI, 1995). A series of other interdisciplinary volumes would be published after Lopes da Silva’s untimely death, including chapters by many of the 31 students she mentored during her time at USP. See: Aracy Lopes da Silva and Mariana K. Leal Ferreira, *Antropologia, história e educação: a questão indígena e a escolar* (São Paulo: Global Editora, 2001); Aracy Lopes da Silva and Mariana K. Leal Ferreira, *Práticas pedagógicas na escola indígena* (São Paulo: Global Editora, 2001); Aracy Lopes da Silva, Ângela Nunes, and Ana Vera Lopes da Silva Macedo, *Crianças indígenas: ensaios antropológicos* (São Paulo: MARI, 2002).

⁵⁰⁹ For a description of Lopes da Silva’s interventions with the National Education Plan, see Luís Donisete Benzi Grupioni, “Reflection and Activism: Aracy Lopes Da Silva and the National Education Plan,” trans. David Rogers, *Vibrant* 12, no. 2 (2015): 572–78. For the plan itself, see Aracy Lopes da Silva, “National Education Plan: Indigenous School Education,” trans. David Rogers, *Vibrant* 12, no. 2 (2015): 579–587.

⁵¹⁰ Vidal, “Maria Aracy de Pádua Lopes da Silva,” 14; Luís Donisete Benzi Grupioni, “Aracy Lopes da Silva,” *Revista de Antropologia* 43, no. 2 (2000): 17–20.

interfaces where appropriate, preserving open spaces when necessary.”⁵¹¹ Grupioni’s words suggest that debates and concerns about how best to combine theory and applicability were by no means absent in a context where anthropologists were regularly called upon to opine about government programs, laws, land demarcations, and even legal cases.⁵¹² However it also reflects how comfortably political action and theoretical scholarship fit together in Brazilian anthropology. Lopes da Silva’s interactions with FUNAI—both those she might have termed a success, like the report she registered for Couto Magalhães, and those that clearly failed, like her abbreviated engagement in the Xavante Project—fell naturally within her purview as a doctoral student and as a professor. This comingling of “reflection and activism” as Grupioni described it—the appropriate combination of theory and praxis—oriented towards advocacy became a gold standard for the field of ethnology in Brazil.

While colleagues praised both Lopes da Silva and Maybury-Lewis for their political action, the vision of the pragmatic and theoretical that Lopes da Silva combined in her career was valorized in Brazil as an embodiment of an ideal for the profession. While Lopes da Silva’s work took her back, again and again, to Xavante territory to implement programs in the field, Maybury-Lewis’ international advocacy was invisible to the people whom he credited with its motivation.

⁵¹¹ Grupioni, “Reflection and Activism,” 572.

⁵¹² For a relatively recent discussion of some of the pitfalls of these multiple roles, see João Pacheco de Oliveira, “The Anthropologist as Expert: Brazilian Ethnology between Indianism and Indigenism,” in *Empires, Nations, and Natives: Anthropology and State-Making*, eds. Benoît de L’Estoile, Federico Neiburg, and Lygia Maria Sigaud (Durham: Duke University Press, 2005), 223–47.

Field Realities under Military Rule

The work of militant anthropologists and international activists inevitably reverberated into the daily practice of fieldwork in Xavante territory. Now I turn back to the story that opened this chapter. Laura Graham's arrival in the field raised a number of questions about the place of anthropologists in relation to military rule. To illustrate these questions in practice, I trace this relationship back to the field and the implication of this positioning for research with the Xavante. Graham's difficulties with local FUNAI employees provide insight into government suspicions about foreign researchers. However, they also demonstrate the heterogeneity of government responses, and the power of prominent anthropologists' voices with government officials, in this case valued over local FUNAI functionaries' grievances.

When Graham made her first trip to Mato Grosso, she was a master's student on a yearlong stay at USP under the supervision of Vidal and Lopes da Silva. In her request for authorization from FUNAI, Graham articulated the relevance of the project for government interests writing, "For the Xavante, currently in a rapid process of integration and adaptation to surrounding society, the analysis of their communication patterns is of utmost importance to understand practices that can promote social cohesion during a time of rapid change. ... Thus, this study will lead to understandings of how the Xavante confront the process of change, in order to achieve an adequate integration into national society."⁵¹³ One FUNAI functionary annotated the proposal, drawing a box around the first sentence and underlining the last; these justifications would be included in the

⁵¹³ Laura R. Graham, "Estudo Linguístico: Etnografia do falar entre os índios Xavante, Mato Grosso, Brazil," Fundo: FF-SEP-PQ, Caixa 588, Doc 9917, SEDOC-FUNAI.

evaluation of the proposed work. Graham, like her advisors in both Brazil and the United States, had to provide concrete reasons for why her project could be of benefit not only to scholarly audiences, but to the government agency still working within an integrationist framework.

Graham was granted permission to begin her study in October of 1981. After spending a number of weeks in Kuluene (now T.I. Parabubure), she moved on to T.I. Pimentel Barbosa where she hoped to compare her initial observations in a second village, less influenced by surrounding Brazilian society.⁵¹⁴ Upon her arrival, just four years after Nancy Flowers had finished her doctoral fieldwork, Graham was unaware of how tense relations were between the Xavante and both the surrounding *fazendeiros* and government employees at the post.⁵¹⁵

Graham had been in the village for less than ten days when the *chefe do posto* sent the radiogram to his superiors in the regional office complaining of her behavior. He accused her of a litany of offenses: “Following the arrival of the aforementioned in this Reserve, our work in this PI [Indigenous post] became difficult. Before, the *Indigenas* were working in diverse sectors of work and peacefully evicting the *Fazendeiros* still occupying the Area, and on the day after her arrival we had the attempted attack against Sr. Diogo’s *Fazenda*.”⁵¹⁶ She was temporarily forced to leave Xavante territory, while the local functionaries built their case with statements from the post’s employees and other

⁵¹⁴ Laura R. Graham, “The Always Living: Discourse and the Male Lifecycle of the Xavante Indians of Central Brazil,” (PhD diss., University of Texas–Austin, 1990), 25. See also, Graham, *Performing Dreams*, 37.

⁵¹⁵ Graham, *Performing Dreams*, x.

⁵¹⁶ José Ubaldino Veiga, Chefe Posto PI Rio dos Mortes, to AJABAG, “Relatório,” 27 January 1982, Doc: PSS 395, Fundo: ASI-FUNAI, Arquivo Nacional.

locals in the nearby town for the central office in Brasília. In addition to apparently inciting the Xavante to forcibly evict the *warazú* at a neighboring *fazenda*, Graham was accused of nearly causing a split in the village, of spreading gossip about FUNAI employees, fomenting resistance to the *chefe do posto*'s work program for the Xavante, moving into the village without authorization, and of convincing the Xavante that she was Xavante, too.

FUNAI employee Luis Barbosa Luz described Graham as a foreign subversive in his statement, saying “[*Senhora Laura*] took the *índio* Supto and told him that they were free and they should not be dominated or ordered around by someone like public servant José Ubaldino Veiga. She even said that if she was not given enough support to complete her work or fulfill other interests, she would go back to her country and publicize how the Brazilian Indians are being treated.”⁵¹⁷ The threat of international pressure, which had caused so much difficulty for the military regime at the national and international level, was seen as menacing even from the *posto indígena*. Or at the very least, this discourse was expected to be a compelling indictment of a foreign researcher in the eyes of superiors in the capital.⁵¹⁸ Another employee finished her statement to the authorities saying that Graham “was already saying that she was of their race, that is to say, Xavante, and it was she who introduced that idea into their heads...”⁵¹⁹ This employee echoed the *chefe do posto*, who wrote, “In my understanding, she exercises great power and control

⁵¹⁷ Luis Barbosa Luz to AJABAG, “Declaração,” 19 February 1982, Fundo FF-SEP-PQ Caixa 588, Doc 9917, p. 104, SEDOC-FUNAI.

⁵¹⁸ I am inclined to think that parts of this statement were fairly accurate, and that Graham likely did express to villagers that she thought they were being mistreated by the *chefe do posto*.

⁵¹⁹ Maxilene Carvalho de Souza to AJABAG, “Declaração,” 19 February 1982, Fundo FF-SEP-PQ, Caixa 588, Doc 9917, SEDOC-FUNAI.

over the *Indigenas*, who venerate her ideas with facility, to the point of saying she too is a Xavante Indian.”⁵²⁰ Graham’s threat, at least as described by the local FUNAI employees, was to disrupt the peace within the *Terra Indígena*, leading to a lack of respect for the systems that Ubaldino had put in place to render the reserve a productive space under his control, and for his profit.⁵²¹ The accusation that she was claiming to be Xavante seems to suggest that her acceptance and close alliance with the community was also seen as a hazard.

Graham protested vehemently with a series of statements that outlined her arrival in the village. She explained that she had been introduced to the *cacique* and his *secretários*, met with elders, suggested that her presence be discussed in the *warã*, and then received an invitation to move into the village. She proceeded to explain how one night, early in her stay, Ubaldino had made advances on her. They had been sitting and talking about what she had learned about Xavante in her time in Patrimônio, when, “He did not want to go back to his house, because his wife had gone to Barra do Garças for treatment for a health problem and he wanted to sleep with me. I said that I did not want to and also did not think it was right, since he was the *chefe do posto* and I was a student. Then he said, ‘forget about our roles here in the area,’ and I told him to leave immediately.”⁵²² Graham went on to describe, in detail, her interactions in the village,

⁵²⁰ Veiga to AJABAG, “Relatório,” 27 January 1982.

⁵²¹ This particular *funcionário público* was accused by the Xavante of corruption, specifically orchestrating unfavorable interactions with surrounding *fazendeiros*. For a discussion of this history, see James R. Welch, Ricardo Ventura Santos, Nancy M. Flowers, and Carlos E. A. Coimbra Jr., *Na primeira margem do rio: território e ecologia do povo Xavante de Wedezé* (Rio de Janeiro: Museu do Índio/FUNAI, 2013), [introduction].

⁵²² Laura Graham to AJABAG, “Declaração,” 1 February 1982, Doc: PSS 395, Fundo: ASI-FUNAI, Arquivo Nacional.

explaining the villagers' discussion regarding her presence and the invitation they made to her to stay in the village rather than at the post.

The documentation regarding Graham's case does not clearly account for the decision-making process to allow her to continue her work. The five weeks following her expulsion from the T.I. created a flurry of telegrams, official declarations, and letters. The majority of these came from Ubaldino's allies, alleging that her presence destabilized FUNAI's work in the T.I. or supporting his assertions that Graham was spreading *fofoca* or gossip that was turning the villagers against the post employees.

Graham credited her eventual permission to return to the efforts of the Brazilian anthropologists who had such experience engaging with the military government regarding Indigenous issues. "After over a month of appeals on my behalf by Brazilian anthropologists and the Brazilian Anthropological Association," Graham wrote in her dissertation, "FUNAI Brasília granted me permission to return."⁵²³ She specified in a footnote, "I owe special thanks to Lux Vidal and Eunice Durham, then president of the Brazilian Anthropological Association, for their efforts on my behalf."⁵²⁴ The same anthropologist who had engaged General Ismarth de Araújo Oliveira in the *Conselho Indigenista* with critiques of FUNAI's failings and claims to excellence in training anthropologists intervened on behalf of Graham.

Another interesting aspect regarding the eventual approval for her return, which came from the President of FUNAI directly, relates to a trip that Warodi and Surupredu made to Brasília during the time Graham was waiting, stuck in Barra do Garças. The

⁵²³ Graham, "The Always Living," 29.

⁵²⁴ Ibid.

regional office advised the president of the trip by telegraph, blaming the anthropologist for “possibly having convinced the *índios* to travel.”⁵²⁵ Three days later Graham was granted permission to return.⁵²⁶ It is unclear what motivated the two Xavante leaders to visit Brasília, or even if they would have arrived in the capital by the time of the re-authorization, but the timing is suggestive that among their activities they may have advocated for the social scientist’s return.

In her dissertation, Graham reflected on the impact of these clashes with FUNAI on her relationship with the community, writing, “Aside from the difficulties I had with the Post Chief’s few acolytes, the ordeals I had survived with FUNAI had positive repercussions for my relations with the rest of the community. In fact, they cemented my relationship with Warodi.”⁵²⁷ For example, she understood the struggle to assert her independence from the *chefe do posto* as part of the motivation for her incorporation into Warodi’s family upon her return to the village. “To keep me within the village’s jurisdiction,” she wrote, “and so that I would not have to deal with the hostile Post Chief, Warodi invited me to become a member of his household. He also adopted me as his daughter. That evening Warodi announced his decision in the men’s council which I attended. I no longer felt vulnerable, but safe and most welcome in the community.”⁵²⁸ Graham’s close relationship with Warodi would become central to her experience in T.I. Pimentel Barbosa and to her resulting scholarship. Similar to later researchers, Graham’s

⁵²⁵ AJABAG to President of FUNAI, telegram, 28 February 1982, Fundo FF-SEP-PQ, Caixa 588, Doc 9917, SEDOC-FUNAI.

⁵²⁶ AJABAG to PI Rio das Mortes, telegram, 1 March 1982, Fundo FF-SEP-PQ, Caixa 588, Doc 9917, SEDOC-FUNAI.

⁵²⁷ Graham, “The Always Living,” 30.

⁵²⁸ *Ibid.*, 30-31.

feeling of safety and inclusion were related to Warodi's work to build connections. As their relationship developed, Warodi not only adopted Graham as his daughter, but also understood her as someone who could help him with his own political and spiritual goals. Graham clearly recognized and articulated both the importance of her relationship with Warodi in shaping her work, and the potential that he began to see in their interactions, in the introduction to her first monograph. "Most of the choices I made involved Warodi in one way or another," she wrote, "His own personal, spiritual, and political objectives and the ways in which he endeavored to accommodate these through my presence in the community influenced the course of my research and ultimately my current understanding of Xavante and Xavante worldviews."⁵²⁹

Graham's relationship with the community at Pimentel Barbosa was formed in a moment of conflict. It was made possible by the support and intervention of Lopes da Silva and Vidal in São Paulo, in the context of the critical but pragmatic approach they took to collaborating with FUNAI under military rule. Graham's stay was also possible due to the disjuncture between local staff members' interests and the interests of the larger institution of FUNAI, suggesting a tenuous conciliatory streak within the bureaucracy of the capital. Finally, in her interactions with Warodi and his nephews Caimi and Jurandir, Graham would establish relationships that would compel her to direct her future work towards community concerns. Beyond becoming a vehicle for Warodi's dream and for the continuation of his voice after his life ended, Graham's future scholarship would include a documentary film on a struggle for water rights, and

⁵²⁹ Graham, *Performing Dreams*, 9.

she would direct her activism along lines that Maybury-Lewis had laid, becoming a member of the board of Cultural Survival.⁵³⁰

Conclusion

“It is not by accident that the photos selected for the cover of our association’s newsletter more often than not feature scenes of life in indigenous societies,” Mariza Corrêa wrote, in 1995.⁵³¹ Considering the influence of studies of ethnology on the field and public image of anthropology she highlighted that the only effective lobby of anthropologists during the Constituent assembly was on behalf of Indigenous groups, and that ethnological research is often the most highly celebrated scholarship in the field. She continued, “As it has been the field in which our discipline has best demonstrated its capacities and most clearly defined its profile, it is only just that it be so.”⁵³² The best demonstration of capacity, in Corrêa’s estimation was not simply a question of excellence in scholarship. Theoretical concerns and practical applicability must be combined.

As comparisons of the careers of Maybury-Lewis, Lopes da Silva, and Graham suggest, *militância* in the Brazilian case has had a very different reception from applied anthropology and anthropological activism as practiced elsewhere in the world. While academic anthropologists in the United States tended to view applied anthropology with suspicion, engagement with and opposition to military rule comingled liberally in 1970s and 1980s Brazil. Under military rule, practitioners of anthropology institutionalized their

⁵³⁰ Laura R. Graham, Caimi Waiassé Xavante, and David Hernández Palmar (directors), *Owners of the Water: Conflict & Collaboration Over Rivers*, (DER Documentary films, 2008), DVD.

⁵³¹ Corrêa, “A antropologia no Brasil 1960–1980,” 104.

⁵³² Ibid.

field. They did so at a moment of profound antagonism, articulating a politics of opposition. However, they tempered discourse with pragmatic engagement, incorporating political advocacy into the heart of the newly professionalized discipline.

Chapter Five

Xavante Affective Labor

Geneticist Fabrício Santos rushed through his words as he told me about his experience of fieldwork with the Xavante. From his office at the *Universidade Federal de Minas Gerais*, he wove an entrancing story of his time in the village of Etênhiritipá. His eyes shone as he recounted a hunting trip, stargazing, and a movie night; I was fascinated. Rather than focusing on days filled with collecting genealogical data and genetic samples, Santos' narrative centered on "the most interesting part," what he called "the anthropological experience."⁵³³ Santos' tale did not fit with my preconceptions of what genetic sampling for the Genographic Project might look like. But perhaps I should not have been as surprised as I was. A number of his colleagues had mentioned to me both before and after I interviewed the geneticist, "Fabrício *ficou encantado*," with the Xavante. He was enchanted; he fell under the spell.

This chapter explores how researchers "*ficam encantados*" in the context of twenty-first century fieldwork, and how that "*encanto*" sometimes evolves into more substantial forms of engagement. From the earliest interactions, from the days of observing the strange habits of Maybury-Lewis, Xavante approaches to researchers have become increasingly sophisticated and self-conscious.⁵³⁴ In twenty-first century

⁵³³ Fabrício Rodrigues dos Santos, interview with Rosanna Dent, 6 March 2014, Belo Horizonte.

⁵³⁴ I have been able to document this most thoroughly for communities in *Terra Indígena Pimentel Barbosa*. While I posit this is true for residents of other Xavante territories, particularly those of São Marcos and Sangradouro, who have also received numerous researchers, the data presented in this chapter focus exclusively on Pimentel Barbosa.

fieldwork, researchers and research subjects alike interact aware of the ethical and moral stakes of their projects of representation. Although perhaps to differing degrees, researchers are aware of the major controversies that have erupted over research in Indigenous territories in Brazil and in the Americas more broadly.⁵³⁵ Anthropologists and historians, trained in critiques of the colonial and neocolonial imbrications of social science research, may carry feelings of guilt with them into the field—as I did—which can influence how they engage and what they learn.⁵³⁶ With their extensive experience of hosting outsiders, Xavante interlocutors also bring expectations—memories of prior harm and benefit—that modulate interactions. All participants test out relationships, trying to make sense of what we can offer and what we can get in return. We are all equally exploring and performing in these interactions. However, these performances are firmly situated in substantial differentials in access to wealth and mobility. Solidarity on the one hand, and power, inequality, and competition, on the other, are two sides of agency, as Sherry Ortner has highlighted.⁵³⁷ This chapter illustrates how Xavante interlocutors in T.I. Pimentel Barbosa have refined a system of interaction that creates a sense of affection, belonging, and obligation among researchers. Building on Chapter 4’s discussion of the political import of long-term engagement with researchers and changing

⁵³⁵ On the Yanomami controversy, the literature is extensive. For examples, see Borofsky et al., *Yanomami: The Fierce Controversy*; Débora Diniz, “Avaliação ética em pesquisa social: o caso do sangue Yanomami,” *Revista Bioética* 15, no. 2 (2007): 284–97. On the Karitiana and Sururí immortal cell lines, see Ricardo Ventura Santos and Carlos Coimbra Jr., “Sangue, bioética e populações indígenas,” *Parabólicas* 3, no. 2 (1996). On the material production of these controversies in historical context, see Radin, *Life on Ice*.

⁵³⁶ For an interesting comment on guilt and the anthropologist’s body as instrument, see Eric Gable, “The Anthropology of Guilt and Rapport: Moral Mutuality in Ethnographic Fieldwork,” *HAU: Journal of Ethnographic Theory* 4, no. 1 (2014): 237–58, doi:10.14318/hau4.1.010.

⁵³⁷ Sherry B. Ortner, *Anthropology and Social Theory: Culture, Power, and the Acting Subject* (Durham: Duke University Press, 2006), 138–139.

norms of anthropological fieldwork in the 1980s and 1990s, this chapter turns to the affective experience of fieldwork.

The affective labor that Xavante community members commit to building research relationships is part of a larger, future-oriented strategy of engagement with outsiders. Xavante actors cultivate relationships by combining performances of identity with mobilization of kinship and gift exchange. These forms of *warazú* enrollment apply as much to researchers as to other outsiders that visit—whether participating in film or music production, social assistance projects, or other modes of sustained engagement—whom Xavante actors perceive as possible long-term supporters. However, as prior chapters have suggested, the Xavante distinguish between researchers and other outsiders early in an encounter. Building on this evidence, I suggest that researchers constitute a special category of outsider from Xavante perspectives. Xavante actors see researchers like other kinds of non-governmental *warazú*, as potential allies to address community interests. Simultaneously, community leaders see scientists and other scholars as capable of producing knowledge that will circulate nationally and internationally and that will hold particular authority in these circulations. As researchers represent knowledge creators of such far reach, the Xavante oversee them with great care. In a context of highly unequal access to material wealth, publication venues, and social and political clout, the cultivation of research relationships is a dynamic way for the Xavante to influence what knowledge is produced and how that knowledge is mobilized for political ends.

Furthermore, this chapter traces the development of the Xavante systems of affective engagement in T.I. Pimentel Barbosa. Since David Maybury-Lewis' first visit to

the community in 1958, the Xavante have gradually established a flexible system to manage research. Villagers have adapted existing institutions to interface in new ways with academic researchers, creating systems that endure beyond a single research protocol and apply to interactions with researchers regardless of their disciplinary orientation, institutional affiliation, or membership in a research team. This approach has its roots, I posit, in previous experience with researchers, and the consistent challenges to health and land that the community faces. Working with three primary examples—the Genographic researchers, the work of public health researchers from the *Escola Nacional de Saúde Pública*, and my own experiences collaborating on an archive project in 2015—I lay out below how Xavante affective labor and systematization of managing researchers constructs and maintains research subjectivities.

Affect and Emotion in the History of Fieldwork

In this chapter, I work with the concept of affect for two primary reasons. First, I choose this term to emphasize the connections between bodily experience, internal realities, and rational processes. Secondly, as conceptualized in recent approaches to affect theory, attention to affect emphasizes the dynamic relation between experience and action in contrast to static, culturally defined categories of emotions. This connection between being affected and taking action is essential to my understanding of how Xavante interlocutors engage scientists in the twenty-first century and how they work to shape research subjectivities in particular ways.

Considering affective states not only provides texture for humanistic accounts of scientists' lives and work, it also informs our understandings of scientific practice (how

science is actually done) and of the moral meaning that scientists extract from their work.⁵³⁸ As other scholars have pointed out, interest in the relationship between cognitive processes and emotional states in the history of science date back to the work of Ludwig Fleck, who described emotions as essential both to thought styles and scientists' epistemic interests.⁵³⁹ Going further, Paul White has argued that emotions are agents, integral to "the practices of observation, experiment, and theory and, reciprocally, the practices of the self."⁵⁴⁰ Feminist approaches to the history of science pioneered attention to the role of emotional, embodied experience as they problematized binaries such as mind/body, reason/emotion, and male/female.⁵⁴¹ Other approaches to emotion have emphasized the sociality inherent in emotional experience.⁵⁴² Historians of science are

⁵³⁸ Ludwig Fleck considered the relationship between emotions and knowledge production in his 1935 work: Ludwik Fleck, *Genesis and Development of a Scientific Fact*, ed. Thaddeus J. Trenn and Robert K. Merton, trans. Frederick Bradley (Chicago: University Of Chicago Press, 1981).

⁵³⁹ *Ibid.*, 38–51, esp. 49. On Fleck and his interest in emotions, see also Dror, Hitzer, Laukotter, and León Sanz, "An Introduction to *History of Science and the Emotions*," 8, 14.

⁵⁴⁰ Paul White, "Introduction: The Emotional Economy of Science," *Isis* 100, no. 4 (2009): 793, doi:10.1086/652019. Anthropologists have often documented and weighed both their emotional responses and their performances of emotion in the field in their daily field notes. This reflexive practice is also a process of self-formation (sometimes not an entirely pleasant experience). For a particularly eloquent reflection on the importance of emotional experience for ethnographic insight, see Renato Rosaldo, *The Day of Shelly's Death: The Poetry and Ethnography of Grief* (Durham: Duke University Press, 2014). With regard to historical approaches to field notes, Bashkow discusses the embarrassment that Clyde Kluckhohn felt upon reading his student David Schneider's personal journal-like fieldnotes, which were filled with descriptions of his emotional state. See Ira Bashkow, "The Dynamics of Rapport in a Colonial Situation: David Schneider's Fieldwork on the Islands of Yap," in *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*, ed. George W. Stocking Jr. (Madison: University of Wisconsin Press, 1993), 170–242.

170. On Maybury-Lewis and Flowers' emotional experiences in the field, refer to Chapter 1.

⁵⁴¹ Some of the earliest and most pioneering works on the role of emotions in the history of science include Evelyn Fox Keller, *A Feeling for the Organism: The Life and Work of Barbara McClintock* (San Francisco: Freeman, 1983); Donna J. Haraway, *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1990); Alison M. Jaggar, "Love and Knowledge: Emotion in Feminist Epistemology," in *Gender/Body/Knowledge: Feminist Reconstructions of Being and Knowing*, ed. Alison M. Jaggar and Susan R. Bordo (New Brunswick: Rutgers University Press, 1987).

⁵⁴² See Sara Ahmed, *The Cultural Politics of Emotion* (New York: Routledge, 2004).

increasingly inquiring into both the emotional experience of doing science and the history of scientific research into emotions; it is the former that is of most relevance here.⁵⁴³

Recent work in the social sciences has drawn on theorists from Baruch Spinoza to Gilles Deleuze and Félix Guattari to develop critical approaches to the study of affects.⁵⁴⁴ This work builds on long-standing feminist critiques of the body/mind dichotomy, which emphasize the lived experience of the body as essential, even foundational, for rational processes. While later scholars would problematize Simone de Beauvoir's approach to male and female embodiment, her observation that "to be present in the world implies strictly that there exists a body which is at once a material thing in the world and a point of view towards the world," served as a starting point for explorations of the inter-relations between bodily experience and the acquisition of knowledge.⁵⁴⁵

With my conceptualization of affect, I follow Michael Hardt, who has written that affects "refer equally to the body and the mind," and "involve both reason and

⁵⁴³ Of particular relevance are a 2009 special focus section in *Isis* and the 2016 volume of *Osiris*. See these two introductory essays, and the pieces they introduce: Paul White, "Introduction: The Emotional Economy of Science," *Isis* 100, no. 4 (2009): 792–97; Otniel E. Dror, Bettina Hitzer, Anja Laukötter, and Pilar León-Sanz, "An Introduction to History of Science and the Emotions," *Osiris* 31, no. 1 (2016): 1–18.

⁵⁴⁴ Many affect theorists build off of Gilles Deleuze and Félix Guattari's *A Thousand Plateaus: Capitalism and Schizophrenia*, trans. Brian Massumi (Minneapolis: University of Minnesota Press, 1987).

⁵⁴⁵ Simone de Beauvoir, *The Second Sex* (New York: Knopf, 1953), 39. There is a rich literature in the history of science that centers bodily experiences in the processes of knowledge production, whether in the field or in the lab. For a relatively early set of examples, see Christopher Lawrence and Steven Shapin, eds., *Science Incarnate: Historical Embodiments of Natural Knowledge* (Chicago: University of Chicago Press, 1998). Janet Browne's biography of Charles Darwin was pioneering in its attention to bodily states and their influence over the scientists' life and work. See Janet Browne, *Charles Darwin: A Biography, Vol. 1 – Voyaging* (Princeton University Press, 1996). As Kuklick explores, from the days of Malinowski and Rivers, the anthropologists' body was understood as the primary "instrument" of anthropological inquiry; see Kuklick, "Personal Equations." In a particularly interesting recent ethnographic exploration, anthropologist and STS scholar Natasha Myers has examined the "body-work" lab scientists use to understand protein folding in three dimensions; see Natasha Myers, *Rendering Life Molecular: Models, Modelers, and Excitable Matter* (Durham: Duke University Press, 2015). Also influential for my thinking is Nancy Scheper-Hughes' and Margaret M. Lock's "The Mindful Body: A Prolegomenon to Future Work in Medical Anthropology," *Medical Anthropology Quarterly* 1, no. 1 (1987): 6–41.

passion.”⁵⁴⁶ Drawing on Spinoza’s parallel between the mind’s power to think and the body’s power to act, Hardt suggested that affects “illuminate, in other words, both our power to affect the world around us and our power to be affected by it, along with the relationship between these two powers.”⁵⁴⁷ Affect draws attention to the complex play of human connection and political action that results from research interactions in the field. While emotions generally refer to feelings that emerge as culturally recognized categories—fear, grief, love, or anger—with corresponding values, affect refers to a field of interaction.⁵⁴⁸ As I explore below, when Xavante actors perform the affective labor of adopting a researcher into their kinship system, for example, I do not claim that they are purposely cultivating emotions of love or gratitude. Rather, they shape a field of engagement in which research subjectivities develop. As opposed to suggesting that Xavante individuals purposefully elicit predetermined emotional experiences in their *warazú* visitors, my use of “affect” emphasizes the dynamic processes of human relations.

The Xavante Genographic

The Genographic Project was not a simple endeavor—scientifically, logistically, or politically. A large-scale initiative sponsored by National Geographic and IBM to

⁵⁴⁶ Michael Hardt, “Forward: What Affects Are Good For,” in *The Affective Turn: Theorizing the Social*, ed. Jean Halley and Patricia Ticineto Clough (Durham: Duke University Press, 2007), ix.

⁵⁴⁷ Ibid. Sara Ahmed’s discussion of the sociality of emotion and the reflexive and self-producing quality of emotions are of great relevance here: Ahmed, *The Cultural Politics of Emotion*, 8–10. She writes that she seeks, “to avoid making analytical distinctions between bodily sensation, emotion and thought as if they could be ‘experienced’ as distinct realms of human ‘experience’” *ibid.*, (6).

⁵⁴⁸ For a recent synthetic discussion of anthropological approaches to emotion and affect across the four fields, see Elizabeth Lewis and Kathleen Stewart, “Anthropology of Affect and Emotion,” in *International Encyclopedia of the Social & Behavioral Sciences*, ed. James D. Wright (Elsevier, 2015), 236–40.

track pre-historic human migration, the initiative positioned Indigenous genes as a window into migratory and evolutionary history. Like similar initiatives in the past, it depended on collecting samples from as many Indigenous groups as possible.⁵⁴⁹

A number of Native activists and social scientists objected to the premises of the project, citing the fraught Human Genome Diversity Project (HGDP) of the early 1990s, and a long history of scientific abuses of Indigenous subjects.⁵⁵⁰ Social scientists and historians have situated the Genographic project within a longer trajectory of human biology, highlighting continuities with previous research agendas from the 1960s and 1990s.⁵⁵¹ The most recent scholarly critiques have focused particularly on the use of

⁵⁴⁹ The Genographic Project is one in a long line of transnational research programs that have emphasized the scientific value of biosamples from Indigenous peoples. For an overview of the Genographic for a public audience, see “The Genographic Project by National Geographic – Human Migration, Population Genetics,” *Genographic Project*, accessed 15 January 2017, <https://genographic.nationalgeographic.com/>. For the initial conception of the Human Genome Diversity Project (HGDP), which shared key scientific goals with the later Genographic, see L Luca Cavalli-Sforza, Allan C. Wilson, Charles R. Cantor, Robert M. Cook-Deegan, and Mary-Claire King, “Call for a Worldwide Survey of Human Genetic Diversity: A Vanishing Opportunity for the Human Genome Project,” *Genomics* 11, no. 2 (1991): 490–491. Spencer Wells’s account of the project was published for a popular audience. See Spencer Wells, *Deep Ancestry: Inside The Genographic Project* (Washington, D.C.: National Geographic, 2007).

⁵⁵⁰ One of the most active groups in opposing the project was the Indigenous Peoples Council on Biocolonialism. The Council members created multiple press releases regarding their opposition to the project, available through their website; see Indigenous Peoples Council on Biocolonialism, “Human Genetics Issues,” *Indigenous Peoples Council on Biocolonialism*, accessed 15 January 2017, http://www.ipcb.org/issues/human_genetics/index.html. Their opposition continued a series of earlier efforts to oppose the Human Genome Diversity Project. See Reardon, *Race to the Finish*, 2 and 205. For an extensive discussion and critique of the Genographic project, including attention to changing priorities and approaches as the project developed, see Kim TallBear, *Native American DNA*, especially 149–176. TallBear also discusses a series of objections to the Genographic raised by NGOs and communities in Peru, which are highly pertinent, but would have benefited from deeper contextualization in the complex terrain of Indigenous organizations, NGOs, and Peruvian politics; see 189–197. Also see Catharine Nash, “Genetics, Race, and Relatedness: Human Mobility and Human Diversity in the Genographic Project,” *Annals of the Association of American Geographers* 102, no. 3 (2012): 1–18.

⁵⁵¹ Jenny Reardon, Kim TallBear, Joanna Radin, and others have explored the genealogy of the Genographic in the context of the 1960s Human Adaptability Arm of the International Biological Program (Radin), the Human Genome Project and the Human Genome Diversity Project (Reardon), and broader attempts to study, characterize, and so construct “Native American DNA” (TallBear). Reardon provided an in-depth discussion of the HGDP, arguing that the lack of recognition of the moral, political, and social dimensions of genetic research on the part of the HGDP made the task of conducting the research nearly

Indigenous samples to inform non-Indigenous knowledge systems.⁵⁵² In Brazil, journalists also picked up on the contested nature of the project. They cast the initiative as a second *Projeto Vampiro*, citing the nickname of the HGDP. They drew comparisons to other controversial scientific endeavors, including the collection and storage of Yanomami blood, and the use of biosamples from Karitiana and Sururí people to create immortal cell lines for research.⁵⁵³

Scientists from the Genographic, including Santos as Coordinator for the South American arm of the project, responded highlighting the lessons learned from the HGDP and stressing that the Genographic had been planned to avoid these same pitfalls. Specifically, the scientists emphasized a few major differences that set the Genographic apart from the HGDP: it focused exclusively on migration; the researchers did not collect medical or clinical data; the project did not include the construction of a biobank populated with immortal cell lines; and, in South America at least, DNA would be collected via cheek swabs rather than blood samples. Finally, the project would focus

impossible; see Reardon, *Race to the Finish*. On the deeper historical and technological roots of these broad, global initiatives, see Joanna Radin, *Life on Ice* and “Latent Life.”

⁵⁵² TallBear and Reardon also offer a pointed critique on this asymmetric knowledge production. They have argued that investigations of prehistoric migration fit with a longer tradition of white claims on Native property, since they aim to expand or enrich origin stories that pertain to a Western scientific tradition but do not contribute to Native cosmologies. In this case of the Genographic project, Indigenous genes were needed to illuminate Western world-views. See Jenny Reardon and Kim TallBear, “‘Your DNA Is Our History’: Genomics, Anthropology, and the Construction of Whiteness as Property.” *Current Anthropology* 53, no. S5 (2012): S233–45. Francisco Salzano dismisses this argument by saying that Reardon and TallBear do not define whiteness and hence adopt “a typological racial reasoning that they are trying to condemn.” He further asserts, “what genome scientists are trying to obtain is a history of humankind in general, not of only one ethnic group”; see Francisco M. Salzano, “Bioethics, Population Studies, and Geneticophobia,” *Journal of Community Genetics* 6, no. 3 (2015), 199, doi:10.1007/s12687-014-0211-3. Salzano does not accept the social scientists’ view that there are multiple ontologies, of which Western science is one that is not universally applicable.

⁵⁵³ Marcelo Leite, “Projeto Genográfico e ‘Projeto Vampiro’,” 17 April 2005, Folha de São Paulo; Maria Amparo Lasso, “Índigenas em guarda ante o projeto Genográfico,” accessed 8 October 2016, <http://www.adital.com.br/site/noticia2.asp?lang=PT&cod=16334>.

exclusively on mitochondrial and Y-chromosome DNA. The affiliated researchers also cited the creation of a special fund as part of the project, meant to support cultural conservation and community proposals for revitalization projects.⁵⁵⁴

Despite scientists' efforts to dispel concerns about the project, the high-profile critiques and the doubts of social scientists in Brazil led to extensive vetting and many rounds of ethical review of the Genographic project at the national level.⁵⁵⁵ As a result,

⁵⁵⁴ These differences between the Genographic project and the HGDP are emphasized on the Genographic website, "The Genographic Project by National Geographic – Human Migration, Population Genetics," *Genographic Project*, accessed 15 January 2017, <https://genographic.nationalgeographic.com/>. Santos emphasized these differences to me in both interviews I conducted with him (Santos, interview, 6 March 2014; Fabrício Rodrigues dos Santos and Theodore G. Schurr, interview with Rosanna Dent, 5 November 2013, Porto Alegre), and in a public presentation of his work within the project; Fabrício R. Santos, "Genetic Insights on Human Evolution," presented at the 59th Congresso Brasileiro de Genética, 18 September 2013, Águas de Lindóia, SP. Spencer Wells also articulated these distinctions in written engagements with critics, such as a special issue of *Cultural Survival Quarterly*; see Spencer Wells, "Genetic Research: How Much We Have to Learn," *Cultural Survival Quarterly Magazine* 29, no. 4 (2005), <https://www.culturalsurvival.org/publications/cultural-survival-quarterly/genetic-research-how-much-we-have-learn>. The same issue featured an article by IPCB executive director Debra Harry and legal council Le a Malia Kanehe; "Genetic Research: Collecting Blood to Preserve Culture?" *Cultural Survival Quarterly Magazine* 29, no. 4 (2005), <https://www.culturalsurvival.org/publications/cultural-survival-quarterly/genetic-research-collecting-blood-preserve-culture>. See also TallBear, *Native American DNA*, 149–176.

⁵⁵⁵ As with any project that involved research with Indigenous people or research on human genetics, the regulatory process for the Genographic project involved approval from the local ethics committee at the Universidade Federal de Minas Gerais, followed by approval of the scientific merit of the project by the *Conselho Nacional de Desenvolvimento Científico e Tecnológico* (CNPq), ethics approval by the *Comissão Nacional de Ética em Pesquisa* (CONEP, the National Commission for Research Ethics), and finally FUNAI approval (in consultation with the Indigenous communities) for each village or T.I. to be visited. Because of the wide publicity and the highly charged debates around genetic research in Indigenous communities, the project went through multiple rounds of review by CONEP. Santos complained that each round of review raised new questions that had not been considered in previous reviews, and that it seemed the CONEP board would find problems indefinitely (Santos and Schurr, interview). Pedro Paulo Vieira, a post-doc who worked with the project, cited former FUNAI president and UFRJ anthropologist Mércio Gomes' advocacy as essential for CONEP to accept the project (Pedro Paulo Vieira, interview with Rosanna Dent, 7 May 2014, Rio de Janeiro). Given disagreements within CONEP about the protocol, the committee made the unusual recommendation that the project be reviewed by the *Comissão Intersetorial de Saúde Indígena* (CISI, Intersectoral Indigenous Health Commission), a board made up of representatives from Indigenous and allied organizations with expertise in Indigenous health. (Carla Teixeira, interview with Rosanna Dent, 24 October 2013, Brasília). For an overview of CISI and its work, see Zilda Arns Neumann, ed., *Memória da comissão intersectorial de saúde Indígena* (Brasília: Editora Universidade de Brasília, 2006), http://conselho.saude.gov.br/web_comissoes/cisi/doc/memoria_cisi.pdf. The process took, according to Santos, four years in total. Salzano decried this delay as a symptom of geneticophobia; see Salzano, "Bioethics, Population Studies, and Geneticophobia," 198. Many geneticists I spoke with used this

the project was well underway in the Andean region before any research began in the South American Coordinator's home country.⁵⁵⁶

By the time Santos and his team first traveled to Xavante territory, they had already conducted research in dozens of other Indigenous communities throughout Peru, Ecuador, and Bolivia. The scientists had also collected samples in Kaingang communities in Southern Brazil. But the story they later told about their work with the Xavante stands out from accounts of other encounters. While some Indigenous groups were wary of participating, or chose not to, the Xavante embraced the project.⁵⁵⁷ Both Santos and post-doctoral researcher Pedro Paulo Vieira spoke of their experience with the Xavante with relish. While they brought general enthusiasm to discussing their fieldwork experiences in oral history interviews with me, they repeatedly set their time in Pimentel Barbosa apart from their experiences in other Indigenous communities, suggesting that their Xavante hosts have been particularly adept at modulating the affective field of

case as an example of why work on the genetics of Indigenous populations has become untenable in Brazil. I have presented my analysis of this regulatory system elsewhere, (Dent, "Bureaucratic Vulnerability: Regulating Research with *Povos Indígenas* in Brazil" presented at the American Anthropological Association, 20 November 2015, Denver CO) and planned publications will flesh out the history of this system as well as my argument that this blossoming of regulation and geneticists' reactions to it create a kind of "bureaucratic vulnerability" for Indigenous peoples in Brazil.

⁵⁵⁶ Research began in the Andean region by 2007, even though the project would not be approved in Brazil until early 2009.

⁵⁵⁷ Groups that chose not to participate cited concerns about control over the use of the samples. The Genographic researchers did not approach certain groups, including the Yanomami, Karitiana, and Sururí, who have been at the heart of controversies over Indigenous blood samples and their use in the past. In Brazilian press coverage, the most commonly cited group to reject participation in South America was a Hatun Q'eros community in the Cuzco region of Peru; see TallBear, *Native American DNA*, 189–196; Antonio Regalado, "Indigenous Peruvian Tribe Blocks DNA Sampling by National Geographic," *Science Insider*, 6 May 2011, accessed 15 January 2017, <http://www.sciencemag.org/news/2011/05/indigenous-peruvian-tribe-blocks-dna-sampling-national-geographic>. At the time that members of one Q'eros community sent their letters of complaint to regional authorities and to the National Geographic Society, *GeneWatch* reported that ninety communities had already participated throughout Peru; see Samuel W. Anderson, "Sacred Ground," *Gene Watch*, 28 May 2011, accessed 15 January 2017, <http://www.councilforresponsiblegenetics.org/GeneWatch/GeneWatchPage.aspx?pageId=340>.

engagement to compel the researchers to return.⁵⁵⁸ As I illustrate, the two scientists articulated a sense of connection and belonging. This section examines researchers' personal reports about their studies of the Xavante to explain the affective experience of research and to make visible the Indigenous labor that made them possible.

The scientists' initial connection with the villages of T.I. Pimentel Barbosa was through Jurandir Siridiwê Xavante, a leader from the village of Etênhiritipá, and his participation in a committee of Indigenous consultants. After long initial delays for ethical approval through CONEP, Santos and colleagues invited individuals from five Indigenous groups to consult on the project in 2007. Anthropologist Mércio Gomes, who had taken the project under his wing to help it through the regulatory process,⁵⁵⁹ recommended Jurandir as the Xavante representative.⁵⁶⁰ The committee met three times, twice before approval was granted, and again after the regulatory body had issued the necessary documentation for the project to begin. After the second meeting, the Genographic researchers started their fieldwork in the communities of those individuals who had participated in their Indigenous committee.⁵⁶¹

The excursion to T.I. Pimentel Barbosa was the second Genographic field trip conducted in Brazil. Once in the village, the team counted on support from a variety of

⁵⁵⁸ Santos, interview; Vieira, interview.

⁵⁵⁹ Mércio Pereira Gomes, according to Pedro Paulo Vieira, took the Genographic under his wing and helped the project navigate the ethical review process following some initial roadblocks. Trained as an anthropologist under Charles Wagley at the University of Florida, in addition to a long career as a professor of anthropology at prominent institutions in Brazil, Gomes served as president of FUNAI from 2003 to 2007 (Vieira, interview). See also Gomes' *curriculo lattes* or online C.V., <http://buscatextual.cnpq.br/buscatextual/visualizacv.do?id=K4788293D8>, accessed 2 August 2016.

⁵⁶⁰ Santos, interview, 34–35. In 2008, the Genographic team held an initial meeting while the rounds of review continued at CONEP; Jurandir joined representatives from Kaingang, Tariana, Wapixana, and Pareci communities.

⁵⁶¹ Santos, interview.

individuals. As their primary interlocutor, Jurandir played a central role in the researchers' understanding and experience of their work in the *aldeia*.⁵⁶² He coordinated their stay, introducing them to the village and officially presenting them to the *warã* on their first night in the village.⁵⁶³ Not unlike Apowê and Warodi before him, Jurandir exercised his political influence in favor of the research project, coordinating with *cacique* Paulo Supretaprã, and other members of the mature men's council. As Santos explained Jurandir's role, he emphasized the importance of Jurandir's cosmopolitan life and perspectives: "He sees these roots in cultural matters, in biological traces, in people's characteristics [across the Americas]," Santos said, "And this kind of person, one who has life experience [*uma vivencia*], is very important for the project. It is in recognizing cultural and biological diversity of Indigenous populations that you see the importance of recovering the past, because it is the past that explains these differences."⁵⁶⁴ According to Santos' narrative, it was due to this shared interest in the Genographic's research questions that Jurandir was so invested and interested in the project. The leader rallied village residents to show up for the scientists' explanations of the project, and helped coordinate the support that the *warazú* would need at each turn.

Upon their arrival, Jurandir directed the visitors to stay in an old, open

⁵⁶² Santos described Jurandir's central role as follows: "Our Xavante experience was really special because it was all under Jurandir Siridiwê's watch [*era o ciclo Jurandir Siridiwê*]. He was a leader, but he wasn't the *cacique* [chief], he was a person who had experience living out of the village" (15).

⁵⁶³ Vieira, interview.

⁵⁶⁴ Santos, interview. Pedro Paulo Vieira echoed many of these same sentiments in his reflections on Jurandir: "Jurandir is an enlightened Indian [*um índio esclarecido*]. He's a curious Indigenous man, a really intelligent guy. Open. And even, here I have to put in a side note: He even smoked a Guarani pipe, just to give you an idea. What I'm trying to say is that that he was always an Indigenous person with an open mind. He looked at his own culture, but with the eyes of someone who is looking from above. And that is not common to find" (Vieira, interview).

schoolhouse at a slight distance from the village. The scientific team was composed of four men: geneticist Fabrício Rodrigues dos Santos as the principle investigator; biophysicist and post-doctoral researcher Pedro Paulo Vieira; Francisco Araújo, a graduate student in social anthropology at the *Universidade Federal do Rio de Janeiro*; and Peruvian graduate student José Sandoval, who conducted the entirety of his doctoral research in the context of the Genographic.⁵⁶⁵

In addition to Jurandir, a number of other villagers provided support. Two village residents assumed the roles of guides and guards, helping the researchers with daily tasks, and protecting them and their equipment from overly curious children. “They were worried about us,” Santos explained, “because the kids mess around a lot [*mexem muito*], and we had all our field equipment. We had computers, cameras. We had everything there.”⁵⁶⁶ These men also took the researchers out to explore the *cerrado*, and taught them about Xavante fire hunting practices. Xavante women came to help prepare food for the men—Santos did not know much about them, but speculated they were likely wives of the two men who acted as caretakers for the researchers. The researchers also worked with two leaders from each of the nine villages that participated in the study, and so this group of eighteen (including Jurandir and Paulo Supretapã from Etênhiritipá) provided

⁵⁶⁵ Francisco Araújo was a student of Mércio Gomes. José Sandoval Sandoval was completing his doctoral research in genetics under Santos’ supervision, funded by the National Geographic as part of the project. As an Aymara scientist, according to Santos’ account, members of the communities that the group visited had an easy time identifying with the Peruvian doctoral student. Santos even recounted an episode where an audience member in one of the public presentations of the project vocally rejected participating, but after speaking with Sandoval wanted to join. Santos recounted the happenings after the man left the public event angrily: “This is an interesting topic, because later, one day Sandoval went out alone to interact with the Indigenous folks. He’s Indigenous, Aymara, right? And everyone identifies easily with him. When he says he’s from Peru, everyone is curious. By coincidence he ran into [the Indigenous man who had been upset] and he explained the project to him. And after that, [the man] wanted to participate” (Santos, interview).

⁵⁶⁶ Santos, interview.

support as the team visited nine of the ten villages in the territory, and again later by meeting with the researchers to hear about the results of the studies.⁵⁶⁷

Santos' narrative of his research experience in Etênhiritipá included a wide variety of interactions that had little to do with the project's stated research goals of collecting genealogical data and genetic samples:

It was really good because we interacted a lot. I brought a movie, I brought my computer. I have a film that tells the story of first contact of an uncontacted Indian group over in Rondônia. ... It shows the original footage by the *indigenistas* contacting an isolated tribe, the Uru-eu-wau-wau, when they were contacted in the 80s. Some amazing things. And they loved it. ... And almost no one speaks Portuguese. So Jurandir translated. Every now and then he asked me things, I explained, and he translated into Xavante. It was really interesting. The film, which was supposed to be an hour and a half, took about four. It was an all-night

⁵⁶⁷ Ibid. The village of Pimentel Barbosa did not participate in the study. This was at a moment of particular political tension between the villages of Pimentel Barbosa and Etênhiritipá, which share a school and a health post, and are located less than a half mile apart. Jurandir advised the Genographic team that the other village would be unreceptive to their research, and Santos relayed this to me as follows: "In [T.I.] Pimentel Barbosa there are ten villages. And there is one that is the enemy of all the others. We couldn't even get close to that one, which is just 300 meters away from the principle village [Etênhiritipá]" (interview). My interpretation is that while Etênhiritipá and Pimentel Barbosa villages often experience tension and it is difficult to be welcome in both villages without having strongly established relationships predating the 2006 split, neither one nor the other maintains better relations with the other eight villages of the territory. Political alliances between different villages within the territory wax and wane. This has been a prominent theme in anthropological and historical literature; see Garfield, *Indigenous Struggle*, esp. 66–88, and citations therein. This was corroborated in an oral history interview: James R. Welch, interview with Rosanna Dent, 21 April 2014, Rio de Janeiro. Residents of the other nine villages might also object to Santos' perception that Etênhiritipá is the principle village of the territory, and residents of Pimentel Barbosa would definitely claim precedence, as it was the first permanent village, established in the early 1970s after the move from Wedezé.

movie session, with that incredible starry sky, everyone sitting. The whole village, you know? A lot of people.⁵⁶⁸

Later in our interview, Santos continued to describe stargazing with a laser star pointer and an iPhone constellation application, with the Xavante pointing out the myths they see written in the sky, and asking how to make an app to show their own constellations. “So it was a moment fully lived in every minute,” Santos sighed. “And I think that really enriches [*engrandece muito*] the research, the way that you see things, the way that you take in their feedback, their responses when you present your results. And you see them—when we went to present the results they were just like this with us—making jokes or discussing the results. And the interaction [*o convívio*] in that moment was really interesting, because they arrived open.”⁵⁶⁹ For Santos, the Xavantes’ openness to the researchers and their affective labor were crucial in providing the researchers with a much richer experience of what might elsewhere have been a day or two of consultation and community informed consent procedures, and an afternoon of cheek swabs.

But the stories that Santos and Pedro Paulo Vieira told went beyond the joys of hunting expeditions, film viewings, and stargazing. They felt most deeply drawn in by what they understood as their inclusion in the Xavante village. “And not only that,” Santos told me, following up on his account of the movie night, “We participated in rituals with them. Not the rituals they put on for tourists, ones that they were really doing.” After describing their nightly participation in the *warã*, where Xavante men had asked the researchers to comment on their activities for the day, Santos explained:

⁵⁶⁸ Santos, interview.

⁵⁶⁹ Ibid.

There were two rituals going on at the same time. One was a baptism. At fifteen the Xavante there become adults. And they receive their adult name. It's not *baptism*, it's something else, but it's like a kind of baptism. They change their names and then can have wives. In that ceremony I was baptized too. I'm *öwawe*, so I can have five or six *poreza'õno* wives. [laughs] Only joking, okay? But that's just the clan baptism. If I were Xavante I would have to have another baptism, but I didn't do that one. Pedro Paulo, the post-doc, he wanted to do it. He even wanted to marry his wife there, but it didn't work out because he found out that to be baptized he would have to hunt at least a giant anteater, all by himself. And he's a *carioca*, who's never lived in the countryside.⁵⁷⁰

Santos' story of his time in the village betrays the joy, excitement, sense of humor, and sense of engagement that set the Xavante experience apart for the Genographic researcher. While he took joy in his "baptism" as a member of the *öwawe* moiety, Santos joked about the researchers' place. They still did not completely belong: The potential for polygamy was only in jest, and Vieira the post-doc might not really have been able to hunt enough game for a Xavante wedding. Nonetheless, the researchers saw themselves as significantly closer to their subjects. Comparing their own time in the field to less positive reports from their interlocutors regarding other research teams, Santos said, "They liked us a lot because we interacted with them ... Since we woke up and slept there, in the same spot each day, we bathed there, with them laughing and playing jokes on us, with the kids playing with us, playing football, doing this, doing that. It was

⁵⁷⁰ Ibid. What Santos referred to as clans are more technically described as patrilineal exogamous moieties. Carioca refers to a person from the city of Rio de Janeiro.

important.”⁵⁷¹ What Santos attempted to convey to me was not a series of emotional responses, but the researchers’ movement through an affective field.

Pedro Paulo Vieira echoed this sense of engagement and inclusion, framing the Xavante as the pinnacle of the Genographic in Brazil both because of their cultural and biological characteristics as well as their interactions with the researchers:

The Genographic was adopted by the Xavante of South America in Brazil. So if you asked me which ethnic group represents the Genographic in Brazil, I would say the Xavante. They are a people with an extremely strong culture—extremely ancestral, extremely rich—who, instead of wanting to understand what we were doing, simply absorbed the Genographic into their own culture. Fabrício, myself, and some other members of our team were even assigned to clans within the village. I was given a name. We participated in Xavante rituals. That is to say, we became part of the Xavante community because of the project. ... They are the group that best understood, that most enjoyed, that had no fear or misgivings. On the contrary, they absorbed [the Genographic], used it to explain what they already knew, and spat out the results. I mean, for me this was the apex of research here in Brazil. ... With the Xavante, we went, we went back, we went back again.”⁵⁷²

Vieira emphasized the Xavantes’ adoption of both the researchers and the project. This inclusion was compelling to the biophysicist because it was both personal and

⁵⁷¹ Santos, interview.

⁵⁷² Vieira, interview.

intellectual; it incorporated an invitation to re-marry his wife in the village alongside a perceived interest and investment in the scientific work itself. The Xavante were exceptional, in Vieira's eyes, because of the strength of their culture and their capacity to "absorb" the scientific narrative of the Genographic and make it their own. The researchers perceived the cultural strength and profound ancestral quality of the community not because they were told about these qualities (although that may also be true—this certainly is a common refrain that the Xavante work to cultivate), but because they participated in the rituals. Santos specified, very explicitly: "Not the rituals they put on for tourists, ones that they were *really* doing."⁵⁷³ The researchers were struck by what they perceive as the authenticity of their hosts. At the same time, they felt embraced, included in this authenticity. These experiences were expressed during our interviews through the enthusiasm, sense of humor, and intense energy of the researchers' accounts.⁵⁷⁴

The stories the scientists wove as they spoke to me are, at their core, about the affective experience of research. The researchers' understandings of their own positions in relation to their Xavante interlocutors are mediated as much by the things the researchers *did* as by the things they thought, said, or heard. Participating in rituals,

⁵⁷³ Santos, interview (emphasis added). This claim is not only about perceived hierarchies of authentic and inauthentic performance of ritual. I am interested in this distinction because I see it functions as a claim to authority on the part of the scientists.

⁵⁷⁴ Vieira enthusiastically reiterated his sense of engagement and belonging following our interview. After reading a short piece I wrote for a conference at the Max Planck Institute for the History of Science, he responded by emailing me an image of a pair of soccer shorts or a kind of uniform that a number of young men in the village made for him. In the email, he retold the story of his adoption into a moiety and an age-set, and the name that he was given, Serenhi'õmo, meaning hawk's feather (Vieira, personal communication with the author, 15 March 2016). The text for the conference was later published, with the photo as an illustration; see Rosanna Dent, "Invisible Infrastructures: Xavante Strategies to Enroll and Manage *Warazú* Researchers," in *Invisibility and Labour in the Human Sciences*, ed. Jenny Bangham and Judy Kaplan (Berlin: Max Planck Institute for the History of Science Preprint, 2016), 65–74.

stargazing, laughing during a “movie night,” hunting and fishing, visiting the *cerrado* to learn about Xavante fire hunting—all of these lived bodily experiences are central to how the Genographic researchers recounted their research, more so than intellectual questions about the ancestral mitochondrial or Y-chromosome lineages, their fundamental topic of study. The scientists described bonding that was fundamentally masculine, and that was possible because they were men connecting with men as sanctioned by Xavante gender norms.⁵⁷⁵ The researchers’ accounts to me were, no doubt, influenced deeply by their prior experiences of social scientists’ and journalists’ writings on the Genographic, which I discuss at more length below. However, even as their accounts of fieldwork implicitly responded to prior criticisms, the scientists emphasized personal connection rather than intellectual or ethical claims in order to valorize their work. In these accounts, the researchers presented their acceptance by the Xavante as their source of legitimacy. They defined the moral valence of their research through this acceptance, inclusion, and adoption.

Turning our attention to the application, uses, and cultivation of affect highlights the extensive care work involved for Xavante communities to host outsiders. The narratives that the Genographic researchers offered suggest that Xavante subjects went to substantial trouble to inculcate certain affective states in the researchers who visited

⁵⁷⁵ Santos described Xavante society as “machista” in our interview, and he set the Genographic researchers’ experience apart from that of other teams. According to Santos, his group, which consisted of men only, was able to interact more closely than mixed-gender groups of researchers, who chose to stay at the government post as opposed to in the village. Santos mentioned, as an example, that I could not attend the *warã* as a woman. The Genographic researchers understood Xavante society as a particularly *masculine*, strong, warrior society, in similar ways to those discussed in Chapter 3. Vieira compared the Xavante to the Spartans in the North American movie *300*, saying, “The Xavante are, I sometimes joke, a ‘300’ people, you know? Spartan. There’s a Spartan school of how to be Xavante, which takes 5 years.”

them. Many people looked after the outsiders, from Jurandir to the “guards,” to the women who cooked, to members of the leadership of eight other villages that would be visited on the next trip. Rituals needed explanation; equipment had to be protected. Moreover, a “movie night” was not a spontaneous happening in the village, but required communication and coordination, both with the staff of the government post, where electricity was available, and with members of the community to rally attendance.⁵⁷⁶

These efforts may not have been visible to Santos, or he simply may not have included it in his retelling to me. But even those aspects of the researchers’ experience that could have seemed like a spontaneous decision, such as a hunting trip or a fishing trip, involved guiding the researchers through territory and likely teaching some of them *how* to hunt. Xavante individuals had to provide near-constant translation, whether of the film, the researchers’ explanations, or simply in day-to-day interactions between the *warazú* and villagers without a strong command of Portuguese. Adopting the researchers into different aspects of the kinship system, likewise, was a process that required labor in order to locate them in a moiety. For Vieira, who was further inserted into the system during a subsequent visit to the village, his interlocutors had to decide which age-set he belonged to, and give him his Xavante name.⁵⁷⁷

I apply the term affective labor to this work to draw attention to the fact that this production of relationships, which is generally invisible or naturalized in most accounts of scientific work, had important implications. This affective labor is one more variety of

⁵⁷⁶ I know this from my own—only partially successful—attempts to gather members of Pimentel Barbosa village for a slideshow of historic pictures, as I discuss at more length below.

⁵⁷⁷ For a discussion of Xavante naming, see Lopes da Silva, *Nomes e amigos*.

invisible labor that historians have explored and documented as central to creating knowledge in the human sciences and beyond.⁵⁷⁸ As Hardt writes, citing feminist scholarship on the importance of caring and kin work, affective labor is the work of human contact. It is a corporeal process with intangible outcomes such as “a feeling of ease, well-being, satisfaction, excitement, passion—even a sense of connectedness or community.”⁵⁷⁹ This experience of belonging was essential to the researchers’ understandings of their fieldwork; the excitement and sense of connectedness that permeated their accounts contributed to their sense of their own identities as researchers. Hardt draws this connection between affective labor and identity clearly, writing, “Affective labor is itself and directly the constitution of communities and collective

⁵⁷⁸ A rich literature in the history of science explores the invisibility of certain laborers. Steven Shapin and Simon Shaffer’s foundational work uncovered the technicians below the floorboards who made Boyle’s Air Pump possible in the mid-seventeenth-century. See Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 2011 [1985]). Certain kinds of work are disproportionately invisibilized, including much caring labor and kin work. Susan Leigh Star and Anselm Strauss, in a discussion of computer supported cooperative work, suggest that important varieties of labor, including articulation and coordination, are often overlooked, and that much scientific labor is invisibilized by publications that condense years of laboratory work into a table or graph; see Susan Leigh Star and Anselm Strauss, “Layers of Silence, Arenas of Voice: The Ecology of Visible and Invisible Work,” *Computer Supported Cooperative Work (CSCW)* 8, no. 1–2 (1999): 9–30. doi:10.1023/A:1008651105359. On the labor of field informants see Schumaker, *Africanizing Anthropology*; Camerini, “Wallace in the Field”; and Roger Sanjek, “Anthropology’s Hidden Colonialism: Assistants and Their Ethnographers,” *Anthropology Today* 9, no. 2 (1993): 13–18, doi:10.2307/2783170.

⁵⁷⁹ Hardt, “Affective Labor,” 96. Xavante affective labor shares important foundations with Hardt’s conception, even as it diverges in a few ways. For Hardt, the shift to a predominance of affective labor is part of a larger economic trend that he calls postmodernization or informatization. Focusing on economies that have shifted from industrial to service-oriented sectors, Hardt understands the postmodernized economy to be one where “Information, communication, knowledge, and affect come to play a foundational role in the production process” (ibid., 93). Immaterial labor—that is, labor that does not directly result in the production of goods—represents the most valued form of production in this new formulation of economy. However, as measured at any scale (internationally, nationally within Brazil, and certainly locally in Mato Grosso and the surrounding area) the work that villagers put into hosting scientists began well before the shift to Hardt’s informatized economy. The scientific economy, moreover, has always been one in which information, communication, and knowledge are central.

subjectivities.”⁵⁸⁰ In the case of the Genographic research, the labor of including the researchers, of creating feelings of acceptance and belonging, created a collective subjectivity—a particular kind of research subjectivity. It made the researchers confident of their strong bonds of friendship with their interlocutors, and bolstered their sense of the possibility and promise that their research could have for its subjects. It energized them, and the scientists mobilized this acceptance and belonging to make claims about their legitimacy in response to the considerable critique and resistance that their research protocol had engendered. But this sense of belonging and acceptance was also—and continues to be—mobilized by the Xavante with the expectation of mutuality in their relationships. As I argue in the next section, Xavante actors have drawn and can potentially draw upon this newly animated subjectivity to meet social, political, and even economic needs.⁵⁸¹

But before moving on, it is important to address the many factors influencing how the Genographic researchers engaged with me. As noted above, from conception to implementation, the Genographic project has come under intense critique by Indigenous activists and scholars, as well as a variety of non-Indigenous social scientists and journalists. Santos, Vieira, and colleagues were acutely aware of these critiques, and had to respond to them throughout the phases of the project, from regulatory approval to the ongoing presentation of results. Santos commented on Kim TallBear’s work, for

⁵⁸⁰ Hardt, “Affective Labor,” 89. Central to Hardt’s conceptualization of affective labor is its potential as an alternative circuit for value production outside of capitalist systems. For foundational scholarship examining caring work see Dorothy E. Smith, *The Everyday World as Problematic: A Feminist Sociology* (Boston: Northeastern University Press, 1987).

⁵⁸¹ Hardt also finds great potential in these forms of labor, which he frames as offering the possibility of biopower from below; see Hardt, “Affective Labor,” 98–100.

example, in our very first conversation before he had decided whether to participate in my research, indicating understated displeasure.⁵⁸² He also had prior experience interacting directly with social scientists that were studying his scientific production and practices. Before I met the Genographic researchers, socio-cultural anthropologist Michael Kent had shadowed them in the field in Peru, and published work on how the Uros people of the Lake Titicaca region mobilized the Genographic project's studies to bolster their claims to distinctive identity and territorial patrimony.⁵⁸³ Santos understood this social-scientific analysis to provide support for the value of the Genographic initiative, and included it alongside his rebuttal of criticisms from the Indigenous Peoples' Council on Biocolonialism in public presentations.⁵⁸⁴ I believe my own background in biology as well as Francisco Salzano's and Maria Cátira Bortolini's support of my study helped me gain Santos' trust.

At different moments in their interactions with me, the Genographic researchers sought to re-articulate their defenses of the project through our conversations. Santos emphasized the slow process of introducing the project to the Xavante leadership, discussing the project in the context of the *warã*, and only with approval from the *warã*, presenting the project to the community.⁵⁸⁵ In doing so, Santos responded, implicitly, to

⁵⁸² In this conversation, I indicated I was familiar with her work. I did not express an opinion in support of it or against it. This was shortly before *Native American DNA* was published.

⁵⁸³ Kent, "The Importance of Being Uros."

⁵⁸⁴ Santos, "Genetic Insights on Human Evolution."

⁵⁸⁵ Santos, interview. Santos explained this as follows: "All this [with the *warã*] was before sampling. So we explained the project before, and we talked with them. We looked into the genealogical situation, to figure out who were the oldest, who could contribute. So we did that whole evaluation before. We participated in the cultural events, and then, after that, on the day of the sampling everyone [from the village] came to hear the explanation. They were really engaged, it took *forever* [*demorou para caramba*], and afterward there was this huge line. But in the end it was just six [participants]. But that's it, that's the

critics who considered the time allotted for community consent processes in the project inadequate.⁵⁸⁶

Santos' version of the Xavante fieldwork included exaggerations. For example, in his enthusiasm he commented on how two hundred men participated in the evening *warã*, while there were only fifty-seven men who were in eligible age grades to participate according to a 2009 census.⁵⁸⁷ He described Jurandir as chief, not only of Etênhiritipá, but in a general sense as leader for the whole of the *Terra Indígena*. Santos also emphasized that he had invited several prominent socio-cultural anthropologists to collaborate with him, two of whom indicated in private conversation that they had no memory of an invitation. However, while the interviews must be read critically, Santos' and Vieira's sentiments of excitement and longing were genuine, and many portions of their accounts regarding their interactions in the field match up with reports from other researchers, including in some ways my own, about how they were received and treated by villages in T.I. Pimentel Barbosa.

Twenty-first Century Difficulties Proposing Research

The Genographic researchers' experience in the field makes clear the labor that Xavante interlocutors invested in their initial interactions with one particular group of researchers. It illuminates some of the ways residents of Etênhiritipá have worked and continue to work to establish and cultivate relationships. In itself, however, this case

project. The purpose isn't to go out and sample right and left just to have a high number" (Santos, interview.)

⁵⁸⁶ TallBear, *Native American DNA*, 190–191.

⁵⁸⁷ Santos, interview. The census numbers are from James R. Welch, e-mail message to author, 24 March 2017.

study is insufficient to get a sense of whether the enrollment of researchers as experienced in the context of the Genographic project is a broader strategy employed throughout the T.I., and if this building of relationships is important for the scientific and political outcomes of field research. In the following sections, I explore the experience of another scholar, from a distinct discipline, who visited Pimentel Barbosa village for the first time in 2004, before the village divided into Pimentel Barbosa and Etênhiritipá.⁵⁸⁸ Examining the experience of this sociocultural anthropologist, complemented by reflections of other researchers who visited T.I. Pimentel Barbosa in the 1990s and 2000s, two points become salient: a pattern emerges regarding how researchers are engaged, and some potential motivations for why villagers go to such lengths to establish these relationships become clear.

A bureaucratic maze brought graduate student James Welch into the door of FIOCRUZ researchers Santos and Coimbra in the early 2000s. Welch, enrolled in the anthropology department at Tulane University in Louisiana, had proposed a dissertation project to study the historical, social, and ecological context of Jupaú (Uru-eu-wau-wau) trekking practices in Rôndônia. Like many researchers, he came up against significant challenges in the regulatory process. Whereas the Genographic had faced delays at the stage of CONEP review, Welch's initial project encountered difficulties at the level of the local FUNAI offices and community consultation. After an initial communication from

⁵⁸⁸ In "Age and Social Identity," Welch notes that prior to its split, the village simultaneously held two names—Pimentel Barbosa and Etênhiritipá. Because claims on the Xavante name were fraught, Welch identified the original unified village as Pimentel Barbosa/Etênhiritipá in his dissertation, and he called the post-split villages the "new village" and the "old village." Here I refer to the old village as Pimentel Barbosa and the new village as Etênhiritipá not to endorse any claims to the name, but following the conventions of subsequent publications that have come from the ENSP research team, including Welch et al., *Na primeira margem do rio*.

the local FUNAI representative, which cited community objections to the project without providing a motive, and a further seven months of delay with no documented response, the *Coordenadora Geral de Estudos e Pesquisa* (the General Coordinator of Studies and Research) from the central FUNAI office in Brasília made a trip to consult directly with the Uru-eu-wau-wau community.⁵⁸⁹ Six weeks after her trip, and ten months after the CGEP had sent the initial request to the regional office, a subsequent communication indicated that the community had rejected the project. Despite the fact that Welch's proposal at no point included the collection of biological samples, human or non-human, the FUNAI administrator for Porto Velho specified that the resistance was due to the precedent of research abuses with the Karitiana, writing, "the Uru Eu Wau Wau and Amondaw peoples, the inhabitants of *Terra Indígena* Uru Eu Wau Wau and the *Chefes de Postos Indígenas*, are aware since the month of May 2003 [of the request to conduct the study], but notwithstanding, still allege that they will not accept due to what happened with the Karitiana, and also a certain reluctance in regards to the presence of foreigners in their reserve."⁵⁹⁰ Welch was never provided with this reasoning for the community's decision to abstain from the research, but by the time the response came back negative, he had already turned his attention to finding an alternate option for his doctoral

⁵⁸⁹ Rômulo Aiqueira de Sá to José Francisco Rodrigues Furtado, Fax no. 046/GAB/AERP VH, 23 April 2003; "Relatório de Viagem," Tereza Cristina Ribeiro 22 September 2003, FF-SEP-PQ, Caixa 767, Doc 12967, SEDOC-FUNAI. Other documents in Welch's dossier show that Welch, his advisor William Balée, his local *co-orientador*, Museu Nacional anthropologist Eduardo Viveiros de Castro, and the director of the Museu Nacional submitted the required documentation to CNPq on 9 January 2003. The request was initially forwarded from the CGEP (Coordenação Geral de Estudos e Pesquisa) to the regional office on 14 February 2003.

⁵⁹⁰ Rômulo Aiqueira de Sá to Cláudio dos Santos Romero, Memo no. 240/GAB/AER/FUNAI/PVH, 24 November 2003, FF-SEP-PQ, Caixa 767, Doc 12967, SEDOC-FUNAI.

research.⁵⁹¹

Welch's interest in ethno-ecology landed him in a session at the SALSA conference in 2004, where he was first introduced to public health researchers Ricardo Ventura Santos and Carlos Coimbra in what he called "a completely fortuitous meeting."⁵⁹² As Welch discussed his interest in youth perceptions of and engagement with the environment, the ENSP researchers commented on the formalized nature of Xavante notions of "youth" as well as the importance of the age-grade system in Xavante society. The conversation evolved into a new proposal, one that Welch would make to the Xavante of Pimentel Barbosa thanks to Ventura Santos' and Coimbra's enduring relationship with the Xavante.⁵⁹³

Establishing Enduring Engagement

Ventura Santos and Coimbra had been working in T.I. Pimentel Barbosa for fifteen years by the time they met Welch. Their work in the T.I. began in collaboration with Nancy Flowers in 1990, when they conducted a re-study of her doctoral research in order to evaluate change in health over time.⁵⁹⁴ Ventura Santos, recalled the initial plan saying, "From the research point of view, there was something that we had not had [previously]: historical depth, which was fundamental."⁵⁹⁵ The team saw possibilities in

⁵⁹¹ James R. Welch, interview with Rosanna Dent, 27 March 2014, Rio de Janeiro.

⁵⁹² Ibid.

⁵⁹³ Ibid.

⁵⁹⁴ After returning from the field it took eight years for Flowers to complete her dissertation, as she was distracted by the responsibilities of life. Already in her sixties by the time she completed her doctorate, Flowers recognized she had little chance of being seriously considered for a tenure track academic job. She assumed an adjunct position teaching at Hunter College-CUNY.

⁵⁹⁵ Ventura Santos, interview.

the layers of data that had accumulated over the years in Pimentel Barbosa. Not only would they be able to draw on the ethnographies and genealogies that Maybury-Lewis and Flowers had created in 1958 and 1976-77 respectively, they would also collaborate with geneticist Francisco Salzano to employ the biomedical and genetic data he had collected with Neel and colleagues in 1962.⁵⁹⁶ The collaboration between Flowers, Coimbra, Ventura Santos, and Salzano would culminate with the 2002 publication of *Xavante in Transition*, one of the earliest and most comprehensive diachronic studies of the health and nutrition of an Indigenous population in lowland South America.⁵⁹⁷

Coimbra and Ventura Santos had little sense of just how much the Xavante project would shape the futures of their careers. When they arrived in 1990, the village made an impression immediately.⁵⁹⁸ Being introduced by an experienced researcher was fundamental to the young Brazilians' success in establishing relationships with members of the community. "Arriving there with Nancy was really straightforward, really calm," Ventura Santos recalled, "That is to say, we weren't arriving as strangers, even though we had never been there. We weren't complete foreigners, because we were with

⁵⁹⁶ As they turned their attention to the Xavante, Coimbra and Ventura Santos were warmly welcomed by the researchers who had preceded them in Wedezé and Pimentel Barbosa. Coimbra described Neel's response saying, "we wrote to Neel, who received us with open arms. I remember that Ricardo and I were in Bloomington, and we grabbed the car and went to Michigan to visit him. And he spent the whole day with us at his laboratory, served us coffee—such courtesy!—and he showed us everything, everything." Coimbra, interview., The reception was equally warm from Salzano.

⁵⁹⁷ Coimbra et al., *Xavante in Transition*.

⁵⁹⁸ Remembering his first days in Pimentel Barbosa, Coimbra commented, "It was a vision of another world, because I was used to working with Indigenous groups from the rainforest [*povos da floresta*] ... And you arrive to the Xavante and there aren't any trees, just tons of light, ... and the organization of the village in the horseshoe shape is completely different from Tupi villages." Part of the affective experience for both him and Ventura Santos of arriving was a kind of "return" to the familiarity of the *cerrado*—they had both grown up in Brasília and felt connected to the landscape of central Brazil (Coimbra, interview)..

someone who was very close to them.”⁵⁹⁹ Coimbra reflected on the warm reception Flowers’ return garnered after twelve years away from the village, saying, “Everyone remembered her, and everyone holds her in high esteem even to today. . . . And it’s interesting, because underneath there was never a moment that Nancy was jealous of “her tribe,” you know? Because that happens a lot between anthropologists [laughs]. It was just the opposite.”⁶⁰⁰ Coimbra specifically mentioned Flowers’ affective labor in being “generous” with her established relationships in the village. However, embedded in his account are the equally important reactions of the villagers who were equal agents in creating the experience that Santos and Coimbra so fondly remember. If the generosity that Neel, Salzano, and especially Flowers showed to the newcomers made the project tenable, the response of the Xavante made it possible.⁶⁰¹

On this first trip, as with future visits to the field, the first step for the researchers was to present themselves and their plans at the *warã*, which had now come to function as part of the Xavante system of oversight for researchers. Ventura Santos explained, “If we arrived one day, the next day at five-thirty in the morning we were there in the *warã*, introducing ourselves, recounting our news, with them wanting to know what we wanted to do there, what our plans were. . . . So that already worked as a way of mediating [our

⁵⁹⁹ Ricardo Ventura Santos, interview with Rosanna Dent, 15 April 2014, Rio de Janeiro.

⁶⁰⁰ Coimbra, interview.

⁶⁰¹ Interestingly, when Flowers, Coimbra, and Santos arrived in Pimentel Barbosa, they coincided with David and Pia Maybury-Lewis and a large film crew who were in the process of filming for Maybury-Lewis’ documentary series, *Millennium: Tribal Wisdom and the Modern World*, which would air in 1992 on public television. According to Flowers, the Maybury-Lewis were not entirely pleased about having other researchers in the village while they were filming, and David Maybury-Lewis asked them to reschedule their fieldwork (Flowers, interview, 22 August 2013).

presence] in a really interesting way.”⁶⁰² After the mature men had discussed the work and granted permission, the researchers were carefully observed as they went about their work. “Since you’re in a semi-circular village, if you go from house to house, in the background you’re always being watched, right?” Ventura Santos explained, “So the process [of research] is socialized from a spatial perspective.”⁶⁰³ And so under the watchful eye of the residents, Coimbra, Ventura Santos, and Flowers went about collecting follow up data complete with duplicate Polaroid photos, an updated village census, biometric measurements, health indicators, and blood samples for subsequent genetic analysis.

As they undertook their first years of research in Pimentel Barbosa, Coimbra and Ventura Santos were busy settling at the *Escola Nacional de Saúde Pública* (ENSP, The National School for Public Health) within the *Fundação Oswaldo Cruz* (FIOCRUZ) in Rio de Janeiro. There they built a research program in Indigenous health with particular attention to the social determinants of health.⁶⁰⁴ Despite spending over a month in Pimentel Barbosa during their first round of fieldwork, their visit was only sufficient to scratch the surface of the research topics they envisioned. In order to replicate more complex aspects of Flowers’ doctoral work, such as the time allocation studies,⁶⁰⁵ they would require longer stays. As Coimbra described it, “In reality, that first visit laid the

⁶⁰² Ventura Santos, interview.

⁶⁰³ Ventura Santos, interview.

⁶⁰⁴ The early 1990s was a time of widespread changes in the Brazilian public health system in the wake of re-democratization. Although I do not discuss the context at length here, it is something I hope to pursue in more detail in future work.

⁶⁰⁵ Time allocation studies involved observing, at regular intervals, the activities of everyone in a household in order to get a sense of how time is divided between different kinds of activities such as cultivating crops, hunting, fishing, child minding, and so on.

foundation [*lançou as bases*] for the development of a series of other projects that our students began soon thereafter here at the school [of public health]. . . . Today I wouldn't even be able to tell you exactly how many people have done their thesis work with the Xavante."⁶⁰⁶ Beginning in the early 1990s, Coimbra and Ventura Santos brought a consistent stream of master's students to the field with them, delving into greater detail regarding individual diseases such as malaria, or health factors beyond their own expertise, such as oral hygiene.⁶⁰⁷ Four years into what was evolving into a much more extensive project than anticipated, Flowers returned to the field with one of their students, Silvia Gugelmin, to conduct time allocation studies.⁶⁰⁸ Effectively, T.I. Pimentel Barbosa, and particularly the village of Pimentel Barbosa/Etênhiritipá within it, became a training ground for waves of public health researchers, who went with the ever-growing team to learn fieldwork methodology, and to be trained by their Xavante subjects on how to behave in the field.

Reflecting on how this lasting relationship unfurled between the research group and the Xavante community, Ventura Santos said, "I do not think any of this would have happened without both them and us being open to such a long-term relationship. I mean,

⁶⁰⁶ Coimbra, interview.

⁶⁰⁷ Early publications and theses include: Rui Arantes, "Saúde oral de uma comunidade indígena Xavante do Brasil Central: uma abordagem epidemiológica e bioantropológica," (master's thesis, Escola Nacional de Saúde Pública, 1998); Rubens Vaz Ianelli, "Perfil epidemiológico da malária em uma população indígena do Brasil Central: os Xavante de Pimentel Barbosa" (master's thesis, Escola Nacional de Saúde Pública, 1997); Santos, Ricardo Ventura, Carlos E. A. Coimbra Jr., Nancy M. Flowers, and Joaquim P. Silva, "Intestinal Parasitism in the Xavante Indians, Central Brazil," *Revista Do Instituto de Medicina Tropical de São Paulo* 37, no. 2 (1995): 145–48; Francisco M. Salzano, M. H. L. P. Franco, Tania de Azevedo Weimer, S. M. Callegari-Jacques, M. A. Mestriner, Mara H. Hutz, Nancy M. Flowers, R. V. Santos, and Carlos E. A. Coimbra Jr., "The Brazilian Xavante Indians Revisited: New Protein Genetic Studies," *American Journal of Physical Anthropology* 104, no. 1 (1997): 23–34.

⁶⁰⁸ Silvia A. Gugelmin, "Nutrição e alocação de tempo dos Xavante de Pimentel Barbosa, Mato Grosso," (master's thesis, Escola Nacional de Saúde Pública, 1995).

we always felt really good there, and the relationship with them developed. Are there tensions? Of course, there are always tensions, but what allowed this relationship to develop was that it is not based on a single person, right?”⁶⁰⁹ Part of what made the researchers’ ongoing visits to the village possible was the open-ended quality of their work. They built connections with a variety of elders, leaders, and others in the community, and they were open to follow the community’s lead when there were issues of relevance to be studied.

Coimbra identified this as a major shift in the approach of both the researchers and the Xavante over the course of the collaboration. “Our perspective really changed,” he said, “[At the beginning,] we went into the Xavante community without knowing anything, just with the Neel and Salzano references in hand to repeat that research and without having a discussion with the community about what we were doing. We only came to discuss it later. In contrast, today the exchange is really intense.”⁶¹⁰ Over time, and with continued interaction, the Xavante started to push the researchers towards investigating community concerns. As diabetes and other metabolic issues became increasingly prevalent, key interlocutors within the village started insisting that the public health researchers turn their attention to chronic health problems. “Really, to be honest,” Coimbra recounted, “At first, when they started to talk to me about diabetes, it took me about two years to come to terms with the fact that I couldn’t escape, because I had always strongly focused on the ecology of infectious disease. I knew something about metabolic disorders, but I didn’t know the field intimately. ... It took me a while to get up

⁶⁰⁹ Santos, interview.

⁶¹⁰ Coimbra, interview.

the nerve, but then we did it,” Coimbra laughed.⁶¹¹ By 2015, research on metabolic issues was a major aspect of the work of Coimbra and Ventura Santos’ research group. It was in the context of these sustained interactions that leaders in Pimentel Barbosa were able to advocate for a new direction in research that would address issues with which they themselves were grappling. Other, newer members of the ENSP research team would also be groomed to address topics of interest to the community, as was the case for James Welch, who began building relationships with the public health researchers and community members around the same time that their research program was broadening to include chronic health challenges.

Kinship and Xavante Research Systems

Much as Nancy Flowers had facilitated the entry of Carlos Coimbra and Ricardo Ventura Santos into T.I. Pimentel Barbosa, the ENSP team introduced Welch. By 2004, he was one in a long line of master’s and doctoral candidates. This positioning meant that his regulatory experience was vastly different from the challenges of his first doctoral project proposal. Welch accompanied Coimbra to assist on an existing research project in May 2004, and met community leaders and presented his project. In Pimentel Barbosa, the leadership in the village were quick to give an initial approval to the project.

Prior to this trip, Welch had spoken at length with colleagues at ENSP about their fieldwork, and he knew he would have to present his project in the *warã* as soon as he arrived. The social institution of the *warã* had become the central system of oversight for

⁶¹¹ Coimbra, interview.

researchers in the village. Traditionally, the *warã* is a place of discussion and decision making for the men's council, and over time it has also become a semi-formalized part of the research process: early in their stay in the village—usually the first night, or at least before starting any investigation—researchers are expected to introduce themselves. At the evening meeting, with the mature and elder men gathered, visitors explain their research questions, methodology, and what the products of the research would be. The *warã* then becomes a space for discussion of the proposal, further questions, and a collective process to accept or decline.

Welch realized he would be required to provide justifications of the benefits his project offered to the community. He had navigated community consultation processes in the past in his prior undergraduate and master's research, as well as during a period of archaeological contract work.⁶¹² “I knew from my other experiences,” Welch explained, “that I had to be prepared for the, ‘well what’s in it for us?’ question. ‘Why should we accept your project? That’s interesting to you, but why should we say yes?’ And I really didn’t have a good answer.”⁶¹³ Welch, like many other researchers, initially doubted how useful his project could be for the community. In his speech in the *warã*, he offered reasons, although almost ten years later they were hazy in his memory: “I talked about the general benefit of science, and ... you know, I don’t really remember at this point. But I had my answers, I just don’t think they were terribly convincing. And, certainly not

⁶¹² Welch, interview, 27 March 2014. See also James R. Welch, *Sprouting Valley: Historical Ethnobotany of the Northern Pomo from Potter Valley, California* (Denton: Society of Ethnobiology, 2013).

⁶¹³ Welch, interview, 27 March 2014.

as convincing as it would have been to say, ‘well here’s 10,000 dollars. That’s why’.⁶¹⁴

As unprepared as he felt, or as unconvincing as he initially thought his own arguments, he had no choice but to present his project to the men’s council.

Welch described the process of requesting community approval at some length in his dissertation. He offered an explanation to the assembled group in Portuguese, which Tsuptó, the chief, translated in its entirety. “Afterwards,” Welch wrote,

multiple people stood to deliver their own addresses while many others chattered on simultaneously. Then the Chief and Vice-Chief of the village, Suptó and Paulo [Supretaprã] delivered particularly long speeches, and several follow-up questions were posed to me. ... Finally, a very elderly man, Sereburã, stood and delivered a speech of his own. Gradually, the background voices diminished somewhat, leaving just two people who continued to speak in parallel to Sereburã. The three punctuated their deliveries with mutual affirmations, demonstrating that they were listening to one another as they spoke. When Sereburã sat down, the conversation ended.⁶¹⁵

Tsuptó informed Welch, rather simply, that the elders were in favor of the project; the *warã* agreed to his proposed yearlong stay in the village.

This early visit to discuss the project smoothed the regulatory process for Welch’s second proposed dissertation. Members of the village even took the step of advocating for the project directly to FUNAI. “They accepted my project,” Welch told me, “and they

⁶¹⁴ Ibid.

⁶¹⁵ Welch, “Age and Social Identity,” 31.

wrote a very concise and directive letter that basically instructed FUNAI to give me permission. I'm exaggerating, but in other words ... my original project was accompanied by a letter from the leadership saying 'We support this project and ask you to issue permission'.⁶¹⁶ The letter, from Tsuptó Buprewên Wa'iri and Paulo Supretaprã, on behalf of the *Associação dos Xavante de Pimentel Barbosa*, (the Association of the Xavante of Pimentel Barbarosa) declared their support to the President of FUNAI, specifying, "We inform you that our community is very concerned with environment issues, and for this reason we reiterate our invitation to James to carry out this project in collaboration with our community."⁶¹⁷ Welch reflected on just how different this regulatory process was from his experience of requesting permission to research in Rôndonia, saying, "I think is really indicative of a lot. It's indicative of how the Xavante relate to the government, and researchers, and Carlos and Ricardo and the amount of time that they have been there and the respect they have."⁶¹⁸ After years of political activism engaging FUNAI, and years of experience hosting researchers, the leadership in Pimentel Barbosa had the sense that it should not be up to FUNAI to decide who they should host. They knew enough about research and their own interests to make autonomous decisions, and as with other community decision processes, they had their own systems to determine what research they would accept.

Still, Welch depended on official regulatory approval, and Santos and Coimbra helped him navigate the myriad forms, procedures, and institutions involved in requesting

⁶¹⁶ Welch, interview, 27 March 2014.

⁶¹⁷ Supto Buprewên Wa'iri and Paulo Supretaprã to the President of FUNAI, Ofício no 004/AXPM/AB-MT/04, 20-May-2004, FF-SEP-PQ Caixa 767, Doc 12967, SEDOC-FUNAI.

⁶¹⁸ Ibid.

research authorization for a second time. After only a few months to prepare, the North American anthropologist arrived to begin his fieldwork in November of 2004.

Over time, Welch's notions of why the village had accepted his proposal grew. In addition to his initial perception of the villagers' high regard for the ENSP researchers, Welch came to see that less concrete factors were also at play: "I think it had to do with their evaluation of how I carried myself and went about things," he explained, "But I think it also had to do with their expectations of me that were not enunciated, that were never spoken." In order for these expectations to later be realized, the Xavante had to put in the affective labor of engaging the *warazú*, and establishing relationships with him.

Welch described his initial days of fieldwork in 2004 as a whirlwind of activity:

At least at the beginning, it was just busy, busy, busy. You think about "oh it's going to be peaceful and calm out in the forest doing research with this indigenous group," but I never used my watch alarm so much as I used it there. Because one person was "oh, I'll take you to my garden." And then the next person, "I'll take you here, I'll take you there. We'll do this and we'll do that.

And then the little bit of interviews and stuff that I was able to squeeze into all of this, it had to be all marked by hour. You know at 11 o'clock I'll do this, and at one o'clock I'll be there, and at two o'clock I'll be there. Anyway, it was just this schedule [snaps fingers four times] like this. Because they were demanding my time and requiring that I do all of the things that they thought that I should do. Which was awesome. But it was really busy. ...

I really felt like a piece of dust in the wind for a lot of the beginning.

In other words, I wasn't setting the priorities. I wasn't setting the agenda.

A lot of it was what people were expecting of me.⁶¹⁹

Much as Jurandir seemed to have a checklist of things that the Genographic researchers *ought* to learn, which turned their sampling trip into a crash course in Xavante culture, when Welch arrived in the village he had the sense that he was being guided through an almost pre-determined agenda. People had expectations for him, formed from their experiences with prior researchers and their understanding of what his project was about. Some of them, and Welch was careful to specify that there were many individuals in the village who were not interested, made particularly concerted efforts to engage the newest anthropologist.

Welch quickly came to see that a hunting trip or a garden visit was more than simply a lesson in Xavante ways. "Any opportunity to do something with me was a means of building a relationship," Welch explained, "I didn't realize to what degree I would be inserted into the kinship system, the social system. And so the people who were interested in having me as a son, or having me as a brother, or having me as a grandson, were all kind of out to make that happen."⁶²⁰ Shortly after he arrived for his extended stay in the village, Welch was invited to take part in the *wai 'a*, a spiritual initiation ceremony. It was his participation in these rituals that cemented his social position. "On the morning of that ritual," he wrote in his dissertation, "I was invited to a forest clearing, where men were rehearsing a song. While there, Valdo, who had taken me hunting, offered to

⁶¹⁹ Welch, interview, 27 March 2014. See also Welch, "Age and Social Identity," 30–33.

⁶²⁰ Welch, interview, 27 March 2014.

prepare my ritual ornaments. Flattered by the gesture, I readily agreed. I was then ignorant that because fathers often prepare their sons' ritual ornaments, Valdo preparing mine marked him as my adoptive father."⁶²¹

In addition to establishing his place in a family and the corresponding membership in the *poreza'õno* moiety, in the *wai'a* Welch was also assigned to an age-set. Already in his mid-thirties when he arrived in Pimentel Barbosa, Welch was indirectly "assigned" to the *êtêpá* age-set, who, at that time, were in the age-grade of novitiate adults.⁶²² Interestingly, by grouping the thirty-five year old anthropologist with youth in their late teens and early twenties, the elders and mature men ensured he would have privileged access to the perspectives of the age-range he was most interested in studying, although it is not clear if this was intentional.⁶²³ In his descriptions of the process of being incorporated into the kinship system, Welch emphasized his unwitting participation in interactions, the meaning of which would not become clear to him for some months or years, as he worked to understand the multifaceted nature of Xavante social belonging.

Welch, like a number of other researchers who had come before, and Vieira who came after him, had now passed through a second step of the loose system for managing

⁶²¹ Welch, "Age and Social Identity," 32.

⁶²² Ibid., 33. Welch described novitiate adulthood (*ritei'wa*) for young men as the period of (approximately) five years after they have completed their time living together in the pre-initiate house (*hö*), once they pass through formal initiation rituals and have been formally inducted into the spiritual life of the village. He was advised of his "belonging" in this group as people repeatedly directed him to go and join the members of this age-set as he was going about his daily life in the village.

⁶²³ Welch described this process writing, "I am not sure why, at 35 years of age, I was not assigned to the mature adult age grade or, as a newcomer to Xavante society, I was not assigned to the formative pre-initiate age grade, but perhaps it was because I expressed to village leaders my anthropological interest in young adult perspectives" (Welch, "Age and Social Identity," 89–90).

researchers. Welch's adoption, in part because of the formality of his participation in the *wai'a*, entailed a great deal of work. There was the effort that went into establishing personal connection through hunting trips, garden visits, and other invitation to explore the *cerrado*. The preparation of ceremonial ornaments, many of which involve laborious spinning of cotton, collection and making of *a'e* (beads), or the hunting of specific feathers, teeth, or hooves, likewise required labor. This was probably assumed not only by Welch's adoptive father, but by many members of the family. Welch's adoption and his initiation were only the beginning of the implied labor, because taking on a researcher meant being prepared to spend many hours explaining and teaching.

Once Welch was located in a family, an age-set, a spiritual grade, and moiety, this social position informed who would help with his research. "Much of my social interaction with the community was flavored by the collateral effects of assuming those social statuses," he wrote in his dissertation, continuing to discuss his privileged access to men's over women's experience in the fieldwork context.⁶²⁴ Members of his adoptive family were more likely to answer questions or help with language, or rally help to build and thatch the anthropologist's house.⁶²⁵ Members of his age-set explained the ritual practices that they performed; they "lent" the anthropologist songs they had dreamed so he (and by extension they) would not lose face when it came time for him to lead them in singing.⁶²⁶ He provided support, affection, entertainment, and Portuguese practice for

⁶²⁴ Welch, "Age and Social Identity," 201.

⁶²⁵ Ibid., 216.

⁶²⁶ Ibid., 218–219.

other members of his age-set and received support from them in return.⁶²⁷

This social positioning not only influenced what Welch learned, it also helped to distribute the labor involved in instructing the outsider. In an analysis of the relationship between mentors and their younger protégés, Welch described how his mentor—a man from two age-sets his “elder” —took special care to instruct him as he and his age-set mates went to learn how to prepare ceremonial ankle bands:

Being the most ignorant of our group and yet expected to perform the same basic roles as my age set peers, I was in need of special attention. One of my mentors, Josimar, noticed this and made a point of keeping tabs on me. ... He didn't force this lesson on me, but asked if I wanted to learn. He showed me once, then discreetly watched from nearby as I did it on my own several times. Once he was satisfied I was on the right track, he drifted away without a word. I found this an unimposing but thoughtful guidance style typical of mentors and their Xavante protégés. To me it signaled a special attitude of respect, responsibility, and intimacy.⁶²⁸

While Welch was specifically analyzing the mentor-protégé relationship in this passage, his words also belie the extra care that was taken to instruct the *warazú* as well as his own perceptions of mutual intimacy and respect.

The care and affection was not always so generously offered. Once Welch was inserted into the kinship system, he was also subjected to the competition and animosity

⁶²⁷ For example, *ibid.*, 33–34.

⁶²⁸ Welch, “Age and Social Identity,” 220.

of those who belonged to rival factions of the community. The age-sets directly above and below his, for example, as well *ōwawe* individuals without a mediating relationship (such as co-membership in an age set, or shared family members), sometimes were not receptive to his presence or accused him of limiting himself to “that side of the village,” meaning his immediate neighbors and closest adoptive kin.⁶²⁹

In his dissertation, and in the context of a broad discussion of how rivalry, pranks, and animosity characterize the relationship between adjacent age-sets, Welch detailed a number of instances where different individuals expressed their age-set-related rivalries. He also, for clarification, included a number of baffling experiences of being harassed that he later came to understand as extensions of these rivalries to him. One woman consistently criticized and berated him when he visited her home:

These uncomfortable encounters escalated over the course of several months, and after having lent my head as a depository for handfuls of her steaming wet rice, my back as target for her well-aimed watermelon rinds, and my food as sacrifices to her theatric thievery, I went to her husband José Paulo, to sort out whatever might be the source of our conflict. “It’s a joke!” he exclaimed. “It’s her prerogative to tease you because she’s on the other side.” ... I only came to enjoy visiting her house after I learned to take her public humiliation in stride, to not expect her friendship, and to be generous once in a while.⁶³⁰

This conversation and José Paulo’s use of ‘the other side’ to describe the rivalrous

⁶²⁹ Welch, interview, 27 March 2014.

⁶³⁰ Welch, “Age and Social Identity,” 230.

relationship Welch had with his wife informed the anthropologists' own understanding of how he was imbricated in the social system he was studying.⁶³¹ It also highlights how gift giving and generosity are central to both acceptance and resentment of researchers' presence in the field, which I discuss at more length below. Within the social institutions that he had been inducted into, Welch was expected to contribute, whether to his family, his age-set, his mentors, or those who worked closely with him for his research, with gifts and food. As many researchers experienced before and after him, Welch was expected—and explicitly reminded—to share his attention and gifts broadly, beyond his immediate adoptive family. When he was perceived to fail to do so, those who felt excluded might reprimand him with an insult—“you have the head of a tapir!”—or a handful of wet rice in the face.⁶³²

Welch was by no means the first *warazú* to be inducted into the kinship system and subjected to its sometimes-incomprehensible rules. As mentioned in Chapter 1, in 1958, Maybury-Lewis had been adopted as a brother by Apowě, and had been incorporated into an age-set (*airere*, the same age-set as Apowě).⁶³³ However, in the first thirty-five years of research, the formal inclusion of researchers in a moiety, age-set, and

⁶³¹ Welch also drew on examples of pranks as he began to train me to go into the field prior to my trip to Pimentel Barbosa Village. He laughed as he described one woman, many years his senior but also a member of an age-set “opposed” to his own, who sometimes held a urinating baby over his head; see Welch, “Age and Social Identity,” 230. I was not sure I would be able to be so good-natured about these pranks, and in fact did find it challenging, as I describe below.

⁶³² Welch, “Age and Social Identity,” 229–230.

⁶³³ As he explained in a footnote, by the end of his time in the village, Maybury-Lewis had not passed through the initiation rituals that all the others of his age set had participated in. This left him in an awkward position when the *wai'a* took place. Because of his uninitiated status, he was not allowed to witness certain portions of the event, despite the fact that he had been included in the activities of his age-set as a sponsor for the initiates. When the *wai'a* had mostly concluded, Maybury-Lewis was, himself, initiated in a small ritual, and told that he had to keep what he had witnessed secret from women and the uninitiated; see Maybury-Lewis, *Akwě-Shavante Society*, 262–263.

family was not as clearly articulated or standardized as it seems to have become in the twenty-first century. Other scientific visitors to spend a short time in the village were not incorporated into the kinship system. Such was the case for the geneticists during their 1962 visit. Nancy Flowers was never specifically instructed regarding her place in the kinship system. In spite of usual proscriptions of women's participation, she was allowed to witness parts of the men's rituals, but she was not included when an initiation ceremony for girls took place.⁶³⁴ She was once addressed the kinship term of "sister," and due to her close relationship with Apowẽ, she may have been seen as his adoptive daughter, but she was never given a name.⁶³⁵ Warodi adopted Laura Graham more officially and publically, and the close personal relationship that she formed with him was essential to both her linguistic analysis and to her sense of her place in the village.⁶³⁶

But during these years, adopting researchers was an uneven practice at best. Coimbra and Ventura Santos, even with all their work in the village in the 1990s and through the 2000s, were never adopted or named. Like Flowers, they were seen as closely tied to the *poreza'õno* moiety. It was following their early work, and with the advent of cohorts of graduate students who would arrive each year or two under their watch that incorporation into the kinship system seems to have become common for the researcher *warazú*. One of the first graduate students to conduct master's research in Pimentel Barbosa village under Ventora Santos' supervision in 1994, Silvia Gugelmin, was located

⁶³⁴ For a discussion of this ceremony and its disappearance with increasing contact with Brazilian national society, see Silva, *Nomes e amigos*, 130–131. On the other hand, Laura Graham reported in Pimentel Barbosa there was a women's initiation in 1985; see Graham, *Performing Dreams*, 92, 258. Welch writes that Flowers observed a girls' initiation; see Welch, "Age and Social Identity," 155.

⁶³⁵ Flowers, personal communication with the author, 16 September 2016.

⁶³⁶ Graham, "The Always Living," 30–31.

with a family for the duration of her stay. This placement was the result of a negotiation between Ventura Santos, Tsuptó, and Tsuptó's sister and brother-in-law who hosted Gugelmin, but Gugelmin was not named until her subsequent fieldwork in T.I. Sangradouro.⁶³⁷ Since the year 2000, however, seven researchers associated with the ENSP team have been named and adopted into a moiety, age-set, and sometimes a family, including myself.⁶³⁸

Stepping back to look at these patterns of interaction as a whole, it is clear the system the Xavante have used and continue to use to manage researchers became increasingly consistent over time. The Xavante invest substantial labor in finding common ground with researchers and drawing them into the village; they adapt existing institution of the *warã* to serve as a space to oversee, discuss, and publically agree regarding proposed research (at least among the male leadership); and they build mutuality through incorporating *warazú* researchers into their kinship systems. Xavante actors use these relationships to care for the researcher, while also distributing associated labor and gifts throughout the community. I am not suggesting that this system was comprehensively planned, charted out, and then purposely applied. Rather, it seems to be an approach that has evolved organically, where existing social institutions are extended

⁶³⁷ Silvia A. Gugelmin, interview with Rosanna Dent, 4 August 2015, Cuiabá, MT. As a young researcher, Gugelmin spent approximately a month in the field alone after Santos, Flowers and collaborators had left. Based on this initial research, she prepared her master's thesis, Gugelmin, "Nutrição e Alocação de tempo dos Xavante de Pimentel Barbosa." She conducted her subsequent doctoral research in T.I. Sangradouro; see Silvia A. Gugelmin, "Antropometria nutricional e ecologia humana dos Xavante de Sangradouro-Volta Grande, Mato Grosso" (PhD diss., Escola Nacional de Saúde Pública, 2001).

⁶³⁸ This list of researchers includes James Welch, Aline Ferreira, Rodolfo Mendonça de Lucena, Maurício Viana Gomes de Oliveira, Rosanna Dent, Hugo Genes, and Verônica Zembrzuski. Welch, personal communication with the author, 26 January 2017; Aline Ferreira, interview, 10 April 2014; Hugo Genes, personal communication with the author, 5 February 2017; Verônica Zembrzuski, personal communication with the author, 30 January 2017.

to the scientists who visit. As of the late 1990s or early 2000s, this loose system has functioned more and more consistently. These different aspects of Xavante management of outsiders were foundational to the Genographic researchers' sense of acceptance and inclusion. They shaped Welch's research, especially who accepted the responsibility of engaging with him and teaching him. In turn, when I arrived in the village for the first time in 2015, I passed through many of the same processes. And as he built relationships, like Santos and Coimbra before him, Welch's research interests developed in conjunction with Xavante interests. As I explore in the next section, obligation and esteem, fostered through the extension of familial belonging, guide scientists to new lines of inquiry and result in unexpected social, political, and scientific engagement.

Turning "Encanto" into Something More

Over time, Welch's research interests, as well as his process of developing new projects and plans changed. "What happened over the course of that year," he reflected, "is that I developed friendships with people. And then I started hearing what they were interested in and during that first year the whole idea of research proposals and authorizations from the community went from being a moment in time to just being an ongoing conversation."⁶³⁹ His doubts about the use of his research for the community began to dissipate as he started to make sense of some of those expectations "that were not enunciated, that were never spoken" in his process of requesting the community's permission to do his research. Xavante affective labor, and the social system to manage

⁶³⁹ Welch, interview, 27 March 2014.

research that Pimentel Barbosa offered had established the possibilities for Welch and his colleagues to explore new lines of inquiry. But they also provided the researchers with direction as to lines of inquiry that were important to the communities.

After enrolling the Welch, Xavante actors built common ground that might lead to their interests being advanced through interactions and scholarship. Welch's initial scholarly contributions were small; he worked to revise details from early ethnographic work that villagers had heard about and insisted were incorrect.⁶⁴⁰ One of the men who had originally supported Welch's research later articulated that this had been a major motivation for his support of the anthropologist's stay.⁶⁴¹

Other work that he took on through the ongoing conversation of his presence in the village contributed to a long line of scholarship instigated by community members. This research grew out of Welch's interest in ecological factors, which he had been unable to develop in as much depth as he had hoped in his dissertation. As he finished his doctoral analysis of age and social organization, one topic he turned to was fire hunting and its ecological impact. A series of conversations with one of the village leaders, Tsuptó, about the potential of Geographic Information System (GIS) research, and the "tangible frustration" of community members regarding *warazú* accusations that their methods for hunting with fire during the dry season caused deforestation developed into a

⁶⁴⁰ Welch, "Age and Social Identity." For substantial revisions of previous scholarship, see 73, 193–198, 272–283. Welch was emphatic that his informants were interested in correcting Maybury-Lewis' assertion that there were three "lineages," two of which combined to make one of the two exogamous patrilineal moieties. Although this may soon change, as of 2017, the vast majority of residents of Pimentel Barbosa are not reading ethnographic accounts of their community, even Portuguese-language publications.

⁶⁴¹ Welch, interview, 27 March 2014.

research project on fire ecology in the *cerrado*.⁶⁴²

However, the new anthropologist was not the only researcher to be enrolled in creating knowledge about traditional fire use and its ecological impact: In a 2014 article in *Human Ecology*, Welch cited a series of seven prior studies by other authors that inquired into the role of fire in issues of conservation.⁶⁴³ He described this enrollment to me as follows:

There is this huge amount of literature on the subject, actually. And it's not by chance. Most of the research on the relationship between *cerrado* ecology and Indigenous societies was done in Pimentel Barbosa. Not just in the Xavante [T.I.s] but in Pimentel Barbosa. And it's because they have been cultivating these relationships for years. I think it goes back to Franz Leeuwenberg, who was one of the first guys who got involved. ... The community has really been looking to develop those relationships, to have that research done. They love the idea of producing science about their reserve and about them.⁶⁴⁴

An accumulated depth of data about T. I. Pimentel Barbosa facilitated the production of more research. While Coimbra and Santos found themselves guided to broaden their focus to include chronic disease by the gentle insistence of village leaders, Welch realized he could build on common interests in GIS and ecology to produce scholarship

⁶⁴² Ibid.; James R. Welch, Eduardo S. Brondizio, Scott S. Hetrick, and Carlos E. A. Coimbra, "Indigenous Burning as Conservation Practice: Neotropical Savanna Recovery amid Agribusiness Deforestation in Central Brazil," *PLoS ONE* 8, no. 12 (2013): e81226. doi:10.1371/journal.pone.0081226; James R. Welch, "Xavante Ritual Hunting: Anthropogenic Fire, Reciprocity, and Collective Landscape Management in the Brazilian *Cerrado*," *Human Ecology* 42, no. 1 (2014): 47–59, doi:10.1007/s10745-013-9637-1.

⁶⁴³ Welch, "Xavante Ritual Hunting," 56–57.

⁶⁴⁴ Welch, interview, 21 April 2014.

in the face of ongoing public challenges to their traditional practices.⁶⁴⁵

It was some four years after Welch first arrived in the village for his doctoral research that he received a much larger and more directive request from his interlocutors. Accustomed to collect calls from the village pay phone, Welch called back one afternoon for what might have been a regular update from his research assistant, an invitation to an upcoming ceremony, or any number of things. This call from Tsuptó was different; he asked Welch if the ENSP research group would conduct a delimitation study for the as-of-yet undemarcated *Terra Indígena* Wedezé.⁶⁴⁶

The Xavante of Pimentel Barbosa were advocating for the return of an area of their territory that had a long history as an object of study and subject of revindication efforts. Wedezé was the region across the Rio das Mortes that Maybury-Lewis and family had visited and trekked in 1958. After SPI officials had negotiated the transfer of the village to Pimentel Barbosa in 1973, the government official responsible for the post convinced leaders to trade a large portion of the area for cattle and machinery in a questionable transaction with a local *fazendeiro*.⁶⁴⁷ During her fieldwork, Flowers had documented the tension and direct conflict with surrounding *fazendeiros* as well as the failure of the *warazú* dealmakers to keep their side of the agreements. Likewise, Flowers repeatedly noted Xavante complaints and anger at previous SPI, FUNAI, and other government employees. These officials misidentified boundary rivers on legal maps and issued illegal *certidões negativas* or “negative certificates,” declaring portions of Xavante

⁶⁴⁵ See Welch, “Xavante Ritual Hunting,” 56, and citations therein for a number of popular news articles alleging fire hunting caused deforestation.

⁶⁴⁶ Welch, interview, 27 March 2014.

⁶⁴⁷ Welch et al., *Na primeira margem do rio*, 45.

land as unoccupied by Indigenous people.⁶⁴⁸ During a period of particularly visible protests during the 1970s, various Xavante groups secured territorial demarcation or expansion by the government, but Wedezé was not among the recognized areas.⁶⁴⁹

Residents of T.I. Pimentel Barbosa maintained their intention to reclaim the area, and had requested a delimitation study in the late 1990s. The government accepted the proposal and the study commenced in 2000, but it met with repeated political roadblocks, including fierce resistance from the governor of Mato Grosso.⁶⁵⁰ By the end of the study, the FUNAI-appointed team had not succeeded in producing a sufficiently detailed and well-supported case to stand up in court. The Pimentel Barbosa leadership stepped away from the project when the president of FUNAI visited to tell them they should limit their claim to the northern or southern region of Wedezé.⁶⁵¹ After years of frustration and negotiation, in 2008 FUNAI officials agreed to appoint a new working group to conduct a second study, but estimated it would take a number of years before they would have staff and funding sufficient to address the request. A group of Xavante leaders asked whether the project would go forward if they could recruit their own researchers, and with FUNAI's agreement, they reached out to Welch, Coimbra, and Ventura Santos.⁶⁵²

“My first inclination was not to do it!” Welch told me in a good-natured tone, “I thought it would be a huge amount of time...” he paused, “which it was.” He took the

⁶⁴⁸ Flowers fieldnotes; Welch et al., *Na primeira margem do rio*, 44–45.

⁶⁴⁹ Garfield, *Indigenous Struggle*, 170; Welch et al., *Na primeira margem do rio*, 60–61.

⁶⁵⁰ Ricardo Ventura Santos, Carlos EA Coimbra Jr, James R. Welch, Jorge Luiz de Paula, Januária Pereira Mello, Sebastião Carlos Baptista, Luis Antônio de Araújo, and Hugo Meireles Heringer, “Relatório circunstanciado de identificação e delimitação: Wedezé, população indígena Xavante,” (Brasília: FUNAI, 2011), 17.

⁶⁵¹ Welch, interview, 27 March 2014.

⁶⁵² *Ibid.*

idea to Ventura Santos and Coimbra: “We decided together that it was important and it was the right thing and we probably had the best data to do it. We were probably the people that could produce a high quality report.”⁶⁵³ Coimbra identified the delimitation study as the biggest project that their research group had tackled that had been exclusively proposed by their Xavante interlocutors. “It was purely the Indians’ project,” he said. “It came from the Indians. Completely. By then they were extremely frustrated with the situation. We couldn’t say no, of course, so we did it.”⁶⁵⁴ The anthropologists accepted the invitation.

In 2008, Santos, Coimbra, and Welch joined a group of three FUNAI employees to complete the Wedezé delimitation study in collaboration with the eleven villages of T.I. Pimentel Barbosa. The formal process for legal demarcation of Indigenous Territory in Brazil begins with a multi-disciplinary delimitation study, which combines ecological, archeological, and anthropological expertise. First, individuals with state-recognized epistemic authority, usually framed in terms of training in the relevant academic fields, are appointed to a working group. They produce a delimitation study for FUNAI. This study then passes to the courts, where it usually faces legal challenge from landowners whom it affects. If the courts accept the study, the land becomes officially demarcated, with timelines for non-Indigenous occupants to vacate the land and return it to the

⁶⁵³ Ibid.

⁶⁵⁴ Coimbra, interview, 33.

Indigenous communities that can in turn occupy it. Indigenous groups do not have complete legal sovereignty over their lands, which remain under federal control.⁶⁵⁵

For the Wedezé delimitation study, the anthropologists were able to draw on their own extensive experience in the area. They also incorporated historical and primary source material drawn from the long line of researchers who had come before them. Here Flowers, who had fundamentally doubted her abilities to help the Xavante in the 1970s, was of particular importance. She offered her field notes as well as her expertise to help construct the 440-page report. Details of what she observed during the critical period of the late 1970s helped corroborate Xavante explanations of how they had lost Wedezé, and how important it continued to be for them. Maybury-Lewis' publications were also central in supporting community claims to the longevity of their connection to Wedezé. To complement the historical data, ENSP researchers were able to draw on publications and data sets that they and their students had produced over the preceding two decades. Work by Luciene Guimarães de Souza allowed them to make detailed demographic projections of population growth and corresponding need for additional territory.⁶⁵⁶ A large body of work on nutrition and documentation of how hunting and collecting continue to provide essential basic sustenance for the population of T.I. Pimentel Barbosa supported the Xavantes' claim that they depend on the area.⁶⁵⁷ Months of collaborative

⁶⁵⁵ FUNAI, "Entenda o processo de demarcação," <http://www.funai.gov.br/index.php/2014-02-07-13-24-53>, accessed 10 February 2017.

⁶⁵⁶ Santos et al., "Relatório Circunstanciado Wedezé," 10, 23; Luciene Guimarães de Souza, "Demografia e Saúde dos Índios Xavante do Brasil Central" (PhD diss., Escola Nacional de Saúde Pública Sergio Arouca – FIOCRUZ, 2008).

⁶⁵⁷ Silvia A. Gugelmin, "Nutrição e alocação de tempo dos Xavante de Pimentel Barbosa, Mato Grosso," (master's thesis, Escola Nacional de Saúde Pública, 1995); Nancy M. Flowers, "Demographic Crisis and Recovery: A Case Study of the Xavante of Pimentel Barbosa," *South American Indian Studies*, no. 4

work produced further evidence including ethnobotanical surveys, oral histories from elders, and technical surveying of cemeteries and ritual spaces. The study was comprehensive, and thoroughly backed up by years of respected research.⁶⁵⁸

The report led to the delimitation of 150,000 hectares of Xavante land in 2011, at a time when few new Indigenous territories were being recognized. With FUNAI's acceptance of the report, the process moved to the courts, where the proposal still faced legal challenges and a long road to official demarcation. However, the strong case that the researchers were able to build in cooperation with a community that knew them well was a major step towards the goal of demarcation. It was also an important affirmation of the commitment that the researchers had with the community that had hosted them and their students for almost twenty years. Santos reflected on the dynamic of invitation and acceptance to conduct the study saying, "When we went to prepare the material for Wedezé, they knew that ... we had the technical elements that were necessary for this kind of thing, to do the study. They knew that. And we reaffirmed our commitment to them. 'Let's do it. Of course we're interested!' So I think that there is this dynamic, of challenges to be faced [together]. And both sides have maintained this relationship over the course of all these years."⁶⁵⁹ Interlocutors in the villages of Pimentel Barbosa recognized Coimbra, Santos, and Welch not only as friends of the village, but as scholars whose authority would be recognized by the state. They differentiated the academics

(1994): 18–36; James R. Welch, Aline Alves Ferreira, Ricardo Ventura Santos, Sílvia A. Gugelmin, Guilherme Werneck and Carlos E. A. Coimbra Jr., "Nutrition Transition, Socioeconomic Differentiation, and Gender among Adult Xavante Indians, Brazilian Amazon," *Human Ecology* 37 no. 1 (2009):13–26.

⁶⁵⁸ This kind of expert-mediated recognition raises many questions about how Indigenous peoples must demonstrate their legitimacy to the state. See: Povinelli, *The Cunning of Recognition*; Simpson, *Mohawk Interruptions*.

⁶⁵⁹ Ventura Santos, interview

from other *warazú* as those most well prepared to present data on territorial claims to Wedezé.

While the invitation to conduct the study may not have been on the researchers' minds, it had been simmering for a long time within the host community. In fact, Welch learned, it may have been a factor for accepting his presence in the village, years before he had the knowledge or position to take on such a complex project: "I found out later on that one particular person, when he supported my coming, he was hoping that one day I could help with the land fight for Wedezé."⁶⁶⁰ Welch had fulfilled one of the unenunciated hopes that his hosts held for him.

For the researchers who participated in the delimitation study, their sense of professional responsibilities intersected with their imbricated social relations with residents of T.I. Pimentel Barbosa. As long as they had been working in Indigenous communities, the scientists' ethical codes compelled them to plan and execute research that would be of benefit to the subjects of their research. However, the delimitation study represented their largest undertaking that responded directly to a community request. They conceived of and carried out previous projects based either on their own research agendas or on hybrid projects, formed through consultation with villagers. Much of their previous scholarship had contributed to making plain the plight of community health challenges, providing baselines for attention from later public health initiatives, or providing care in the context of the research itself. In the case of the Wedezé delimitation, the researchers were asked to dedicate months of their time and they readily

⁶⁶⁰ Welch, interview, 27 March 2014.

accepted. While it is possible that they would have accepted without such a longstanding relationship with the village, Coimbra's sense that "we couldn't say no," underlines the sense of obligation that the researchers felt.⁶⁶¹ In part, this dedication had been cultivated by the investment of Xavante leaders and community members in the researchers, and by the village's ongoing work to find common ground to continue collaborating.

Surprise, Anger, and Greif: On Being Affected

As I was writing about the personal experiences that Santos, Vieira, and the ENSP researchers had shared with me, I was aware that I was presenting a vision of research that focused primarily on the positive. The researchers' enthusiasm, whether in response to new affective ties or more enduring ones, inflected my recounting. But other more complicated exchanges and tensions have also been central to researchers' presence in T.I. Pimentel Barbosa, as reflected in texts from Maybury-Lewis, field notes from Flowers, and accounts in a host of interviews. There were affective elements that I was failing to capture—loneliness and anger, for example—that were related to the resistance and confrontation also inherent in research interactions. Researchers experienced intense frustration, and at times their presence was highly disruptive for villagers. In earlier drafts

⁶⁶¹ Anthropologists in Brazil have frequently been called upon to provide different kinds of technical reports, either by Indigenous or Quilombo communities for land delimitation, or in criminal cases where charges against Indigenous individuals are tried in concert with the 1973 *Estatuto do Índio*. For a discussion of broader ideas about anthropological activism in Brazil, see Chapter 4. For a critical overview of the conflicts this positioning produces, see João Pacheco de Oliveira, "The Anthropologist as Expert." For a discussion of anthropological reports in criminal cases, see Bruce Granville Miller and Gustavo Menezes, "Anthropological Experts and the Legal System: Brazil and Canada," *The American Indian Quarterly* 39, no. 4 (2015): 391–430. For a broader overview of land rights in South America as of 2005, see Anthony Stocks, "Too Much for Too Few: Problems of Indigenous Land Rights in Latin America," *Annual Review of Anthropology* 34 (2005): 85–104.

of this chapter, I struggled to integrate my own story into the text—in fact, my experience in the village was so different from the other portrayals in this chapter, I wondered if my analysis could hold.

I was unsure how to reconcile the fact that some parts of my experience had been so difficult, and yet I still felt affective ties and moral obligations to the people who had hosted me. I had been effectively drawn into relationships, even as I doubted at times how sincere they might be or how useful my participation could be for the community. My doubt and confusion was compounded by the long history of colonial imbrications, appropriation, and misrepresentation that non-Indigenous scholars have often perpetrated in their research with Indigenous communities. Fieldwork in the village was shrouded in a kind of fog of unease and misunderstanding, and yet it was compelling even as it was troubling.

It was the saddest moment of my research that crystalized my vision of how this affective labor had extended to me and how its effects endured even after my departure from the village. Early one morning as I struggled with this chapter, I received bad news from the village. Sidówi Wai'azase Xavante, the man I called by his Portuguese name—Barbosa—had passed away. Just a week before, Welch had told me of Sidówi's probable cancer and his decision to forego chemotherapy in favor of alternative treatments. I had not realized how dire the prognosis was. I was overcome by sorrow. And simultaneously I was incredulous at my own reaction. Sidówi had been warm, caring, and enthusiastic. He was the person to tell me I would “do important work”—both an imperative and a vote of confidence. He was my adoptive uncle. But I had only spent three and a half weeks in the village. Did I deserve to feel this loss so acutely? As I put the pieces

together of my relationship with Sidówi, I realized that my experience in fact fit closely with those of Santos, Vieira, and Welch. It was Sidówi whose affective labor most defined my experience.

In my earliest meeting with community members from Pimentel Barbosa village in June 2014, Sidówi spoke first in favor of my work. In those earliest interactions, the elders and Tsuptó had already begun the affective labor of setting me at ease, of finding common ground, and of telling me I would be welcomed. A year later, when I arrived with a collection of scanned images and new equipment for the documentation center, their work intensified. I was to be ushered through a similar system to that which Welch and the Genographic researchers had experienced. My short time in the village, however, plays a different role in this chapter for two reasons: my gender made for a different kind of insertion into Xavante spaces, and my own position allowed me to reflect on many of the difficult and unpleasant aspects of field work often left out of oral history narratives.

By the time I traveled to Pimentel Barbosa, I thought I had a fairly good handle on what researchers' first days in the field might be like: disorientation, a buzz of activity, a sense of loneliness, struggle to get to know people from the community, and frustration with attempts to learn Xavante words and names. I also knew that as a woman my reception in the village would be different from that of Santos and Vieira or Welch. I was not aware quite how much of a difference it would make.

The day I arrived, the leaders who had met with me in the town of Água Boa were occupied in a three-day meeting with government officials at the school. Another man, who identified himself as a relative of one of the men I knew, took it upon himself to show me around. I felt deeply apprehensive as we walked around the village and he

introduced me at each house, thinking about how I might become embroiled in local politics simply by these introductions.

My unease increased as he suggested that I could be his third wife, and proposed, rather explicitly, that we *namorar* (be romantically involved). I brushed off his suggestions as if they were a joke, until he became very insistent and I told him pointedly to leave me alone. Later, after apologizing, he offered to take me to the school and post where the FUNAI meeting was taking place. There he introduced me to a leader from the neighboring village, Etênhiritipá, and the two men had a conversation in Xavante, discussing moieties and appearing to size me up. “But she is wearing blue,” the leader from the other village said in Portuguese, referring to my shirt and a color associated with the *poreza’õno* moiety. “No, she will be *õwawe*,” my host responded. It dawned on me a few days later that my moiety belonging would determine if I was marriageable to this man. My adoption would settle the question—I became *poreza’õno*—and he made no further advances.

The evening of my arrival, the effusive welcome of the men I had met in Água Boa began to calm my nerves. But this first interaction set me on edge. Over the next days, several young men from Pimentel Barbosa and one visiting from another Xavante territory made statements or insinuations in Portuguese that ranged from very threatening to innocuous.⁶⁶² Settled in a tent inside the documentation center, I felt vulnerable as a woman alone. To make matters worse, the building sat in the center of the horseshoe-shaped village, away from the circle of houses. My discomfort was compounded by the

⁶⁶² It is worth noting that rural Mato Grosso is not a feminist haven, and I was also approached very directly by non-Xavante men in Água Boa, but there I felt less isolated and more certain of how to respond.

fact that, like other researchers, the majority of my interaction was with men and took place in the center of the village—the men’s space.⁶⁶³

Another aspect that I found unsettling, although less surprising, was the question of gifts. Prior to arriving, I had read extensively about the importance of gift giving in research relations, a particular point of tension for many scholars as well as an important analytic tool.⁶⁶⁴ In the 1950s–1980s materials goods and funding for development projects distributed by the government fueled conflict and splits in villages.⁶⁶⁵ Gifts from researchers had the potential to do the same, even if their scope was much reduced as compared to that of the government agents.⁶⁶⁶ Speaking with other researchers, I heard rumors or allusions to projects or gift distributions made by scholars or other *warazú* engaged in cultural production that had contributed to these kinds of splits in T.I.

Pimentel Barbosa. Although I had difficulty finding concrete evidence of this, it seemed

⁶⁶³ Both Nancy Flowers and Laura Graham discussed how their unusual position as women without partners or children informed their experience, resulting in their being seen as masculinized or male-like. Flowers, interview, 22 August 2013; Graham, *Performing Dreams*, 16. This was not my experience, although I stayed for a very short time compared to either of them. However, it was interesting that I was consistently interpreted through memories of their visits (the two North American women to have done extended work in Pimentel Barbosa). I was more often called “Laura” than “Rosanna” during my time in the village.

⁶⁶⁴ The classic anthropological studies on reciprocity and exchange grew out of Bronisław Malinowski’s work in Melansia. Marcel Mauss’ essay *The Gift: Forms and Functions of Exchange in Archaic Societies*, trans. Ian Cunnison (Martino Fine Books, 2011).

⁶⁶⁵ Garfield, *Indigenous Struggle*, 202–204; Graham, *Performing Dreams*, 50–55; Lopes da Silva, “Dois séculos e meio de história Xavante,” 377–78. See also Chapter 1 on Maybury-Lewis and gift giving, and Chapter 3 on factionalism and SPI distribution of material goods.

⁶⁶⁶ Researchers’ presence can also provide a flashpoint for existing political debates without involving material exchange. Welch reflected in his dissertation on how his ear piercing became a point of contention regarding ceremonial authority in the village. It precipitated a boiling over of pre-existing community tensions, leading the village to divide into two in 2006. Welch discussed these events at some length and expressed his regret about the conflict: “It is with some degree of apprehension that I lay bare those facts in this dissertation, although I know I could not have anticipated them and believe that had my ear piercing not precipitated the conflict, some other event or events would have done so soon thereafter. That view is maintained by all of my Xavante contacts on both sides of the division, who express that I was an unsuspecting pawn in political affairs beyond my control,” Welch, “Age and Social Identity,” 322.

likely since research and cultural production projects are an important source of resources and prestige in the community. This accentuated my anxiety.

Maybury-Lewis wrote consistently in *The Savage and the Innocent* about his frustration, first during his research in Xerente villages, and then in his time in Wedezé and other Xavante villages, with the demands for a constant stream of gifts (see Chapter 1). Geneticists Neel and Salzano had brought a long list of presents, which they offered at the beginning of their time in the field (see Chapter 3). Nancy Flowers' field notes were filled with references to buying and distributing gifts, and she took the strategy of sharing the majority of her material goods shortly after her arrival or after each trip to town, building up credit, as it were, with her hosts and hoping it would last her until her next chance to buy gifts. While this was a foundational system of interaction for all *warazú*-Xavante relations in the first decades after permanent contact was established, sources from the 1980s on made much less mention of gift exchange.

During formal interviews, researchers who visited more recently commented only briefly on gift giving. Few researchers talked to me about gift giving in any substantive way, and few recent publications included any mention of it.⁶⁶⁷ I developed the impression that as the years went on, material goods came to play a smaller role in Xavante interests in research, while emphasis on politically relevant projects increased.

⁶⁶⁷ Welch mentioned that part of the incentive to build relationships might include an interest in reciprocal gift exchange, and he said that anyone who goes to the village will likely participate in some kind of informal exchange, as is usual in Xavante culture (Welch, interview, 27 March 2014). In contrast, Santos emphasized that CONEP explicitly prohibited the Genographic project from offering anything that might be interpreted as material enticement or compensation for participating in the study. Apart from one anecdote about trading tee-shirts with someone in the village, he made no other mention of gifts (Santos, interview, 3 March 2014).

It was not until I had firm plans to travel to Pimentel Barbosa village that my perception changed. Much as fieldwork is a foundational aspect of a sense of anthropological identity, it was an essential aspect of my inclusion in the broader community of *xavantólogos*. When it was clear to other researchers that we would be able to compare personal experiences, that we were imbricated in the same complex networks of adoptive kinship and messy obligation, a whole world of stories about exchange and reciprocity opened to me. Moreover, once in the village, I realized that the food, coffee, and presents and how I shared them would be as fundamental a daily concern for me as it was for Flowers during her stays in the 1970s.

This became clear almost the instant I arrived. As I first slid my boxes to the ground from the back seat of the taxi that dropped me off, a small group of people was already watching. An elderly man with a walking stick came forward. The younger man who first greeted me, gestured to the boxes and asked, “Do you have something for my father? Some sweets? Some biscuits?” I felt a wave of relief—I had prepared exactly for this kind of interaction. Right at the top of a box, I had a bag of *rosquinhas* (a simple cookie) big enough for twenty people. I reached for it and went to open it to offer some to everyone who had gathered. “Não abre, não. Todos,” the old man instructed me. “Don’t open them. All.” And he took the bag, turned on his heel, and walked away. This was Sereburã. A few days later he would adopt me.

As soon as Sereburã made off with my *rosquinhas*, I was navigating the politics of building relationships. Although I had been advised by other researchers to wait to give most gifts towards the end of my stay, I had also heard them speak with special affection about Sereburã and how important he had been for facilitating researchers’

work. I took to stopping by his house in the morning with coffee, since he did not come visit me in the mornings as Ventura Santos and Coimbra had predicted. On my third day, however, he came to me in the afternoon when no one was around. He asked me if I had any warm clothes. I fished out a red sweatshirt with a hood, the largest one that I had found in town, and he tried it on. Snug, but sufficient, he chastised me for the color (red being associated with the *öwawe* moiety), took it off, slipped it into his bag and headed home.

The next morning, a group of elders gathered on the front steps of the *centro* to talk with me about my plans.⁶⁶⁸ After I re-introduced myself, I presented the donations I had brought for the *centro de documentação*, including a laptop computer for community members to use for the ongoing audio-visual production. I explained again my interests in collaborating on the *Museu do Índio* project and my work thus far to recuperate photographs and other materials from researchers who had worked in the village over the years. Serebruã was the first to speak. He said that the elders valued this work to *resgatar a história* (to recover history) and recuperate documentation. He added that he would take care of me, and consider me like his daughter.

At the time, what turned out to be my adoption seemed almost unilateral to me. Sereburã made this decision after I had been in the village for only four days. I had no idea if there had been discussions between different members of the community in the lead-up to the adoption. Although in retrospect I recognized that I had also invested in my

⁶⁶⁸ The night before, after the three-day government meeting was over, I had been introduced at the *warã* where I had talked briefly about my work and the *Museu do Índio* documentation project. At that time, the men present said that they wanted to meet with me during the day when they could see me to hear more about what I wanted to do.

relationship with Sereburã, at the time I wondered if I was only a source of presents, coffee, and biscuits, or if my work was of actual interest in his eyes. But the adoption established formalized support for me—in the sense that someone had promised to look out for me—at a moment when I felt vulnerable, alone in the village. It also meant that Sereburã’s brother, Sidówi, would be my uncle. This relationship would prove necessary to my work and even more so for my sense of affective ties.

After a week in the village I had made very little progress on the documentation project. Predictably, given that I had just arrived, I felt displaced and unable to read basic cues about whether my presence was welcome or not. I was still feeling very uncomfortable with the comments I received from men. In a moment that felt like desperation one night, I spoke with Sidówi. I confessed my discomfort and my frustration. I said that I did not feel secure/safe/certain [*segura*], and that I was not convinced that people in the village wanted me to be there, that it seemed I had not brought the right kinds of gifts. I cried. Were people dissatisfied with my being there? Should I leave? I was asking for the most basic kind of help—to understand social cues that might have been obvious to others, but were not to me as a child-like visitor. I wanted direction, and Sidówi responded with encouragement and care.

Sidówi insisted I should stay. He insisted he was furious at the young men: he would talk at the *warã*; he would talk to Tsuptó; he would tell them off. He offered to advocate for me however would help. In that moment, Sidówi extended care to me that made it possible for me to stay.

I am not sure exactly what he did, but in the next days, suddenly everything seemed to be moving. Two young men showed up to help me with my attempts to learn

Xavante. The elders agreed to come and see the images I had brought, sitting to record memories about Flowers' photographs from the 1970s that we could use for Xavante-language classes at the school. The mask project, which the *Museu do Índio* had commissioned, and the community intended to document for the archive project, was suddenly underway. It was two days after I spoke with Sidówi that he sat to look at images for the archive, and told me "You are going to do important work."

Sidówi's speech to me about the importance of the researchers who had come in the past, which opened this dissertation, must be interpreted in this context. His caretaking, his "emotional and practical acts," enabled my presence in the village and have since sustained my intention to continue collecting publications, images, and recordings for the digital archive. In upholding the value of prior researchers' work and affirming his affective ties with them, Sidówi also affirmed his ties to me. He dreamed a Xavante name for me, and called me his niece and daughter.⁶⁶⁹ I felt safe.

In the following weeks, our relationship would continue to be defined by the coexistence of competing desires, interests, and agendas. Sidówi, (who Flowers once described saying, "...and Barbosa [Sidówi] likes gifts!")⁶⁷⁰ would ask me to bring him a digital camera, and ask if the *Museu do Índio* project could help the village buy a truck. He stopped by for coffee. I depended on him and his kin for help in almost every aspect of my work with the documentation project. Our relationship was imbued with power dynamics that lay along intersecting lines of age, gender, economic resources, and status. I had access to material goods and mobility that far exceeded my hosts in the village.

⁶⁶⁹ The word in Xavante is the same for niece and daughter.

⁶⁷⁰ Nancy Flowers, discussion with the author, 4 February 2017, New York City.

Sidówi, and others who interacted with me, were exploring how they could engage me to gain access to some of those resources, but while this practical interest at times invoked frustration, anger, and guilt on either side of the relationship, it did not undermine genuine affection, care, and laughter.

Like my work in the village, this account of my affective experience is only preliminary. But these initial experiences help make intelligible my sense of grief over Sidówi's passing, while bringing into relief the extent and variety of labor and embodied caring that Xavante actors take on in adopting researchers.

Conclusion

The village of Pimentel Barbosa offers little by way of built infrastructure for scientific research: there are few *physical* signs of the scholarly productivity of the space. In combination with a deep intellectual foundation—a wealth of past studies and data sets—T.I. Pimentel Barbosa has become a hub of scholarly attention due to the (mostly) invisible labor that villagers commit to the cultivation of relationships.

A few individuals like Sidówi and Tsuptó take leadership in this engagement. But communal memory also profoundly influences how researchers are guided through their visits. What has become a semi-standardized system for enrolling and caring for researchers directs the flow of research materials, whether audio recordings, gifts, blood samples, photographs, anthropometric measurements, or ecological data. It also directs researchers' attention, sometimes to the topics their subjects think will be of interest, and sometimes to topics their subjects wish to be the focus of study. As mentioned in Chapter 1, Flowers was instructed to take notes in addition to photographs; Coimbra and Ventura

Santos were pressured to open a new line of research into metabolic disorders; Welch's interest in ecology was leveraged for a study on fire hunting. In Pimentel Barbosa, to host researchers is to hope for a generative and open-ended relationship. Engaging in research is a practice of future-thinking; having faced and facing ongoing challenges in health, education, and especially territory, Xavante interlocutors *hope* researchers might fulfill their obligations of mutuality.

This chapter shows that Xavante interlocutors work towards this hope through the construction of an affective field. Community members shape knowledge production by participating in it and by engaging with the hopes, desires, and fears of the scholars who come to study them. This is not to say that there are never moments of refusal—questions are avoided, researchers are turned away from certain topics, projects are allowed to perish in inaction. However, actively working with scholars is also an exercise of agency by the Xavante, even within a context of unequal access to power, knowledge, resources, and understanding. As Sherry Ortner points out, citing Laura Ahearn, the point “is not that domination and resistance are irrelevant, but that human emotions, and hence questions of agency, within relations of power and inequality are always complex and contradictory.”⁶⁷¹ But within this complexity, care and affective connection are both possible and imbued in research relations.

The interactions of fieldwork are opaque, and can bring frustration and displeasure. They imply risks—of exposing private or privileged knowledge, of being misrepresented or of misrepresenting, of getting things wrong. These risks are part of

⁶⁷¹ Ortner, *Anthropology and Social Theory*, 138.

what makes affective labor so important, not only on the part of the subjects, but as a moral imperative for researchers. Inherent in these interactions is a form of caregiving. Although often unequal, at different moments both researchers and subjects practice care of the other.

Epilogue

It was Saturday night after dark and men began showing up at the *centro de documentação* in Pimentel Barbosa village. I had anticipated spending the evening alone, and was somewhat surprised when they started filing in. The elder who came in first set down his plastic chair with great purpose and set his eyes on the whitewashed wall. A younger man arrived next and explained that in the *warã* Tsuptó had announced that I would be showing the digital scans that I had brought back to the village. Happy to oblige, even though I had not been expecting everyone until the next day, I hooked up the projector to the jury-rigged electrical line running from a neighbor's house. A group of some ten men assembled, folding their arms across their chests and waiting. As more trickled in, I offered an explanation of the origin of the images. These were photographs from researchers' books and personal papers, collected from Porto Alegre, São Paulo, Rio de Janeiro, Philadelphia, and New York. In returning the images, I was working to fulfill a request from a group of elders who had expressed their interest in retrieving the documentation that had dispersed with the researchers who had visited over the years. I was also demanding their help with another kind of affective labor, the labor of memory—thinking about the past and remembering the dead.

Projected on the wall, I clicked through the black and white images. I began with the rather familiar ones from David Maybury-Lewis' books. Next I opened the first of the photographs from the 1962 genetics study, taken by physical anthropologist Friedrich Keiter. They were a series of portraits, or photographs that had become portraits, morphing from their original purpose as tools for anthropometric study. Apowẽ, Subject

01, stared out at us, illuminating the wall. “*Homem perigoso*,” one of the elders said in an irreverent tone. “Dangerous man.” The room filled with chuckles.

Apowê’s photograph indexes the history of research in Xavante territory. In its transnational circulation, continued relevance, and recent return to Pimentel Barbosa, the image draws attention to the ways research in Xavante territories has changed since the mid-twentieth century, and also to some striking continuities. In Christopher Pinney’s words it is a “complexly textured artifact,” like many other colonial and post-colonial images that underscores the “mutability of interpretational frameworks and potentialities.”⁶⁷² Both a scientific artifact and social object, it binds together many kinds of knowledge making, memories, and research subjectivities. Over the course of fifty years, it traveled from its site of production in Wedezé over national borders and through a photo lab and a genetics lab, into a 1964 issue of a scientific journal, from the personal archive of one researcher to another, through digitization and editing, and finally came to me on a hard drive to be returned to the community in 2015. The image is the product of scientific practices, of creating typologies and characterizing populations for the study of health and genes. However, the photograph and the new meanings it accrues are also a product of social relations. Apowê’s reputation as a dangerous man still clings to his image, whether under analysis in a genetics lab or as residents of Pimentel Barbosa village perform the affective labor of receiving and recuperating the digital file.

As this project has emphasized, Apowê was one of many community members whose personal and political relations with *warazú* shaped knowledge production about

⁶⁷² Christopher Pinney, “Introduction: ‘How the Other Half...?’” in *Photography’s Other Histories*, ed. Christopher Pinney and Nicolas Peterson (Durham: Duke University Press, 2003), 5, 14.

human variation. As a research subject, he became a focal point for Maybury-Lewis to make sense of political rifts, moieties, and the kinship system. Apowê was classified and quantified—rendered through anthropometry, immunoglobulin analysis, blood groups, and dermatoglyphics. He crystalized James Neel’s and Francisco Salzano’s ideas about human microevolution and their fission-fusion hypothesis. Apowê became an exceptional research subject because he was an exceptional figure in Xavante history—he had been credited with accepting peaceable contact with Brazilian society (or initiating it, depending on who is telling the story); he was instrumental in the (relatively) successful campaigns to demarcate T.I. Pimentel Barbosa over years of incursions and government-sanctioned arrogation. It was also his care and interest in hosting researchers that helped make Wedezé and Pimentel Barbosa into a space for the study of everything from public discursive practice to hemoglobin levels and malaria incidence.

Apowê played a role in creating the affective field that has defined researchers’ experiences, and helped established the tradition of influential male leaders taking care of academic *warazú*. When Nancy Flowers looks at an image of the late Xavante leader, she may remember not only his “disproportionate” genetic contribution to the next generation or the strength of his political engagement, but also his love of sweets and his purple pajamas.⁶⁷³ It was Apowê’s eldest son, Warodi, who adopted Laura Graham and was immortalized through his work with her to render his dream narrative legible across the linguistic divide.⁶⁷⁴ Warodi’s nephew and Graham’s collaborator, Jurandir, went on to

⁶⁷³ Flowers, fieldnotes, 12.

⁶⁷⁴ *Performing Dreams* is still taught regularly in university courses throughout North America twenty years after its publishing, which I imagine would please Warodi immensely.

enthusiastically host the Genographic project. Other community members who watched Apowê and Warodi would also cultivate research relationships, although not all community members had the same interest or opportunity to establish these economically and socially productive relations. Sereburã, Sidówi, and Tsuptó worked particularly closely with Flowers and the ENSP researchers, and they also extended their care to me. Like Apowê, their biosamples, images, ideas, and knowledge have been incorporated into the epistemic projects of visiting scholars, and like Apowê, they found value in the sociality of research.

The photograph and its travels hint at two parallel afterlives of this scientific-social object, which emphasize the changes and continuities in research practice since Apowê was first studied. In 2017 it still has potential futures as an object of scientific research or as an object of historical research and memory. Each afterlife has yet to be fully realized.

As of 2015, Apowê continued to animate the laboratory that Francisco Salzano founded at the *Universidade Federal do Rio Grande do Sul* (UFRGS). Geneticist Maria Cátira Bortolini, who took over Salzano's lab when he officially retired (although he continued to work forty hours a week even in his mid-80s), spoke with particular energy about the Xavante and their genes: "The Xavante always attract our attention. It's not that we choose the Xavante. ... From the point of view of population genetics, the Xavante always bring us surprises, scientific challenges."⁶⁷⁵ As Bortolini characterized this inevitable draw—a charismatic gravity—she used me and my research as an example:

⁶⁷⁵ Maria Cátira Bortolini, interview with Rosanna Dent, 18 August 2015, Porto Alegre.

“But you saw what happened with your work?” she said, “You were researching, and you slowly changed course—you ended up with the Xavante.”⁶⁷⁶

In genetic analysis, according to Bortolini’s account, the Xavante have consistently stood out from other Indigenous populations including other groups from Central Brazil that also speak Jê languages. As she explained it, her research group has continued to trace the genetic differentiation of the population back to interactions of chance—which led to some men having an unusual genetic profile—and culture, which led those men to have more children due to their political positions of influence.⁶⁷⁷

Apowê continues to be cited by name in conversations about this research.

As Bortolini and Salzano’s research team has turned increasingly to collaborative studies with a Mexican laboratory focused on gene variation and cranio-facial morphology,⁶⁷⁸ Apowê’s photograph could potentially continue to serve as an object of research for the lab. Although existing techniques had yet to accommodate the lack of standardization in the historical images, when last I discussed this with the geneticists, it is possible that the 1962 photograph could be incorporated into future studies using

⁶⁷⁶ Ibid. My original proposed project was a history of human genetics in Brazil without an expectation to focus specifically on one Indigenous group, or to broaden my attention to many disciplines. Maria Cátira Bortolini had supported my research from that early proposal, and watched my attention be drawn in by the case of the Xavante and the *xavantólogos*. The original Portuguese quotation was, “Mas tu viste uma coisa que aconteceu no teu trabalho? Que tu foi direcionando, foi direcionado e acabou nos Xavante.” I have taken some liberty in the translation to convey the sense of the changing direction or focus of my attention, which is what I believe she was emphasizing.

⁶⁷⁷ Bortolini, interview 18 August 2015. In that conversation, Bortolini cited a 2012 paper as an example. See: Tábita Hünemeier, Carlos Eduardo Guerra Amorim, Soledad Azevedo, Veronica Contini, Víctor Acuña-Alonzo, Francisco Rothhammer, Jean-Michel Dugoujon, et al., “Evolutionary Responses to a Constructed Niche: Ancient Mesoamericans as a Model of Gene-Culture Coevolution,” *PLoS ONE* 7, no. 6 (2012): e38862, doi:10.1371/journal.pone.0038862.

⁶⁷⁸ Hünemeier et al., “Cultural Diversification Promotes Rapid Phenotypic Evolution in Xavante Indians”; Jorge Gómez-Valdés, Tábita Hünemeier, Mirsha Quinto-Sánchez, Carolina Paschetta, Soledad de Azevedo, Marina F. González, Neus Martínez-Abadías, et al. “Lack of Support for the Association Between Facial Shape and Aggression: A Reappraisal Based on a Worldwide Population Genetics Perspective,” *PLoS ONE* 8, no. 1 (2013): e52317.

digital analysis techniques.⁶⁷⁹ But Bortolini has yet to visit Xavante territory and try her hand at the reciprocal obligations of fieldwork. While she articulates a sense of debt to the subjects whose genes she studies, her hopes that genetic research will lead to future health gains are abstract. She has not developed her research agenda in the context of fieldwork, where Xavante subjects apply their own strategies to enroll and direct their researchers. While another laboratory in the department invited Tsuptó and Paulo Supretaprã to visit as part of their collaborative study with Coimbra and Ventura Santos on genetic susceptibility to tuberculosis,⁶⁸⁰ Salzano and Bortolini have not, to my knowledge, invited Xavante leaders to learn about their laboratory's ongoing work.

In the absence of current discussions with Xavante subjects about the use of old research materials, the laboratory's copy of Apowẽ's photograph continues to serve as a kind of "immutable mobile,"⁶⁸¹ a document that connected a network of scientists, enabling abstraction and a common language about human variation, ostensibly subsuming the individuality of the subject and coming to represent the population. The image is a material reminder of the continuing influence of notions that scientists developed in the mid-twentieth century about the nature and culture of the Xavante, and by extension about human evolutionary history. Apowẽ, however, is difficult to subsume. He continues as a genetic icon.

⁶⁷⁹ Tábita Hünemeier, in conversation with Rosanna Dent, June 2014, Porto Alegre.

⁶⁸⁰ Mara Hutz, interview with Rosanna Dent, 7 July 2014, Porto Alegre. The findings from this research study were reported by Verônica M. Zembrzuski, Paulo C. Basta, Sidia M. Callegari-Jacques, Ricardo V. Santos, Carlos E. A. Coimbra, Francisco M. Salzano, and Mara H. Hutz, "Cytokine Genes Are Associated with Tuberculin Skin Test Response in a Native Brazilian Population," *Tuberculosis* 90, no. 1 (2010): 44–49, doi:10.1016/j.tube.2009.11.002.

⁶⁸¹ Bruno Latour, "Visualization and Cognition: Thinking with Eyes and Hands," *Knowledge and Society* 6 (1986): 7-14.

The other as-of-yet unrealized afterlife of Apowē's image is as an object of memory making. In this process, it is the mutability of the photograph that becomes clear. In this afterlife there are two intertwined processes: my own work to trace the path of the photograph through time and through its varied uses, and the work of residents of Pimentel Barbosa village who are faced with the affective labor of receiving and making sense of the image, incorporating it into a new form of archive that is still to be determined. The two are interrelated; it was once I was already enrolled in the digital archive project that the photograph and its mutations came to my attention, and in turn I participated in its initial but as-of-yet incomplete return.⁶⁸²

Pinney suggests this process might be understood as “recuperation,” which “takes the form of a homecoming: the naming of the formerly anonymous, the individuation and recognition of persons whose work in the archive had usually been to ‘typify’—that is, to exemplify some category.”⁶⁸³ The labor is doubled with this kind of return: first to remember and recognize, and second to reclaim the image from its anonymized typology. But this story is more complicated. On the one hand, the image can be thought of as the product of settler colonial structures—the structures that drew researchers’ attention to the Xavante as subjects, that formed the matrix for the scholarly methodologies and fields of those who travel to study in *Terra Indígena* (including me), and the structures of access to wealth and mobility that meant, for example, that I could access these images

⁶⁸² Here I draw on Kowal, Radin, and Reardon’s discussion of mutation as a way to make sense of the changing meanings and uses of scientific specimens over time. While their discussion is specifically focused on biological samples, their insights apply equally well to biometric photographs, which have different ontological meanings for Xavante kin and diverse community members. See: Kowal, Radin, and Reardon, “Indigenous Body Parts, Mutating Temporalities, and the Half-Lives of Postcolonial Technoscience.”

⁶⁸³ Pinney, “Introduction,” 4.

while community members could not. On the other hand, the collections were personal, not institutional or state colonial archives. They had travelled from Keiter to Neel, and from Neel to Flowers. In Flowers' possession the photographs were *both* scientific and personal. She knew or had known many of those pictured, and so as she used the images to correlate data, it was with the names *and* numbers that she had carefully inscribed on her own Polaroid census in 1976. When they arrived in her hands in the early 1990s the 1962 photographs still represented people with whom she had relationships; they had never truly been anonymized.

In her New York apartment a few months before my trip to Pimentel Barbosa village, Nancy Flowers transferred the digital images of Apowẽ and fifty-four others to my hard drive. She had scanned them as part of her effort, with the help of Coimbra and Ventura Santos, to send her personal papers to Rio where they would be more accessible to the communities of T.I. Pimentel Barbosa. For the 1964 genetics paper, the physical anthropologist Friedrich Keiter had used the photos to compare Hamburgian and Xavante physiognomy, illustrating a proposed methodology for semi-quantitative analysis of facial variation.⁶⁸⁴ For Flowers the images served to correlate data from the genetics paper with her doctoral research and her subsequent work with Salzano, Ventura Santos,

⁶⁸⁴ Neel et al., "Studies on the Xavante," 64-67. Keiter had been developing these approaches since the 1950s. See: Friedrich Keiter, "Über Zehenbeerenmuster und Kompliziertheitsindex," *Zeitschrift für Morphologie und Anthropologie* 42 (1950): 169-183; Friedrich Keiter, "Vaterschaftsdiagnostik mittels 'Trennlogarithmus'," *Aerztliche Forschung* 11 (1957): 537-551. Physiognomy, here, referred only to the measurement, quantification, and comparison of physical facial traits. The scientists were not correlating character with these traits. For physiognomy's longer history as a study of facial features as relating to character, see Sharrona Pearl, *About Faces: Physiognomy in Nineteenth-Century Britain* (Cambridge, MA: Harvard University Press, 2010).

and Coimbra. They helped provide a continuity of data, aligning a series of studies and permitting the depth of the diachronic study.

In their conversion to digital media, however, Flowers transformed the images. She repurposed them from a scientific tool into a kind of family album. Selecting the best portrait from each five-image anthropometric series, Flowers cropped and edited out the classificatory number hanging around each person's neck. "I think they will like them better this way," she told me. Her editing was an act of care, rendering beautiful individual pictures out of scientific documentation that was typological in aim. Flowers did not mention if she, like me, found the unedited images unsettling. I had difficulty seeing past histories of race making and the objectifying side of the scientific enterprise. Her edition also mutated the images, partially obscuring their origins as artifacts of population making. Like the other data and experiences of earlier research engagements, the photographs entered a process of repurposing for open-ended futures.

In the village on the day of the first public slide show, the men who attended looked intently at the projection, mostly silent as the edited images appeared. I showed one of the unedited photographs and tried to explain what the numbers were for, but was unsure how clearly my explanation came across in either Portuguese or once translated into Xavante. As the images passed, at certain moments they would chuckle or converse quietly. A photo of Sidówi, who had been only a young boy in 1962, raised a swell of laughter. But generally, a sense of nostalgia and melancholy pervaded. Most of these people had already passed on, and for some of those witnessing, it was the first time they were seeing images of a grandparent since their passing. The work of return, of

repatriation, implies a labor of memory, and as such the archive project implies yet another form of affective labor.

This project of collecting is a reassembling of an absent archive, an archive that elders in Pimentel Barbosa imagine but whose contents are still mostly unknown to them. It is the gathering of scattered materials that have contributed to the careers of dozens of scientists as well as my own as a historian. In their transnational dispersal, these publications, images, and audio recordings attest to the transit of the idea of the Xavante as a population uniquely capable of informing understandings of the past, or as a culture in danger of extinction. It also speaks to the role of these ideas in forming professional identities, academic disciplines, and advocacy NGOs. But as I began the collecting process, many scholars expressed immediate enthusiasm, offering help without hesitation for digitization and return. In this sense the reassembling also reflects ongoing shifts in ideas about ownership of materials that have been cogenerated, and the interest of scholars in maintaining or reinvigorating their ties to the communities that hosted them.

Asking for their return, the elders in Pimentel Barbosa foresee diverse uses for the materials, from personal practices of memorializing loved ones to their ongoing active production of cultural representations, which increasingly complement the economy of the village.⁶⁸⁵ Central to their concerns, however, are their experiences of engaging the Brazilian state. In discussions about the project they emphasized the increasing reliance on the written word, an acknowledgement of the power that textual sources have in

⁶⁸⁵ Laura R. Graham, "Image and Instrumentality in a Xavante Politics of Existential Recognition: The Public Outreach Work of Eténhiritipa Pimentel Barbosa," *American Ethnologist* 32, no. 4 (2005): 622–41, doi:10.1525/ae.2005.32.4.622.

Brazilian society and legal systems. They valorized the effort to return materials, not only for continuity of knowledge as new generations became increasingly fluent in Portuguese, but with a great deal of experience that enables them to know that certain kinds of documentation hold weight in the struggle to protect land or demand health services. Recognition, in all its cunning, must be mediated through forms that fit with *warazú* expectations of rationality.⁶⁸⁶

My collaborators in Pimentel Barbosa and Rio and I are still conceptualizing what this archive should be once reassembled. It will include researchers' published and unpublished material and also function as a living archive that community members can populate as they wish. Most important, however, is the intent to structure the archive according to Xavante epistemologies and protocols. Rather than rendering this body of knowledge increasingly accessible and legible for any to access, decolonial archiving offers possibilities for Xavante actors to creatively limit access to sensitive and sacred knowledge.⁶⁸⁷ In opening the archive to *warazú* with limitations, the digital interface may even be designed to require sustained engagement and self-reflexive practices for those interacting with the sources.⁶⁸⁸

Among the images that academics have shared with me, most document the Xavante with the researcher behind the lens. Some, however, are more personal. They document the making of the *xavantólogos*: an image of Nancy Flowers with her namesake Nancy, who was born during Flowers' first period in the field and was a

⁶⁸⁶ Povinelli, *The Cunning of Recognition*.

⁶⁸⁷ Margaret M. Bruchac, "Decolonization in Archaeological Theory," in *Encyclopedia of Global Archaeology*, ed. Claire Smith (New York: Springer Science and Business Media, 2014), 2069–77.

⁶⁸⁸ Elizabeth A. Povinelli, "The Woman on the Other Side of the Wall: Archiving the Otherwise in Postcolonial Digital Archives," *Differences* 22, no. 1 (2011): 146–171, doi:10.1215/10407391-1218274.

teenager by 1990 when the anthropologist returned; Ventura Santos, Coimbra, and Tsuptó with physician and master's student Rubens Ianelli around a table piled with data sheets; one researcher or another with a crowd of children. Much as researchers took relish in telling me the stories of their time in the field, they often spoke of their own *saudade* or nostalgia as images evoked a story or a memory.

For me, the collecting of photographs complemented my sense of the oral histories I had recorded. It enriched my understanding of the affective experiences these researchers carried with them. But my underlying sense is that the digital archive will do more than allow me to understand the researchers, or help villagers access written, audio, and visual sources. In inviting me into the archive project, the elders were also embarking on a project of relationship building. The project works to reestablish institutional connections with the sponsors at the *Museu do Índio*; it continues to build ties with Welch and Coimbra. It also compels me into the affective field of the village.

Part of the caring labor that I agreed to take up by joining the project is that necessary to address the afterlives of the scientific objects that will be incorporated. The image of Apowẽ in its many potentialities raises questions about how to work with materials whose meaning has mutated and will continue to do so. Will villagers in Pimentel Barbosa accept images and “look past” their objectifying past, or will the typological photograph be rejected as in some other Native communities?⁶⁸⁹ It seems already that in his reception Sidówi practiced what Hulleah Tsinhnahjinnie has called

⁶⁸⁹ Michael Arid, “Growing Up with Aborigines” in *Photography's Other Histories*, ed. Christopher Pinney and Nicolas Peterson (Durham: Duke University Press, 2003), 25.

“photographic sovereignty”⁶⁹⁰; firmly positioned in Xavante epistemologies, in his dream Sidówi spoke to the loved ones he had seen in the photographs. In conversation with them, he dreamed a Xavante name for me before appealing to me to “do important work.”⁶⁹¹ As my Xavante interlocutors continue to engage creatively with me, urging me, among other things, to care for this archive, they implicitly raise questions I have yet to answer about how to mediate between the two understandings of Apowẽ’s image. How will I engage with researchers and subjects to make sense of the history of this image? How will these discussions extend to other scientific materials in use and reuse? And what are the ethical and moral issues at play as they continue to be used as knowledge sources?

Absent and partially reassembled, the Xavante archive and the history of research in Xavante territory more generally provide insight into what Talal Asad called “the radically altered form and terrain of conflict inaugurated by [Western Imperialism]—new political languages, new powers, new social groups, new desires and fears, new subjectivities.”⁶⁹² This new terrain includes the extensive documentation of Xavante bodies and lives as part of a much broader social and natural-scientific project of legibility that is foundational to state and imperial power.⁶⁹³ In its heterogeneity, these regimes of knowledge production now include new political languages such as that of *antropologia militante* or Xavante demands to understand what purpose research will

⁶⁹⁰ Hulleah J. Tsinhnahjinnie, “When Is a Photograph Worth a Thousand Words?” in *Photography’s Other Histories*, ed. Christopher Pinney and Nicolas Peterson (Durham: Duke University Press, 2003), 41.

⁶⁹¹ Sidówi, interview, 15 July 2015.

⁶⁹² Asad, “Afterword,” 322-323.

⁶⁹³ Michel Foucault, *The Archaeology of Knowledge* (New York: Vintage, 1982 [1969]); Silverblatt, *Modern Inquisitions*.

serve, how it will circulate, and who might profit from it. New powers include everything from the sequencing of DNA to the uploading of images and films through file sharing, to the ease with which villagers can now contact their *warazú* researchers with mobile phones and Internet access. New desires and fears emerge as both subjects and researchers become increasingly cognizant of the power of documentation and expert knowledge. Finally, these new landscapes are also peopled with new subjectivities, new affective ties and senses of obligation.

APPENDIX

This appendix lists academic publications that have been produced since the earliest research published in the 1950s.

Abreu, Fabio de Almeida, and Josiane do Socorro Aguiar Souza. “Dinâmica Espaço-temporal de Focos de Calor em Duas Terras Indígenas do Estado de Mato Grosso: uma Abordagem Geoespacial sobre a Dinâmica do Uso do Fogo por Xavantes e Bororos.” *Floresta e Ambiente* 23, no. 1 (2016): 1–10. doi:10.1590/2179-8087.041813.

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Bianca Coelho Moura, Brasília, 30 May 2014
Carla Costa Teixeira, Brasília, 24 October 2013
Carlos E. A. Coimbra Jr., Rio de Janeiro, 19 March 2014
Cláudio dos Santos Romero, Brasília, 19 May 2014 and 20 May 2014
Fabrício Rodrigues dos Santos, Belo Horizonte, 6 March 2014
Fabrício Rodrigues dos Santos and Theodore G. Schurr, Porto Alegre, 5 November 2013
Fernando José da Rocha, Porto Alegre, 2 July 2014, 23 July 2014
Francisco Mauro Salzano, Porto Alegre, 11 July 2012, 13 July 2012, 17 July 2012, 19 July 2012, 23 July 2012, 26 July 2012, 3 July 2014, 17 August 2015
Girley V. Simões, Porto Alegre, 10 December 2013, 19 December 2013
James R. Welch, Rio de Janeiro, 27 March 2014, 21 April 2014
Luciene Guimarães de Souza, Rio de Janeiro, 25 April 2014
Mara Hutz, Porto Alegre, 7 July 2014
Maria Cátira Bortolini, Porto Alegre, 28 January 2014, 18 August 2015
Marilza Jota, Belo Horizonte, 7 March 2014

Nancy Flowers, New York City, 22 August 2013, 23 August 2013
Nilza Pereira, Rio de Janeiro, 24 April 2014
Paulo Sérgio Delgado, Cuiabá, MT, 4 August 2015
Pedro Paulo Vieira, Rio de Janeiro, 7 May 2014
Regina Aparecida Polo Müller, São Paulo, 17 September 2014
Ricardo Ventura Santos, Rio de Janeiro, 15 April 2014
Roque de Barros Laraia, Brasília, 18 July 2014
Rubens Ianelli, São Paulo, 5 September 2014
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Tsuptó Buprewên Wa'iri Xavante, Barbosa Sidówi Wai'azase Xavante, Luiz Hipru
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DVD

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