

ASSEMBLING THE ANCIENT: PUBLIC SCIENCE IN THE DECIPHERMENT OF MAYA  
HIEROGLYPHS

By

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To my parents, Dwight and Jamie, and my brother, Evan

## ACKNOWLEDGMENTS

According to the *Oxford English Dictionary*, the verb “to acknowledge” derives, in part, from a now obsolete Middle English use of the word “knowledge” as both a transitive and intransitive verb. In addition to the usage that fits more closely with today’s “knowledge,” “to recognize or admit as true,” *to knowledge* also entailed the meaning “to confess.” *Acknowledgement* still seems to retain something of this thirteenth and fourteenth century sense. I confess that the cover page is lying to you; I’m not alone here. Acknowledgements sections confess the plural authorship of our thoughts and words. And, they confess the inadequacy of academic conventions such as citation for giving due where due is due. They are confessions of the impurity of our knowledge production. In this sense, if we aspire to imagine more *democratic* knowledge practices, as I elaborate below, acknowledgements sections should be understood not as supplements revealing a work’s context of production, but as an integral part of the work itself, and as testimonies to the inadequacy of institutional scholarly practices for producing documents that sincerely and carefully display their own worldliness. With that in mind, I confess a significant debt to two organizations that generously funded research that made this study possible: the National Science Foundation (Award Number 0723636) and the Wenner-Gren Foundation for Anthropological Research.

Just as integral to the production of this study has been my dissertation advisor, Susan D. Gillespie. I have had the privilege of engaging in classrooms, offices, and hallways with Susan for seven years at the University of Florida. I have benefited in innumerable and immeasurable ways from this series of engagements. Perhaps of most direct relevance here, Susan encouraged me to undertake this study. She helped

me find my legs in the wild world of Maya studies. These legs are probably still a bit shaky, but through no fault of hers. Susan has been a compassionately dedicated mentor for me during every stage of this project, generously offering her time and energy for countless brainstorming sessions (or “confessions?”). Her contributions were not only requisite to the project’s conception, but also to the acquisition of funding and the writing stage. Susan is a relentless and relentlessly supportive critic. For years I have read her multicolored comments in the margins of my documents with a sense of disquieted appreciation. Susan recognized that this project was about encounters across difference, and that the advisor-advisee collaboration was an integral one. I thank Susan for her dedication and care.

I have had the further privilege of a committee comprised of truly committed scholars and mentors: Florence Babb, Malini Johar Schueller, and V. Betty Smocovitis. Florence and Malini have been integral interlocutors throughout all stages of this project. Florence was especially important in helping me refine my anthropological knowledge of Latin America. Malini let a wayward anthropologist into her seminar on Postcolonial Theory and American Studies in 2005 and has influenced my thinking ever since. Betty was a late arrival to the committee. I deeply appreciate her willingness to become a part of this project. I now realize that her knowledge and enthusiasm would have been a great asset all along. Florence, Malini, and Betty, I cannot thank you enough.

This study is dedicated to my family: Dwight, Jamie, and Evan Watson. In the final days of writing the draft of this document that I defended, I took the privilege of liberating myself from the confines of a desk in the far corner of Turlington basement

and enjoying how Florida's spring sun rendered the lights in my house unnecessary. At this time, April 2010, I came to relish the lively stir of three birds scuttling between trees' branches and the ground below. Taking my coffee on the porch while contemplating Maya animal spirit companions, one morning I came to the farfetched and ethically troubled conclusion that the fluttering cardinal, bluebird, and woodpecker might be surrogates for my family members.

The cardinal and bluebird, my father Dwight and mother Jamie, have influenced this project, as well as my professional and personal trajectory, in more ways that I can imagine. For some reason I don't quite know, I have always taken my eighth-grade science fair project as a model for how you think, Dad. I don't really remember whether it was my idea or yours, but I vividly remember us going together – for a final year – to the Wabash College Theater Department shop and transforming some scraps of wood, lightbulbs, wires, screws, gels, and clothespins into a “light piano.” Music, science, art, theater; it's all the same to you, Dad. You are the liberal arts. Though, you did leave me to come up with a hypothesis retroactively. Well, here it is.

Mom, you are a truly gifted thinker, teacher, and actor (in more ways than one). Do you remember when I read your story about working in that Los Angeles psychiatric geriatric facility? I said that it was “all description” and needed plot. Well, in the meantime, I've come to encounter a long and powerful tradition of description in anthropological writing. The author whose work I cite most pervasively in this document has said that description is truly hard, that we should stick with description, that worldly descriptions are “the highest and rarest achievement” (Latour 2005b:137). I'm not sure that I've lived up to that standard here. But if I've come close, it's because of you.

I have a question for Evan, the woodpecker always drumming away on the body of that guitar. Did you “leave the seafarer to rest under the Bodhi tree?” It wouldn’t be an awful place to leave him. I’ve thought about trees a fair bit in the past seven years. I looked up the Bodhi. It reminded me of the Ceiba. Bodhis and Ceibas anchor us into the soil with their roots. You know how important the tree is. But, I know that you know that the “vast, unpredictable, and ultimately unknowable” sea is where the action is. So, keep on sailing “in sync with the ocean’s rhythms coasting into a sunset, casting vibrant colors of grace on a cloudless sky.”

In addition to my committee and my family, I have benefited by a truly exceptional set of friendships crafted during my time as a graduate student. For ongoing dialogues and caring commitments, I owe specific personal debts to Bryan Tucker, Megan Teague, Erika Roberts, Ingrid Newquist, Julienne Obadia, Josh Toney, Rachel Harvey, Heather Freiman, and Lizzy Hare. I have also benefited through intellectual engagements with countless other graduate student colleagues, including Scott Catey, Randy Crones, Rebecca and Greg Gorman, Isaac Shearn, Jon Simon Suarez, and Ed Tennant. I thank Lucas Martindale Johnson for his generous last-minute assistance in drawing requisite figures. I also wish to thank my fellow denizens of the Mesoamerican Archaeology and Iconography Laboratory, Karen Pereira and Jeffrey Vadala. I owe trans-species gratitude to Buddy and to Phoenix, contributors whose absence from our world today does not render their ongoing contributions any less powerful.

Anthropological research is inevitably collaborative and a wide number of people have aided in the production and dissemination of my research. I owe a debt to Katya Gibel Mevorach for putting me on a path towards this project in ways that neither of us

could have anticipated. I wish to thank Allan Burns, Michael Heckenberger, and Stacey Langwick for being crucial contributors to facilitating my progress along that path. I owe thanks to Karen Jones, particularly for her support with administering grants. I also owe particular gratitude to all those who were willing to participate in interviews. These interviewees are listed in Appendix A. In addition to those listed as interviewees, I would like to thank the following people: Ed Barnhart, Nancy and David Orr, Carol Karasik, Susan Prins, Alonso Mendez, Moises Morales Marquez, Julia and Alfonso Morales, Dan Rogers, and Rick Warner.

I wish to thank all those who have aided with this project, including many, no doubt, who I have not named here. The support and encouragement of colleagues, friends, and family has been vital to the project's success. I confess that the study's faults are, of course, my own.

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Abstract of Dissertation Presented to the Graduate School  
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By

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This study examines how empirical, political, and ontological techniques innovated within science studies provide new ways to understand Maya hieroglyphic decipherment. Practitioners of science studies have attended to how natural sciences operate as social, cultural, and material sites that condition and control how we know and represent the cosmos, biological bodies, and environments. Science studies scholars have paid comparatively little attention to the cultural and political consequences of historical knowledge production. I begin to fill this lacuna through an ethnographic and historical analysis of how experts and non-experts have established arguments for the status of Maya hieroglyphs as a fully functional writing system, a process that began in the 1970s. I argue that a science studies approach reveals Maya hieroglyphic decipherment as set of processes that has depended integrally on historically-specific public collaborations and imaging practices that abstract hieroglyphs from their material contexts. The specific approach to science studies that I employ integrates the object-oriented metaphysics of Bruno Latour with research into public engagement with science and postcolonial critiques of scientific knowledge.

I show how decipherment achieved epistemic legitimacy through public workshops and imaging practices. Workshops at the University of Texas at Austin cultivated a collective of public witnesses through material practices that conveyed a sense that they were participating in a process of revealing historical truth. I compare the Austin setting with workshops conducted with Mayan-language-speaking linguists and activists in Antigua, Guatemala, which established a mutualism in which instructors and participants collaborated to consolidate the concept of a trans-historical Maya culture. I then trace how epigraphers have established their control over the concept of an underlying kernel of Maya culture by transforming hieroglyphs into objects that carry clearly intelligible ancient Maya voices from the past. Through the analysis of transforming interpretations of a single hieroglyph, I show how such historical narratives elide the local associations that enable hieroglyphs to acquire semantic values in the present. The study ultimately shows how hieroglyphic analysis requires a reduction of complex historical objects to contemporary concepts of writing and counterbalances this reduction with a care-oriented ethical consequentialism and an attachment-oriented epistemological realism.

CHAPTER 1  
INTRODUCTION: A SCIENCE STUDIES APPROACH TO MAYA HIEROGLYPHIC  
DECIPHERMENT

**Introduction**

This study examines how key conceptual and empirical innovations achieved within analyses of the natural sciences elucidate knowledge production within a *historical* science. The relevant objects of analysis are assemblages of public engagement and scholarly collaboration entailed in the decipherment of Maya hieroglyphs between the 1970s and the present. The analytical frameworks that I bring to bear on the decipherment of Maya hieroglyphs issue from research within the field of “science studies,” a wide-ranging set of critiques and analyses of science undertaken by philosophers, sociologists, historians, anthropologists, and – increasingly – some natural scientists (see Hackett et al. 2008). The study is generally situated within a literature that explores sciences as variegated sets of historically, technologically, materially, and culturally occasioned practices.

More specifically, I analyze the contexts and consequences of Maya hieroglyphic decipherment through frameworks developed for apprehending sciences as complex networks of heterogeneous actors (e.g. Haraway 1997, 2008; Latour 2004a, 2005b; Law and Hassard 1999; Law and Mol 2002; Ong and Collier 2005). In so doing, I draw centrally on the analytical program developed by sociologist, philosopher, and anthropologist Bruno Latour. In addition to extending Latour’s program for understanding what he has called “science in action” (Latour 1987), I draw on research that elucidates the imbrication of sciences in processes of colonialism and empire. I make the case that extending science studies methods to the analysis of Latin American historical knowledge production requires a careful attentiveness to the roles of

historical sciences in producing and reproducing structures of inequality emergent out of forms of colonial encounter and postcolonial governance. Thus I work to place what is sometimes termed “postcolonial science studies” into engagement and cross-fertilization with a particular historical science.<sup>1</sup>

This extension of postcolonial science studies to Maya hieroglyphic decipherment attends empirically to two distinct sets of practices that I position as necessary, indeed central, to hieroglyphic decipherment: public collaborations in workshop settings, and imaging practices that enable simulations of Maya hieroglyphs, artifacts, and landscapes to circulate among experts, enthusiasts, and the public. The topics of public engagement with science and scientific inscription practices have received significant coverage within the field of science studies. In examining hieroglyphic workshops that stage encounters between professional experts and lay experts or enthusiasts, I am providing further evidence for claims that the strength and legitimacy of many contemporary scientific practices rest on their forms of public engagement. Thus, I depict Maya epigraphy (in at least some of its formulations) as what Brian Wynne (2005) calls a “public science,” or a science whose practitioners engage constituencies ostensibly external to the scholarly establishment as an integral component of the research process. In turn, my attention to inscription practices illustrates how the legitimacy of claims to the decipherment of Maya hieroglyphs pivots on historically- and technologically-specific aesthetic practices for affirming the objectivity of interpretive claims (Daston and Galison 2007; Latour 1987, 1999a).

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<sup>1</sup> For overviews of the field of “postcolonial science studies,” see Warwick Anderson (2002a) and Anderson and Vincanne Adams (2008). Specific studies that fit within the rubric of postcolonial science studies include Arun Agrawal (2005), Timothy Choy (2005), Julie Cruikshank (2005), Sandra Harding (1998, 2008), Cori Hayden (2003), Stacey Langwick (2007, 2008), Timothy Mitchell (2002), Hugh Raffles (2002b), Peter Redfield (2000), David Turnbull (2000), and Hellen Verran (2002).

The overarching rationale for my study of these two sets of practices is to probe the value (and limits) of extending a Latourian postcolonial science studies to the analysis of historical knowledge production. In so doing, I argue centrally that this approach reveals a picture of both Maya hieroglyphic decipherment and, crucially, Maya hieroglyphs that substantially differs from accounts produced by scholars with vested professional interests in the reproduction of Maya hieroglyphic studies' disciplinary forms (e.g. Coe 1999[1992]; Montgomery 2002a; Stuart 2006b; Stuart 1992). Through this conceptual framework, I am able to show how publics and hieroglyphs themselves played necessary mediating roles in the transformation – and, I maintain, reduction – of complex Maya inscriptions into objects comprehensible as signs equivalent to contemporary forms of “writing.”

To some practitioners of Maya studies, it is apparent that analyses of hieroglyphs since the 1970s have tangibly altered scholarly and popular images of ancient Maya social structures, political governance, and belief systems (e.g. Coe 1999[1992]; Houston 2000; Houston et al. 2001; Stuart 1992). Thus, my intervention into understandings of decipherment and hieroglyphs further entails an exposition of how ancient Maya culture has become a categorically different object through historically specific material networks of collaboration and public engagement. In other words, it is my charge to evoke the particular social and material textures of historical science in action that buttress a way of knowing the ancient Maya.

Contemporary practitioners of Maya epigraphy have inherited and propagated a concept of “the Maya” as a coherent cultural entity. The cultural designation “Maya” covers speakers of historically related languages who have principally inhabited eastern

Mesoamerica, including areas now territorialized as southern Mexico, Belize, Guatemala, and parts of Honduras and El Salvador. Further, in popular publications scholars claim that a continuous Maya culture of has inhabited these territories from approximately 2000 BCE to the present (e.g. Freidel et al. 1993). The contestable notion of a unified “Maya culture” predates the rise of contemporary hieroglyphic studies (e.g. Stephens 1963[1843], 1969[1841]). Yet, epigraphers have helped solidify this singularity by treating inscriptions rendered on diverse media throughout the Maya area between the third century BCE and the sixteenth centuries CE as a single, legible (if not all equally decipherable) script. In recent years, a small, influential group of hieroglyph experts have abandoned their field’s antiquarian and art historical roots in favor of claims that epigraphy is grounded in a linguistic science that permits them privileged access to the literal meanings of Maya hieroglyphs (Houston 2000). In so doing, they have come to exercise significant – and, arguably, problematic – control over the definition of the Maya both inside and outside academia. While I insist on the contemporary specificity of these knowledge practices and the consequent narratives and images of Maya culture, my motivation is not solely to historicize or contextualize (let alone, to critique).

My argument addresses such problematic concepts through an anthropological analysis that exposes the inner workings of decipherment. Thus I position this study as a tool enabling both science studies practitioners and Maya hieroglyph experts to better understand what Maya hieroglyphs were in the past and have become in the present. Building on examples from science studies (e.g. Barad 2007; Haraway 1997; Latour 1999a), I propose that acts of scientific analysis entail performative, or partially

determining, effects on objects of analysis, such as hieroglyphs. By drawing out the many actors helping to make hieroglyphs into objects knowable within the present, I am also proposing a less reductive image of hieroglyphs. Part of this reframing entails experimenting with how we might understand hieroglyphs as nonhuman actors who play crucial roles in *mises-en-scène* partially but not wholly determined by forces outside their control.

Ultimately, I maintain that rewriting hieroglyphs as complex actors capable of plural forms of articulation may result in a more open and careful consideration of these objects as things that have had differentially significant functions as they have moved into new sets of material and semiotic associations. I further draw out the variegated consequences of hieroglyphic interpretation, which have ranged from cultural constitution to cultural empowerment to cultural violence, revealing manifold questions concerning the ethics of historical interpretation. This reframing and invocation of attendant ethical dilemmas within a postcolonialist spirit depends on conventions familiar to science studies practitioners and foreign to hieroglyph experts, many of whom may be inclined to accept Mayanist Stephen Houston's (2000:128) assertion that "certain readings [of hieroglyphs] are now set, regardless of personal whim and inclination, as reconstructed confidently by us and as intended explicitly by the Maya." By revealing the complex imbroglios through which these readings become attached to the hieroglyphs that they purportedly explain, I draw out how hieroglyphs are objects considerably more culturally significant, evocative, and dynamic than decipherment in its current form can reveal. Thus, while I do not wholly reject the utility of current methods of inquiry into hieroglyphs, I work to open these objects to more liberal forms of

analysis, significance, and action in the contemporary world. This is, in other words, an attempt to *democratize* Maya hieroglyphic studies in the sense advocated by Latour (2004a).

I begin to detail what this sense of “democracy” entails in the following section of this chapter, an attenuated overview of the conceptual framework, which I further develop in Chapter 2. After briefly summarizing how my use of science studies concepts and methods may better elucidate and democratize processes of historical analysis, the following section provides the reader with basic background on the rise of Maya epigraphy as a collaborative public science. I then outline my integration of this conceptual framework with the public science that I have chosen as an analytical object through an overview of my research design and methods.<sup>2</sup> In the final section, I summarize the organization of the study’s chapters.

### **Democratizing Epigraphic Science**

This study continues the trend of recent years to extend the purview of science studies. The scientific laboratory conventionally served as the central empirical locale of science studies research (e.g. Knorr Cetina 1981; Latour and Woolgar 1979; Traweek 1988). Understanding science anthropologically has entailed empirically studying the material and literary practices that define the laboratory as a place where people and machines meet to produce knowledge of the physical world. In turn, science studies scholars (particularly European sociologists) have paid significant attention to the public uptake or understanding of laboratory-produced knowledge (e.g. Bucchi and Neresini 2008; Epstein 1996; Irwin and Wynne 1996; Michael 1998; Wynne

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2005). Further, the “postcolonial turn” in science studies has introduced diverse new questions. Science studies scholars, and anthropologists in particular, have traced how scientists conduct research in the “field” and how scientific (including medical) ways of knowing enter into negotiation with local and indigenous ways of knowing (e.g. Cruikshank 2005; Hayden 2003, 2005; Langwick 2007, 2008; Lowe 2004; Turnbull 2000, 2007; Verran 2001, 2002). They have also examined how technoscience shapes alternative modernities, state ideologies, and nationalisms (e.g. Agrawal 2005; Anderson 2002b; Anderson and Adams 2008; Choy 2005; Harding 1998, 2008; Mitchell 2002; Redfield 2000). Yet, I maintain that practitioners of postcolonial science studies problematically continue to take some of the most general contours of science for granted. As I elaborate further in Chapter 2, anthropologists tend to study the reconfiguration of sciences and technologies crafted and defined as such in Europe and the United States as they move to former colonial territories and acquire new methods, meanings, and applications (or stay resolutely the same). Less work within the field of science studies has attended to how fields such as art history and anthropology, which are often considered “non-scientific” or “marginally scientific” in the United States and Europe, obtain the mounting social, cultural, and political authority of “science” in postcolonial contexts.

In demonstrating how sciences work locally, and how knowledge circulates effectively, science studies scholars have labored to demystify science and render its connections with other domains transparent (at least to other science studies scholars, if not always – unfortunately – to different publics or the scientists themselves).<sup>3</sup> As

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<sup>3</sup> While the situation has, perhaps, improved since the early 1990s, Shapin (1992a) presents an important commentary on the extra-disciplinary obligations of science studies.

Latour (2005b:2) puts it, today's science is "almost coextensive with the rest of social intercourse." In Latour's view, the longstanding distinction between "science" and "society" no longer holds, is undemocratic, and merely deludes us into thinking that we can study anything purely human (the "social" sciences) or purely nonhuman (the unmarked or "natural" sciences). Latour's alternative is to study how ontologically generic "associations" form and solidify among heterogeneous processes and materialities. He presents a very effective conceptual toolbox to study hybrid networks of associations, rather than clear-cut "objects" or "subjects."

I think that taking this position as a serious starting point requires that science studies scholars extend their analytical purchase to practices considered non-scientific or marginally scientific in popular discourse, as Latour has more recently explored in his studies of law (Latour 2010[2002]), social theory (Latour 2005b), and religion (Latour 2009). In the case of Latin America, knowledge practices gain coherency, scientific status, and societal influence through locally occasioned associations. These include associations with Europe and the United States, which reflects the continued link between imperialism and science in Latin America. While historians have carefully examined the colonial applications of science in Latin America (e.g. Aguirre 2004; Cañizares-Esguerra 2001, 2006; Schiebinger 2007; Tenorio Trillo 1999), sociologists and anthropologists have attended relatively little to the exercise of imperialism through scientific, including *social* scientific, research.<sup>4</sup> Thus, truly understanding the cultural significance and function of sciences in Latin American states entails a more thorough

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<sup>4</sup> Exceptions that have touched on such issues include the works of Arturo Escobar (1998, 2008), Cori Hayden (2003), and Peter Redfield (2000).

consideration of how hybrid associations that become rhetorically glossed as “science” achieve and mobilize this status to effect cultural and political consequences.

Protagonists of hieroglyphic decipherment have marginalized or not actively attended to such consequences in their personal narratives and analyses of the experience and logic of decipherment (e.g. Coe 1999[1992]; Houston 1989, 2000; Houston et al. 2001; Stuart 1992). Thus, Mayanist epigraphers have found their discipline subjected – and I think subjected fairly – to the criticisms of postcolonial intellectuals rejecting the field’s propagation of a violent image of the ancient Maya, without attending to how this image implicitly feeds on and sustains a colonial construction of indigenous worlds (e.g. Cohodas et al. 2002; Montejo 1993, 1999). Likewise, as early as the late 1980s, it was apparent to some Mayanists that the shifting popular concerns of the U.S. middle class had clear bearings on trends in interpreting the ancient Maya (e.g. Wilk 1988). Holding that “explanations have to appeal to common experience” (Wilk 1988:319), Mayanist ethnographer Richard Wilk (1988:317) went so far as to graph the relation between archaeological explanations of the rise and decline of the ancient Maya that invoked warfare, ecology, and religion relative to events of the Vietnam War and the emerging environmental movement during the late 1960s and 1970s.

By empirically examining specific, located practices of historical interpretation and pedagogy, this study adds empirical texture to such recognitions of the cultural positioning of ancient Maya studies. In the spirit of an ongoing reconciliation between scientists and critics of science,<sup>5</sup> I work to position this study as an initial attempt (in the

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<sup>5</sup> Since the exhaustion of the 1990s “science wars” (see Fujimora 1998; Gould 2000; Latour 1999d, 2002), some science studies scholars have increasingly positioned their work as a bridge between

spirit of postcolonial science studies) to ground subsequent conversations concerning how Maya epigraphy's cultural representations have sustained the inequalities perpetuated by colonial discourse and contemporary imperial forms, while simultaneously recognizing that Maya studies has an admirable history of activism and public engagement, as well as its own relatively autonomous, rather esoteric motivations.

Thus, here I work towards a new understanding of the processes and consequences of hieroglyphic decipherment through application of two methodological rubrics that are drawn from science studies and are commensurable with Latour's metaphysics. The first of these rubrics has become known as "Public Engagement with Science" (or PES). PES developed out of the "Public Understanding of Science" movement in Europe, which sought to evaluate general scientific literacy (Royal Society 1985). More recently, scholars such as Brian Wynne (2005) have shown how imagined and actual publics play integral roles in all stages of scientific research. As described above, the field of Maya hieroglyphic studies has continuously engaged several such publics through workshops. So, the first analytical component of the study focuses on how the ethic and practice of workshop collaboration has preconditioned and shaped hieroglyphic decipherment and hieroglyphs themselves. Further, my analysis of epigraphy as a public science promises to clarify how professional and non-professional participants in hieroglyphic decipherment and analysis negotiate the interests and

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humanistic research grounded in and oriented towards politics and scientific research grounded in the ongoing struggle to represent and intervene in material worlds. The viability of this unification remains a matter of concern to many science studies scholars, if not to many scientists. Both Latour (2004a, 2005) and feminist critic of science Donna Haraway (2008) make the important claim that it is precisely the stark opposition between scientific and non-scientific ways of knowing that science studies practitioners have destabilized. Thus, the project of demonstrating sciences' forms of cultural imbrication has left any straightforwardly anti-science standpoint, thankfully, *sans* ammunition.

ideologies that permeate the field's interpretations. In other words, Maya workshops serve as empirically delineated spaces of encounter and gathering where the intersection of Maya hieroglyphic interpretations with assemblages of interests, technologies, and cultural forms intensifies in practice and becomes more apparent for analysis. As I show, workshops become public laboratories.

The second rubric, which I am calling "inscription studies," is more of a methodological orientation than a defined field of inquiry like PES. Traditionally, science studies scholars have paid a great deal of attention to laboratories as places where humans and nonhumans assemble to produce representational inscriptions that circulate widely. As the first analytical part of the study examines these assemblages from a PES perspective, the second analytical part follows the circulation of inscriptions among the communities of Maya hieroglyphic experts and beyond. Collaboration has taken place not solely through face-to-face encounters in workshops, but also through the production and circulation of drawings, photographs, letters, newsletters, workbooks, and weblogs. I show how specific media have shaped the content of Maya hieroglyphic decipherments as well as the sense of interpretive consistency and certainty that has led Maya hieroglyphic scholars to argue that they grasp the literal meanings of Classic Maya inscriptions, as in Houston's statement quoted in the introduction.<sup>6</sup> The assumption that interpretations reproduce Maya hieroglyphs' literal meanings is now pervasive in Maya epigraphy, and rests on epistemological suppositions that require ethnographic explication to be identified. Further, such images

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<sup>6</sup> For an overview of science studies approaches to imaging, see Regula Valérie Burri and Joseph Dumit (2008). Lorraine Daston and Peter Galison (2007) also historicize concepts and practices of scientific objectivity through attention to the role of imaging practices. Finally, there is a burgeoning literature on the role of imaging practices in archaeological knowledge production, with manifest relevance for analyzing epigraphic knowledge production (e.g. Moser 2001; Smiles and Moser 2005)

and documents provide intersecting points of entry into “systems of imagination” (Herbert 2001:xiii) that compel research and reveal the collusion of the Maya experts’ cultural and historical questions concerning how life was structured in the past with the ethical question of how social life should be structured in the present. Such systems of imagination reveal themselves through images, publications, and informal communications through which scholars explicitly cogitate on the past. The imaginative, speculative quality of historical inquiry proves ripe for such analysis.

The study further suggests that the utility of the anthropology of science in postcolonial contexts (which have not been governed as strictly by European rationalism) depends on scholars’ willingness to pursue forms of knowledge that have central political import in these states, regardless of whether they are self-identifying sciences rooted in U.S.-European contexts. I think that this extension of the analytical terrain of science studies fits within the spirit of Latour’s (2004a) effort to “bring the sciences into democracy.” Such an effort involves abandoning the notion of a stark epistemological fissure between natural science, on the one hand, and politics and social science, on the other. Any claim that a few humans can transparently represent massive social or natural “constituencies” or complexes of objects elides the intricate and sometimes precarious sets of linkages among disparate mutually-sustaining actors that amount to what we call “knowledge.” Democratic scientific and political representation, as yet unrealized, will entail commitments to acknowledging, as best we can, the vast complex of associations that comprise our worlds. In line with Latour’s (2005b) empirical commitments described further in the section on Research Design and Methods below, the mutual democratization of science and politics requires careful

attention to the specific material associations that actors constantly enact, associations irreducible to neat categories of “science” and “society.”

### **Decipherment, Collaboration, and Publics**

This section offers readers a glimpse into the rise of Maya epigraphy in its contemporary form, as a preface for the analyses presented Chapters 4-8. I focus in particular on the emergence of a significant collaborative ethic that motivated Mayanist experts to open their field to public engagement. I review some of the more significant collaborative events that enabled the articulation of epigraphers and Maya hieroglyphs resulting in the latter’s transformation into a readable script. Like subsequent chapters of the study, this section focuses on collaborations among scholars as well as collaborations between scholars and lay epigraphers or enthusiasts. Subsequent chapters more clearly reconsider these collaborations through the analytical tools of postcolonial science studies.

Scholars have worked to interpret Maya hieroglyphic signs since at least the early nineteenth century (or the sixteenth century, if we include friar Diego de Landa’s [1994(c. 1566)] colonial research in Yucatan in the genealogy). One compilation of contributions to decipherment even makes the bold claim that inquiry into the Maya hieroglyphs has proceeded with relatively few gaps since the colonial period (Houston et al. 2001). Despite such longstanding interest, these efforts did not yield substantial results for the linguistic portions of the inscriptions until the 1950s, and more certain interpretations, including a well-formed argument for the existence of verb signs (Schele 1982a), did not emerge until the 1970s and 1980s. While, as I show throughout this study, these shifts in decipherment and the images of the Maya that they have produced are strongly shaped by contemporary cultural, scientific, and technological

systems, many Mayanists vested in decipherment consider such results profound innovations independent of their contemporary contexts (e.g. Coe 1999[1992]; Houston 2000; Schele and Freidel 1990; Schele and Mathews 1998; Schele and Miller 1986; Stuart 1992).

These innovations began with and continue to be premised on the individual efforts of Yuri Knorosov (1952, 1958) and Tatiana Proskouriakoff (1960) to demonstrate the respective presences of phonetic signs and historical content in the Maya inscriptions. Their efforts thus created the foundation for thoroughgoing inquiry into the entirety of the writing system. When it took off in earnest in the 1970s, this inquiry adopted collaborative social and epistemological conventions that were largely foreign to earlier Maya hieroglyphic studies. While Mayanist archaeological research of the twentieth century depended on institutional support and research teams capable of significant amounts of physical labor, epigraphic and iconographic pursuits tended to lend themselves to more solitary inquiry (as in the cases of Knorosov and Proskouriakoff). Indeed, until the 1970s, the field of Maya hieroglyphic interpretation was dominated by a single figure, archaeologist J. Eric S. Thompson, though Knorosov and a few other researchers also made significant contributions (e.g. Berlin 1958; Kelley 1962b, c). Unlike Knorosov (and David H. Kelley), Thompson was a strong opponent of linguistic decipherment and, while his system for cataloguing the hieroglyphs (Thompson 1962) remains a necessary resource for hieroglyphic interpretation, he is cited in a popular account of decipherment as a source of stagnation in decipherment (e.g. Coe 1999[1992]).

Whether or not decipherment stagnated under Thompson's reign, this interpretive terrain clearly changed in the 1970s. In 1973, Mayanist art historian and iconographer Merle Greene Robertson initiated what would become a series of gatherings for interested parties to hash out interpretations of Maya art, iconography, and archaeology in the town of Palenque, Chiapas, Mexico, near the major Maya archaeological site of the same name. These meetings, the Palenque *Mesas Redondas* (or Round Tables) sparked novel collaborations among Mayanists (or aspiring Mayanists) and staged one of the first recurring social interfaces between Mayanists and members of the non-academic public. They set in motion a transformative collaboration among Robertson, art instructor Linda Schele, epigrapher and archaeologist Peter Mathews, and linguistic anthropologist Floyd Lounsbury (Coe 1999[1992]; Robertson 1996). With interests and fields of expertise that differed markedly, these scholars combined each other's complementary knowledges to answer established questions surrounding the names and biographies of ancient Maya kings referenced in hieroglyphic texts (particularly those at Palenque). Indeed, reflecting on this transformation, Schele and Mathews (1998) have described the impact of the *Mesas Redondas* on Maya hieroglyphic studies as a "revolution." Twenty-five years after this initial gathering, these authors wrote, "How different it is to walk through a ruined city when it has become a historical place – to 'read' a building and to know who looks out from a sculptured portrait. The ruins cease to be anonymous places admired only for their beauty and mystery" (Schele and Mathews 1998:13). Thus, part of the transformation entailed in hieroglyphic decipherment has involved the repositioning of the ancient Maya as a "historical" culture, or a culture that produced written records of historical individuals.

An equally diverse nonacademic public, ranging from local tour guides and amateur archaeologists to enthusiasts who traveled to the meetings from the United States, witnessed this breakthrough. Thus, the *Mesas Redondas* (and the subsequent publications of workshop presentations) helped to create a public audience and a scholarly ethic of public engagement. This ethic of engagement was sparked initially through Robertson's connections with the local Palenque community and the curiosity of community members living in the shadow of a significant Maya site. Schele should be recognized as one of the most significant actors in making this public commitment more sustainable and widespread. Transitioning to Maya studies from a career as an art instructor and surrealist painter, Schele effectively parlayed her concerns with visual art as a medium for public engagement and social commentary into a commitment to public engagement through hieroglyph workshops.<sup>7</sup>

Schele left her post as an art instructor at the University of South Alabama to become an instructor and doctoral student in the Institute of Latin American Studies at the University of Texas at Austin. Beginning in 1977, she instituted pedagogical workshops on Maya hieroglyphs. Schele received her Ph.D. in Latin American studies in 1980 and proceeded to gain employment at UT-Austin as a professor in the Department of Art History. By the early 1980s, the annual "Workshops on Maya Hieroglyphic Writing" (later, the "Maya Meetings") became a central laboratory for public engagement and developing new hieroglyphic interpretations. Though they have been altered significantly, the Maya Meetings continue today and have served as the model for "spin-off" workshops throughout the United States. The interpretations crafted at the Maya Meetings were indexed annually in the form of workbooks, pedagogical texts used

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<sup>7</sup> Schele, D. Personal Interview. 12 July 2007.

in the meetings and stocked and made available, along with a range of other unpublished hieroglyphic interpretations, for reproduction at the local Austin “Kinko’s,” a photocopy shop. While pedagogy developed out of increasingly solidified interpretations of the grammar of hieroglyphic texts, methods for constructing a collective with a common understanding of the Maya past relied on newly affordable slide and overhead projectors as well as photocopier technologies. Though such technologies may today seem unremarkable or obsolete, they had profound consequences for the management of historical information. Scholars such as Linda Schele gathered aspiring Mayanists around images produced by projectors. They taught enthusiasts how to cut out and reassemble intelligible hieroglyphic texts from photocopies, coding hieroglyphs with colors corresponding to their grammatical roles. This process has enabled publics to develop hieroglyphic literacy without seeing the original script in its archaeological context. I attend in detail to the social-material practices of hieroglyphic decipherment and instruction that have characterized the Maya Meetings in Chapter 5.

A decade after initiating the Maya Meetings, Schele and epigrapher Nikolai Grube began to conduct a less successful, but – from a postcolonial science studies perspective – equally provocative, workshop session in Antigua, Guatemala. Schele and Grube’s Antigua workshops introduced Maya activists and community leaders to the structure and composition of Maya hieroglyphic texts (Schele and Grube 1996). This scholar-public interface took place within institutional settings established by North American linguists collaborating with Guatemalans aspiring to achieve skills in linguistic analysis for efforts towards community activism and the cultivation of a nationalist-ethnic

identity consolidating speakers of the many distinct Mayan languages. These workshops provide the material for Chapter 6. In the following section, I describe the empirical work that I have undertaken in the effort of analyzing the public science described here. I oriented such empirical work through the aim of further democratizing Mayanist historical knowledge production.

### **Research Design and Methods**

The fieldwork necessary to compose this study took place through visits to three sites where important workshops on Maya culture (and hieroglyphs in particular) have taken place: Austin, Texas; Antigua, Guatemala; and Palenque, Mexico. The guiding objective of this research was to reproduce documents and conduct interviews that evidence the forms of collaboration requisite to hieroglyphic decipherment. My research in Austin had three components: interviews, document work, and participation in the 2008 Maya Meetings. I conducted interviews with nine associates of the Maya Meetings, including all three individuals responsible for the chief administration of the Meetings since their inception in 1977. Further, David Schele generously provided me with access and permission to photograph the privately held personal documents of the late Linda Schele. Research in Antigua involved creating connections with Oxlajuuj Keej Maya' Atz'iib' (OKMA),<sup>8</sup> an institute for linguistics activism and community development that has sponsored Antigua epigraphy workshops. In Antigua, I interviewed an OKMA linguist and employee, a workshop instructor, and two workshop participants. I acquired a copy of a pedagogical workbook used in the context of the OKMA epigraphy workshops, to complement the complete set of Texas Maya Meetings

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<sup>8</sup> The name "Oxlajuuj Keej Maya' Atz'iib'" means "13 Deer Mayan Writers," a reference to the Maya ritual calendar date (13 Deer) on which the organization was formed (Fischer and Brown 1996:vii).

workbooks. The principal research conducted in Palenque involved photographing documents produced during the first Palenque *Mesa Redonda*, in the possession of Mexican archaeologist Alfonso Morales.

Through this research, I produced extensive images of relevant documents and notes based on interviews and participatory experience. I digitally photographed approximately 2300 images of letters from Linda Schele's personal archive, in addition to the original decipherments proposed at the first Palenque *Mesa Redonda*. While I produced an audio recording of only one interview, I took extensive notes during all interviews, and subsequently developed these into ethnographic fieldnotes. Finally, I participated in the 2008 Maya Meetings, taking select photographs and producing fieldnotes based on my experiences.

My analysis of these materials is clearly guided by the empirical methods of Bruno Latour's (2005b) sociology of associations as well as cultural anthropologists' cognate approaches to analyzing (and, thus, participating in) open scientific systems (see Fischer 2007a; Fortun 2003). These methodologies help the researcher "follow the actors themselves" (Latour 2005b:12), or trace how associations among different participants in the scientific process form and solidify. As should be apparent from my introduction of the study, this project traces how shifting interpretations of the ancient Maya, Maya hieroglyphs, and the value of epigraphy formed through workshop collaborations as well as literary and imaging practices. Such interpretations include the emphasis of blood-letting rituals popularized by Schele and Mary Miller's (1986) popular exhibition catalogue *The Blood of Kings: Dynasty and Ritual in Maya Art* (based on an exhibit at the Kimbell Art Museum in Fort Worth, Texas) and Schele and David Freidel's

(1990) *A Forest of Kings: The Untold Story of the Ancient Maya*, and the notion of an essential Maya culture defended in Freidel, Schele, and Joy Parker's (1993) *Maya Cosmos: Three Thousand Years on the Shaman's Path*, as well as more esoteric hieroglyphic interpretations.

The documents and interviews that I have conducted offer much more detailed accounts of this process of historical discourse formation than is currently available in published reflections (e.g. Coe 1999[1992]; Houston 1989, 2000; Houston et al. 2001; Stuart 1992). Thus, the guiding method of this research is to trace connections among the varied unpublished accounts that I have collected and coproduced, as well as published research and reflection in order to describe the processes of transforming historical interpretations and Maya hieroglyphs themselves, processes that often correspond to (as well as constantly alter) the scholars' and publics' contemporary forms of life.

Especially since "collaborative" public engagement is a recurrent conceptual, ethical, and practical theme among participants in hieroglyphic interpretation, this ethnographic imperative of "following the actors themselves" is not an overly complicated process. I have begun this task by visiting OKMA and attending the Maya Meetings, becoming a part of the public drawn to hieroglyphic interpretation, and I have acquired documents that evidence the movement of these actors (primarily hieroglyphic objects and interpretations, rather than epigraphers or members of the public themselves). In my analysis of these materials, I turn to identifying the particular events, or structures of encounter, documented in the materials that I have collected that identify collaboration as a factor enabling new interpretations of the ancient Maya.

While I do not exclusively discuss them, I prioritize the collaborations that took place in workshops and through a gray literature of informally circulated documents. These collaborations are the foundation of the contemporary Mayanist epistemology of decipherment, ethics of representation, and ontology of hieroglyphs.

### **Outline of the Study**

Between the introduction and conclusion chapters, this study is comprised of three parts: concept-work devoted to extending the purview of science studies into Latin American historical knowledge (Chapters 2-4); analyses of workshop collaborations through the lens of PES (Chapters 5-6); and analyses of the inscriptions and imaging conventions that have mediated the popular image of the ancient Maya (Chapters 7-8). This section reviews the contents and coherence of these parts.

The first part of the study contains three chapters, beginning with Chapter 2, “From Actor-Network Theory to Postcolonial Science Studies.” This chapter outlines the basic conceptual frameworks that this study subsequently applies to the decipherment of Maya hieroglyphs. Here I introduce Latour’s framework, sometimes called “actor-network theory,” or ANT, as my rudimentary conceptual apparatus. I build on the work of metaphysical philosopher Graham Harman (2009) to explain four key concepts that ground Latour’s metaphysics: actor, irreduction, translation, and alliance. With these concepts in mind, I proceed to examine relevant literatures within science studies that enable Latour’s metaphysics to gain empirical traction within specific cultural and institutional assemblages: public engagement with science, and postcolonial science studies. I present the case that a perspective employing Latourian postcolonial science studies to examine the forms of encounter predicated in public workshops and instantiated in their interpretive inscriptions requires a slightly more elaborate

conceptual apparatus than that outlined in Latour's and Harman's accounts. Particularly, I combat the documented diffusionist tendencies of ANT (e.g. Strathern 1996) through a concept of "cuts" that foregrounds intersections of ethical, ontological, and epistemological problems in a mode congruent with the spirit of postcolonial and empire studies. I further make the case that a truly *postcolonial* science studies necessitates a more thoroughgoing engagement with historical narratives that figure into prominent colonial and anticolonial discourses.

Chapter 3, "The Intersections of Science, Politics, and History in Latin America," explores the existing English- and Spanish-language science studies literature on Latin America. Like the early twentieth century social scientific disciplines analyzed by historian Mauricio Tenorio Trillo (1999), Maya hieroglyphic studies emerged through collaborations between United States and Latin American scholars, which constructed Latin America – and especially Latin American indigenous populations – as a vast laboratory. With this in mind, I review how scientists (including social scientists) in Latin American nation-states have tended not to distinguish science and politics as starkly as is common in Europe and North America. In addition to Latin American scholars working to translate social scientific knowledge into political policy, many Latin Americanist ethnographers from the United States have recognized the intrinsically political qualities of their research. Thus, here I show that Latin American social science is science in an unqualified sense, and that this science is intrinsically political. I further introduce a conceptualization of "witnessing" (following Haraway 1997; Shapin and Schaffer 1985) that becomes important for understanding the significance of open, public workshops for stabilizing claims to the validity of hieroglyphic interpretations.

These observations provide further evidence for my assertion that postcolonial science studies should attend to historical and social scientific discourses that have played major roles in the constitution (and racialization) of populations such as the Maya. The close connections between Latin American science and politics aid my efforts both to open postcolonial science studies to historical knowledge and to argue for the entanglement of Maya studies' constitutive systems of imagination within forms of contemporary imperial politics. As I proceed to show in subsequent chapters, these systems of imagination form through local articulations of science and politics.

Chapter 4, "Phoneticism and the Rise of Hieroglyphic Decipherment," moves this study into the specific conceptual terrain of Maya hieroglyphic decipherment. The chapter serves as both background for the subsequent empirically grounded chapters and as an assessment of the value of a Latourian metaphysics for apprehending historical knowledge production. Here I begin with a basic summary of the structure of Maya hieroglyphic writing as epigraphers understand it today. I make a case that this understanding of hieroglyphs has reduced them to writing through two stages of extrication them from their material contexts: the early twentieth-century imperial research of scholars such as Sylvanus Griswold Morley (1920, 1938, 1946) and J. Eric S. Thompson (1959, 1962, 1971[1950]); and Yuri Knorosov's (1952, 1958) currently successful case for hieroglyphs as writing with phonetic components, to which I devote the bulk of the chapter's analysis.

My analysis of the rise of phoneticism draws on Latour's basic metaphysical framework (Harman 2009; Latour 1988) refracted through his more explicit work on the democratization of science (Latour 2004a), questioning how we might reframe Yuri

Knorosov's case for the pervasive presence of phonetic signs as an ethically- and epistemologically-troubled reduction of complex objects into a single known category, "writing." A sizable corpus of science studies research focuses on the emergence and resolution of controversies reflecting the ontological and semiotic uncertainty of scientific objects.<sup>9</sup> Here, I attend to the consequences that issue from practices of transliteration and translation that abound in progressivist rhetoric of discovery and position historical artifacts attributed to a colonized culture as writing, a narrow ontological category of Euroamerican provenience that carries multiple, sometimes contradictory moral valences. This chapter thus consolidates the general ethical and epistemological bases for the interventions that I enact in the following four chapters, which are grounded in my ethnographic research.

The second part of the study contains the first two chapters fully based in my interview and document research: "The Maya Meetings as Public Science," and "Articulating a Maya Singularity in Guatemalan Epigraphy Workshops." These chapters present two workshop series involving different sets of participants. Building on Latour's metaphysics as well as frameworks from PES, each chapter shows how the workshop instructors and participants imagined and facilitated each other's roles in Maya hieroglyph interpretation. Reading Maya Meetings workbooks against transcripts and letters exchanged among participants as well as my interviews and published accounts, Chapter 5, "The Maya Meetings as Public Science," explores how a small group of innovative scholars in Maya hieroglyphic studies imagined a public as integral to their production of historical knowledge, and created conditions for continuously engaging this public. I pay particular attention to the visual and material practices of witnessing

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<sup>9</sup> See Latour (2005b) for a discussion of this research tradition.

and replicating innovative acts of Maya hieroglyphic decipherment, as a constitutive performance of a contemporary community and a historical imaginary. Thus, this work illustrates how historical practices coproduce understandings of the present and the past, transforming hieroglyphs in the process.

Chapter 6, “Articulating a Maya Singularity in Guatemalan Epigraphy Workshops,” builds another example into this coproduction of present and past and reconstitution of the hieroglyphic object, arguing that the OKMA workshop-based alliance between United States and European epigraphy scholars, on the one hand, and Maya linguists and activists, on the other, developed into a mutualism providing beneficial political resources to the Maya activist-linguists and beneficial epistemological resources to the epigraphers. The mutualism hinges on a collectively crafted narrative of essential Maya identity. This analysis draws from my interviews with participants, as well as a workbook and published sources. Through contrast with the previous chapter, this piece suggests how scholars and lay participants stylize Maya epigraphy’s historical narratives to make sense for different participants engaged through different modes of collaboration. This attunement further suggests how we might rethink the concept of “public science” itself in light of divergent experiences of engagement (specifically, the kind of publics constructed through historical sciences).

At this point in the study, I transition to two chapters on the imaging conventions and media of Maya hieroglyphic studies: “Inscribing and Circulating the Ancient Maya;” and “Translations and Transformations of the Half-Spotted *Ahau*.” In order to understand the persuasiveness of the interest-laden historical discourses coproduced by scholars and publics in workshops, Chapter 7, “Inscribing and Circulating the Ancient

Maya,” explicates the specific imaging and literary practices that have defined the community of hieroglyph scholars and their forms of public engagement. Maya hieroglyph experts generally study images of hieroglyphs instead of actual hieroglyphs. Thus, understanding Maya epigraphy from an anthropological perspective entails tracing the history of the technologies and aesthetic conventions that shaped the artistic rendering of Maya hieroglyphs. This chapter examines how a series of highly individualized, yet interconnected, innovations have facilitated decipherment and altered the contemporary imaginary of the ancient Maya: J. Eric S. Thompson’s catalog of Maya hieroglyphs, Merle Greene Robertson’s rubbings of bas-relief imagery and hieroglyphs, and Linda Schele’s drawings of hieroglyphs. I argue that each of these inscription practices shaped how audiences imagined the ancient Maya and had performative effects on hieroglyphs-as-actors. Further, the vividness of this developing imaginary served as an epistemological underpinning for scholars’ claims to read the literal words of the ancient Maya. Hieroglyphs become abstracted from the original material contexts and reduced to images that spurn complexity and travel smoothly across cultural and epistemological differences, fortifying scholars’ claims to objectivity.

The persuasive power of these imaging conventions and technologies directly mediates and shapes the specific semantic values ascribed to Maya hieroglyphs. Thus, in Chapter 8, “Translations and Transformations of the Half-Spotted *Ahau*,” I complete a Latourian analysis of the transformations of one specific Maya hieroglyph as it circulated through distinct assemblages of material, cultural, and aesthetic practices. This chapter exemplifies the value of reconsidering the process of decipherment as a complex site where multiple differentially-interested human and nonhuman actors have come into

relations of association, shaping how specific hieroglyphs have come to be articulated or allied with their meanings. I trace how J. Eric S. Thompson mediated the consolidation of dispersed signs as a single hieroglyph, “half-spotted *ahau*” (half-spotted lord), and then how this sign became articulated with distinct epigraphers, who proceeded to mediate its redefinition as “*balan-ahau*” (hidden-lord) and then “*way*” (a term meaning a spiritual co-essence, or animal spirit companion). In the process, I examine how this particular sign became a concentrated site through which the ethical and epistemological stakes of the field were articulated.

The conclusion of the study proceeds to reflect on the broader ethical consequences of public representations of ancient Maya violence that emerged in the 1980s. In concluding, I advocate an ethics of care and *attachment* in the production of historical narratives,<sup>10</sup> demonstrating how decipherment co-emerged with the retooling of Maya political-cultural representation. Here, I outline a method for invigorating an attachment-based public science of Maya hieroglyphic studies that incorporates attention to the discipline’s contemporary political positioning and consequences as a necessary, but insufficient, research practice. Hopefully, my reading will also help invigorate hieroglyphs as complex, worldly actors.

Importantly, the space of Latin American knowledge production and the case of Maya hieroglyphic studies also alter some of the basic presuppositions of PES and inscription/association studies. In summarizing the study’s arguments, the final chapter further comments on how the increasing emphasis within science studies on the power

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<sup>10</sup> I draw my understanding of the ethics of care and attachment from Haraway (esp. 2008), explained further in Chapter 2. Here, I am also building on theorizations of the politics of science grounded in such ethics (esp. Latour 2004a; Stengers 2005) and cultural and literary studies formulations of the “ethical turn” (Davis and Womack 2001) and the “ethics of respect” (Childs 2003). I recognize that the ethics of care has a longer genealogy in feminist philosophy (see Held 2006).

of sciences to define and govern populations through “biopolitical” techniques (following Agamben 1998; Foucault 1991) should be complemented with systematic analysis of how historical discourses shape widely held concepts of selves and collectivities (such as Maya culture). This is especially significant in nation-states where the ethnic distinctions materialized out of colonial histories shape identity more pervasively than medical and technological interventions. The relative power of historical and scientific discourses depends, primarily, on their use for political ends (regardless of whether we consider the political to be pervasive). Thus, I conclude by arguing that postcolonial science studies would enhance its scholarly and popular relevance by focusing primarily on the knowledge practices that most directly shape local cultural politics, rather than limiting its purview to a sense of “science” exported from Europe and the United States.

## CHAPTER 2 FROM ACTOR-NETWORK THEORY TO POSTCOLONIAL SCIENCE STUDIES

### **Introduction**

In this chapter, I outline the specific theoretical frameworks drawn from science studies that ground my analysis of Maya hieroglyphic decipherment, in order to elaborate their utility for understanding historical knowledge practices. I review three interrelated frameworks: actor-network theory; public engagement with science; and postcolonial science studies. The initial section treats actor-network theory because it provides some of the most basic metaphysical and methodological positions that I employ in this study. I proceed to summarize the relevance of the public engagement with science movement and postcolonial science studies, concentrating on approaches that further elaborate the methods and concepts of actor-network theorists, particularly Bruno Latour.

I build from a basic presentation of the key concepts of Latour's metaphysical program to a more specific framework, which accounts for how publics have participated in the generation of knowledge and draws from the literatures of postcolonial theory and subaltern historiography to locate the use of historical knowledge in propagating contemporary expressions of colonialism and imperialism. The style of "postcolonial science studies" that I advocate diverges from established approaches within this rubric by taking sociohistorical knowledge as its object of analysis, as opposed to the knowledge produced by the "hard sciences" more conventionally analyzed by science studies scholars, such as physics, genomics, and health sciences. I ultimately argue that postcolonial science studies should attend

prominently how the scientific production of historical knowledge entails “cuts” (Barad 2007; Strathern 1996), a necessary conceptual supplement to the Latourian stance.

Thus, my principal conceptual intervention here concerns the necessity of expanding Latour’s metaphysics and methodology to historical knowledge practices that effect racial and ethnic categories. I hold that accounting for the racialization of these collectives requires attending to how scientific knowledge practices enact boundaries, *cutting* away certain actors as irrelevant, and *marking* others as belonging to a certain racial-ethnic collective. My discussion of this negative (and often violent) process builds on postcolonial critiques of representation discussed further in the next chapter.

### **Actor-Network Theory**

Originally developed through sociological ethnographies of scientific laboratories and field practices (Callon 1980, 1986; Latour 1987; Latour and Woolgar 1986[1979]), actor-network theory (ANT) is now a widely used theoretical framework. This section provides the reader with a basic framework for understanding how ANT emerged as a significant set of conceptual tools for analyzing scientific practices. The first subsection briefly traces the intellectual context in which ANT emerged. I review how advocates of ANT came to distinguish their approach from competing analytical frameworks in the sociology of science. Having established this intellectual genealogy, I move to a second subsection, outlining four concepts distinctive to Latour’s approach. These concepts provide the rudimentary framework for this study’s conceptual and empirical reconsideration of Maya hieroglyphs and Maya hieroglyphic decipherment.

#### **The Rise of Actor-Network Theory**

ANT emerged in the mid-1980s as one of several programs whose protagonists sought to provide alternatives to the established sociology of science associated with

Robert Merton (e.g. 1976[1963]). Mertonian sociologists aspired to understand the abstract system of ideals and institutions that governed science, and tended to consider science as a process independent of culture. Such an approach had begun to falter as early as the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), which refuted the assumption that scientific knowledge gradually progresses and accumulates.<sup>11</sup> By the 1970s, alternatives to the Mertonian approach began to solidify into coherent, named research programs. The most prominent early program was the sociology of scientific knowledge (SSK) associated with philosopher David Bloor. In a 1983 review article on the sociology of science, Harry Collins clarifies that Bloor's (1973, 1976) program, sometimes termed the "Strong Programme at Edinburgh," was not a "reaction" against Mertonian sociology, but, rather, an extension of the philosophical positions of Imre Lakatos (e.g. 1970) and Ludwig Wittgenstein (e.g. 1967).

The sociologists of the SSK movement explicitly advocated a form of relativism in which all beliefs about the world should be evaluated through the same methodological procedures. In other words, their position was that sociologists should adopt an "equivalence postulate," or method of treating competing beliefs about the world as equally true or equally false (e.g. Barnes and Bloor 1982). Sociologists of science should study the explanations of the natural world that scientists consider valid and

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<sup>11</sup> While Kuhn's intervention was distinctive, it was not the first challenge to the pervasive ideology of modern scientific progress (and its sociological affirmation). Latour, for example, regularly cites Ludwik Fleck's (1981[1935]) earlier sociological ethnography of biomedicine as a founding moment for science studies. Further, Kuhn's work was probably not an intentional contribution to the nascent postpositivist sociology of science: "I have a vivid memory of Thomas Kuhn accepting the Bernal Prize [for distinguished contribution to the field] from our society [Society for Social Studies of Science] with more than a slight embarrassment" (Latour 2008:441). The attention paid to Kuhn's (1962) concepts, such as "paradigms" and "normal science," as well as its common re-inscription as a predecessor of science studies (indulged here), may reflect how his ideas actually reaffirmed scientists' rationalizations of their discoveries as innovative breaks from previous cognitive orders (Fischer 2007b:588; Stengers 2000:5).

those that they consider invalid *symmetrically* (Bloor 1973, 1976), with the ultimate goal of identifying the role of social processes in distinguishing true and false claims.

SSK was the original approach to what, by the 1980s, would be termed the “new” sociologies of science. The new sociologies – SSK and ANT as well as the social construction of technology (SCOT) – initially seemed to share a “constructivist” orientation, or a tendency to explain scientific knowledge and innovations as the outcomes of social interests, beliefs, and power-dynamics. Their claim to novelty also turned on their attention to the actual practices of scientific knowledge production, as opposed to more abstract epistemological questions (Fischer 2007b). Regardless of whether Bloor’s program in particular was a “reaction,” the constructivist orientation amounted to a strong critique of Mertonian assumptions. John Law, a sociologist involved in formulating ANT, summarizes the gist of the constructivist turn thusly: “The argument is that it is wrong to imagine that nature somehow impresses its reality directly on those who study it if they just set aside their own biases. The picture of science offered by Merton is not right” (Law 2004:19).

As an alternative to abstract epistemological analyses, sociologists, philosophers, and – increasingly – anthropologists applied ethnographic methods to examine “science-in-the-making” (Latour 1987; Shapin 1992b), or the translation of material, laboratory practices into circulating texts that proposed “facts.” ANT was just one school of thought within a growing body of research emerging at this time that emphasized the necessity of understanding scientific practices as a prerequisite to apprehending the power, significance, and consequentiality of scientific claims (e.g. Cartwright 1989; Galison 1987; Hacking 1983, 1991; Lenoir 1998; Pickering 1984,

1995; Wise 1988). Within the context of this movement, ANT demonstrated precisely how the actions of both humans and machines precipitated, enabled, and constrained the production of scientific knowledge, showing that both scientific data and phenomena are “constructed” in laboratories through the careful arrangement and coordination of heterogeneous entities (relational networks of “actors” or “actants” which include the scientists, scientific objects such as “microbes,” laboratory equipment, publications or “inscriptions,” institutions, funds, governments, etc.) (Latour 1987, 2005b; Law and Hassard 1999). Thus, what could be called “the laboratory turn” (see Knorr Cetina 1995) demonstrated precisely how science is itself a set of cultural institutions and practices, no longer a “culture of no culture” (Traweek 1988). Cultural and natural actions could no longer be distinguished ontologically and even ethnographically.

ANT studies (e.g. Callon 1986; Latour 1992; Latour and Woolgar 1986[1979]), however, were not uniformly accepted as viable contributions to constructivist sociologies of science and technology. In this respect, they differed from the work of advocates of the social construction of technology (e.g. Bijker 1994). Advocates of SSK and ANT shared skepticism towards scientists’ capacities to proffer representations that neutrally depicted or corresponded to an “external,” natural world. But this shared skepticism inspired divergent research orientations. Advocates of SSK had worked to destabilize the certainty of scientists’ claims by revealing how scientists are just another set of humans involved in constant social negotiations (e.g. Bloor 1976; Collins 1999[1974]; Lynch 1993). Advocates of ANT, on the other hand, radicalized the problem of representation, questioning not just how humans effectively represent the world, but how nonhumans play active roles in the scientific knowledge production. The

novelty of ANT countered that of conventional “social constructivism,” in granting *more* agency to nonhumans (than scientists) rather than granting less. For example, sociology Michel Callon (1986) classically defended the representation of scallops as legitimate actors in scientific negotiations, a position further described below. Whereas the social constructivists desired studies that demonstrated how interpersonal interactions and negotiations were fundamental to the construction of representations of the world, the ANT advocates sought to expand the meaning of *the social* to include the multitude of nonhumans that participate in all domains of activity, including science.

This divergence came to a head in a set of chapters now referred to as the “epistemological chicken debate” (Callon and Latour 1992; Collins and Yearley 1992a, b).<sup>12</sup> The debate emerged out of Harry Collins and Stephen Yearley’s (1992a) claim that Bloor’s principle of symmetry (which they tweak and re-designate “meta-alternation”) serves as a superior sociological tool to anything offered by the two rising trends within the sociology of science: “reflexivity” (e.g. Ashmore 1989; Woolgar 1988; Woolgar and Ashmore 1988) and ANT. Collins and Yearley’s major objection to ANT concerns Michel Callon and Latour’s attribution of agency to nonhuman objects.<sup>13</sup> Collins and Yearley (1992a) defend the ability of sociologists to have expertise about

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<sup>12</sup> “Epistemological Chicken” was the title of Harry Collins and Stephen Yearley’s (1992a) initial contribution to the debate. The reference is to the childhood game of “chicken,” in which competitors run across a busy road, competing to delay their crossing the longest. Collins and Yearley (1992a:302) claimed that the new scholarly collectives diverging from classic SSK frameworks (ANT and the reflexivists) were engaging in an “escalation of skepticism,” an *epistemological* version of chicken destined to destroy the competing parties.

<sup>13</sup> Collins and Yearley (1992a) also object to Latour’s use of what they call “the counterfactual method.” Fundamental to Latour’s concept-work is a practice in which he speculates about how actors play consequential roles in material systems by questioning what would happen to the systems if they were removed. For example, in the case to which Collins and Yearley (1992a) object, Latour (1992) deduces the agency “delegated” to an electric door by asking what alternative means of entering a building would be in the absence of the door. What work does the door do by virtue of its existence? Collins and Yearley see such speculative experiments as a kind of sophistry that lacks methodological control. Thus, they cannot accept the more important point that ANT is at its core an expansion of the social to nonhumans.

the human, intersubjective dimensions of scientific research. But they think that sociologists lack the training and conceptual resources to attend to the worldly materials that scientists construct as their objects of knowledge. For Collins and Yearley (1992a), these objects are not part of social systems and any claim that they *act* rather than *behave* is, at best, anthropomorphism and, at worst, a behaviorist negation of human agency. Instead, they leave knowledge of the nonhuman world to the scientists themselves, and concentrate on the intersubjective processes by which scientific explanations achieve assent among members of scientific communities.

Callon and Latour's (1992) response emphasizes how Collins and Yearley (1992a) misapprehend the core objectives of ANT as an empirical philosophy. Collins and Yearley present the possible epistemological alternatives for sociologists of science as the natural realism of the scientists (and positivists like Merton) or a social realism inherited from SSK. In Collins and Yearley's perspective, granting agency to nonhumans entails giving ground to the scientists' "naïve" realism and thus eviscerating science studies' "moral struggle to strip science of its extravagant claim to authority" (Callon and Latour 1992:346). Callon and Latour counter that their project does not entail accepting any ontological distinctions between social and natural worlds, or social and natural explanations.

Callon and Latour's project is neither a "modernist" positivism that accepts scientific accounts as neutral reflections of the natural world, nor a "postmodernist" social constructivism that denies the possibility of coherent explanations of how the world works. It is, rather, an attempt to define an entirely different ontology. The purpose of this ontological project is to open up the possibility for understanding the

production of scientific knowledge without recourse to deterministic macro-actors like “nature” or “society:” “Wherever we devise a hybrid that carries *some* weight – the mass spectrometer of the TRF story, the immutable mobiles, the spokesperson, the texts – Collins misunderstands us and accuses us vehemently of bringing nature back in” (Callon and Latour 1992:365, emphasis in original).<sup>14</sup> So, if sociologists and philosophers debating the respective agencies of scallops, electric doors, and mass spectrometers are not “bringing nature back in,” what exactly *are* they doing? Of what does this ontological “third way” consist?<sup>15</sup>

### **Key Concepts in ANT**

There are now several existing attempts to answer these questions and identify ANT’s major contributions to the sociology and philosophy of science. Latour himself offers a rather systematic overview of ANT’s methodological disposition in *Reassembling the Social: An Introduction to Actor-Network-Theory* (2005b). Law devotes several chapters to ANT-related approaches in his book *After Method: Mess in Social Science Research* (2004). Additionally, philosopher Graham Harman has recently argued that Latour’s approach constitutes an original metaphysics, in *Prince of Networks: Bruno Latour and Metaphysics* (2009). Though Harman does not delve deeply into Latour’s more recent works, the first half of his book probably offers the best existing primer on Latourian philosophy. At Latour’s behest (Harman 2009:12), Harman

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<sup>14</sup> Two terms in this passage may require clarification. First, “the mass spectrometer of the TRF story” refers to Latour and Woolgar’s (1986[1979]:Ch. 3) account of scientists’ “social construction” (or redefinition) of TRF (thyrotrophin releasing factor) as a peptide. This construction entailed involving a mass spectrometer, as well as other technologies, as significant actors that merit acknowledgement in sociological explanations of fact construction. Second, “immutable mobiles” is an analytical term developed by Latour (1987) to designate scientific representations such as graphs, which circulate throughout collectives of scientists, and presumably inspire scientists’ assent.

<sup>15</sup> I should also note that the epistemological chicken debates presented no conclusion to the SSK-ANT methodological conflict. Similar issues flared up in 1999, sparked by an article written by David Bloor, bluntly titled “Anti-Latour” (Bloor 1999a, b; Latour 1999b).

begins his inquiry with a review of the key concepts outlined in *Irreductions*, a ninety-page set of philosophical aphorisms appended to Latour's historical study *The Pasteurization of France* (1988). Harman helpfully organizes the metaphysics of Latour's *Irreductions* through a list of basic axioms summarized by four terms: actor, irreduction, translation, and alliance. Thus, with the aid of Harman (2009), here I introduce these four key concepts as the basis of Latour's metaphysics, and a requisite vocabulary for grasping my reinterpretation of hieroglyphic decipherment.

The first concept identified by Harman could be termed Latour's "reality principle." In Latour's analyses, we find a world comprised of *actors* (or "actants")<sup>16</sup> that are all equally real: "An atom is no more real than Deutsche Bank or the 1976 Winter Olympics, even if one is likely to endure much longer than the other" (Harman 2009:14). Genes are no more or less real than individuals. Mickey Mouse is no more or less real than Bruno Latour. The ancient Maya king Pakal is no more or less real than the contemporary epigrapher David Stuart. Thus, Latour advocates a *democratic* ontology, in the sense that all actors that effect consequences in the world are equally real, if not equally enduring, represented, or allied (see below). Philosopher Manuel DeLanda (2000; Harman 2008) calls his similar realist position a "flat ontology," suggesting that it does not hierarchize sets of actors. Discussing biological categories, DeLanda (2000:2-3) writes,

while an ontology based on relations between general types and particular instances is hierarchical, each level representing a different ontological category (organisms, species, genera), an approach in terms of interacting parts and emergent wholes leads to a flat ontology, one made exclusively of

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<sup>16</sup> Latour occasionally substitutes the semiotic term "actant" (Greimas and Courtés 1979:3) for "actor" in order to help evade or trouble the common assumption that humans represent the only acting agents in the world.

unique, singular individual entities, differing in spatio-temporal scale but not in ontological status.

In Latour's approach, this rejection of hierarchies as well as major ontological divides (natural/cultural, human/nonhuman, spiritual/material) derives from his position that all entities or actors lack essences; they are always utterly or fully "deployed in the world" (Harman 2009:14). We cannot infer that there are underlying structures hidden by the superficial appearances of objects. But this does not leave us in a house of mirrors where nothing is what it seems. Quite to the contrary, all actors are concrete, real things that can affect all other actor. The world *is* exactly how it seems. This is the fundamental basis of Latour's metaphysics.

This capacity of any actor to articulate with (or "ally" – see the fourth axiom below) any other is what drew the ANT theorists to the term "network," before the rise of the Internet and the adoption of the term by globalization and media theorists altered its scholarly meanings. These more recent senses of "network" differ from the ANT concept, in that a Latourian network is composed of heterogeneous actors rather than a homogenous set of forces (Latour 1999c). Gilles Deleuze and Felix Guattari's (1987[1980]) sense of the term "assemblage" has largely come to replace "network," as the former term seems to better encapsulate how actors constantly "assemble," entering into association and performing entirely new actors. Networks are heterogeneous assemblages of actors. Laboratories, human bodies, and books are no less "networky" than the Internet or global activist collectives. Thus "network" is less a topological form than an ontological category.

The second axiom is *irreduction*: "nothing is, by itself, either reducible or irreducible to anything else" (quoted in Harman 2009:12; Latour 1988:158). By this,

Latour means that no object or concept ever fully replicates or substitutes for another (in representation or other types of relations). Genes do not autonomously determine personalities. Socioeconomic interests do not autonomously explain scientific interpretations. As an example relevant to this study, line-drawn images of hieroglyphs cannot transparently reproduce the inscriptions carved into architectural panels.

Harman (2009:13) aptly describes the profound implications of the basic principle that actors do not directly reduce to or transform into actors of other types without mutation:

Every human and nonhuman object now stands by itself as a force to reckon with. No actor, however trivial, will be dismissed as mere noise in comparison with its essence, its context, its physical body, or its conditions of possibility. Everything will be absolutely concrete; all objects and all modes of dealing with objects will now be on the same footing. In Latour's new and unreduced cosmos, philosophy and physics both come to grips with forces in the world, but so do generals, surgeons, nannies, writers, chefs, biologists, aeronautical engineers, and seducers [Latour 1988:154-6]. And though all these examples of actors are human, they are no different *in kind* from the forces that draw objects to the center of the earth or repress desires in the unconscious. The world is a series of negotiations between a motley armada of forces, humans among them, and such a world cannot be divided cleanly between two pre-existing poles called 'nature' and 'society'.

Yet, while no actor straightforwardly reduces to another, this does not mean that explanation is impossible. Rather, "it is always possible to explain anything in terms of anything else – as long as we do the work of showing how one can be transformed into the other, through a chain of equivalences that always has a price and always risks failure" (Harman 2009:15). Socioeconomic interests exist and they might shape scientific interpretations, but this has to be demonstrated through empirical and writerly work by tracing how they touch and transform these interpretations materially. In turn, any attempt to *explain* a phenomenon entails materially *intervening* in and thus altering that phenomenon (a rather profound implication for any historical analysis).

Understanding how Latourian actors touch and alter each other without reduction requires the third axiom: *translation*. In the Latourian metaphysics, all action occurs through chains of local mediation. With the concept of translation, Latour emphasizes the importance of recognizing or acknowledging each step through which one type of object becomes another. As Latour (1999d:311) puts it in a more recent definition of the term, “[translation] refers to all the displacements through other actors whose mediation is indispensable for any action to occur.” This is probably best exemplified in Latour’s empirical chapter “Circulating Reference,” included in *Pandora’s Hope: Essays on the Reality of Science Studies* (1999d), wherein Latour follows step-by-step how Brazilian scientists gradually transform observations, measurements, and soil samples into a report on the shifting relation between forest and savanna ecological zones. This straightforward empirical study signals Latour’s divergence from the Kantian philosophical tradition in holding that there is no absolute distinction between human consciousness or representation and the external world. The noumena/phenomena (or mind/world) distinction collapses into a multiplicity of linkages between worldly actors.

Scientific concepts form through articulations between actors. In the ecological research narrated in “Circulating Reference,” these actors include human bodies as well as topofils, pedocomparators, and Munsell soil color charts. Each of these *mediators* actively shapes the formation of new objects, ultimately including scientific texts. As Harman (2009:15) puts it, “Latour’s guiding maxim is to grant dignity even to the least grain of reality. Nothing is mere rubble to be used up or trampled by mightier actors. Nothing is a mere intermediary. Mediators speak, and other mediators resist.” Further, in these chains of translation, no mediator simply becomes the next without any

deformation. Building on the work of Latour's teacher, philosopher of science Michel Serres (1974), Law (2007) emphasizes how translation inevitably entails betrayal; to translate is to traduce. All mediations require that the qualities of the participating actors transform. In mediation, actors both gain and lose qualities. All real actors are participating in mediation. Indeed, these mediatory relations constitute the very existence of objects, as Latour's metaphysics effaces the possibility of objects' underlying essences. Beyond individual objects' essences, Latour further differs from other non-Kantian philosophers such as Henri Bergson and Deleuze by denying any singular, underlying substance, dynamism, or flux of becoming from which actors issue (Harman 2009:6, 30). Thus, Latour must hold the position that all worldly actors exist in a state of constant transformation and mediation (and our accounts of scientific practice should reflect this).

At this point, Latourian metaphysics may seem quite radical in its attribution of agency to all human and nonhuman actors and its extreme sociotechnical relationism, or "interobjectivity" (Latour 1996).<sup>17</sup> Does this radical metaphysics leave us with no ability to judge the accuracy or effectiveness of scientific arguments? Does it force us into an epistemological relativism in which an explanation that the flu is caused by viral infection is no stronger than an explanation that the flu is caused by malevolent spirits? In other words, does it differ epistemologically from Bloor's symmetry? The fourth axiom provides a sufficient solution to this problem: *alliance*. An actor's strength does not issue from some underlying *res extensa*, structure, or compositional coherence, but

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<sup>17</sup> Indeed, while he invokes "stabilized" objects such as technological "blackboxes" – machines that internalize previous technological innovations – Latour's metaphysics does not seem to accept the existence of bounded, isolable objects or structures. My study evades this pure relationism through a concept of "cuts" elaborated in the section on "Postcolonial Science Studies" below.

from its allies. The stronger actors, including scientific propositions, are those that can assemble a greater number of fellow actors working towards a compatible end. Establishing and stabilizing alliances is a messy game; all participating actors must sacrifice or compromise some of their qualities in order to achieve greater strength. While discussion of “alliances” has an explicitly political sense, with Hobbesian undertones if we forget the presence of nonhuman actors (Callon and Latour 1981), recall that the research sites that inspired ANT were scientific laboratories. The stronger scientific argument is the argument most thoroughly attached, associated, or allied with a multitude of other actors, humans as well as machines that inhabit the laboratory, policies crafted by politicians, or any other worldly actors. Knowledge emerges neither through experiments serving as translucent windows into the structure of the natural world nor through the “social consensus” of a community of experts. Instead, the accuracy of scientific statements reflects the quality and stability of constellations of heterogeneous alliances that materially manifest as consequential actions.

It is somewhat unfair to reduce the entirety of Latour’s metaphysical program to the four basic axioms identified by Harman. On the one hand, Latour advocates that anthropological and sociological ethnographers work with as limited a special vocabulary as they can manage, so as to avoid overdetermining informants’ explanations with categories external to their experiences (Latour 2005b). On the other, a rather elaborate vocabulary comprised of dozens of redefined terms is fundamental to his alternative ontology (lists of definitions include Akkrich and Latour 1992; Latour 1999d:303-11, 2004a:237-50):

All the shifts in vocabulary like “actant” instead of “actor,” “actor network” instead of “social relations,” “translation” instead of “interaction,” “negotiation” instead of “discovery,” “immutable mobiles” and “inscriptions” instead of “proof” and “data,” “delegation” instead of “social roles,” ...are hybrid terms that blur the distinction between the really social and human-centered terms and the really natural and object-centered repertoires (Callon and Latour 1992:347).

This apparent contradiction concerning the utility of a hybridizing analytical vocabulary reflects what I see as a tension in Latour’s *oeuvre* between a methodological program that aids ethnographers to trace how actors’ movements are not delimited by categorical dichotomies such as nature/culture (e.g. Latour 1999a, 2005b) and a quasi-utopian program that involves imagining alternative systems of research and governance that represent ostensibly nonhuman actors more democratically (Latour 1993, 2004a, 2005a). Ultimately, however, both the methodological and the utopian dispositions arise from the desire to apprehend reality as a process of local engagement among materially heterogeneous actors. They differ merely in whether they broach this problem from the starting point of scientific research or political governance.

In this section I have traced how ANT and Latour’s specific object-oriented ontology emerged out of the sociology of knowledge. I have additionally introduced some of the basic vocabulary that define this approach to analyzing heterogeneous assemblages, what Donna Haraway (2008) calls “naturecultures.” Building on this framework, I now move to analyze the more specific cases of the science-public interface and the relevance of postcolonialist concepts of transcultural encounter and historical knowledge to the anthropology of science.

### **Public Engagement with Science**

One of the more established science studies research themes is the engagement between sciences and publics. Building on the basic theoretical approach presented

above, in this section I briefly explore how science studies scholars have become concerned with public participation in and engagement with scientific practices. I begin by tracing the emergence of concepts of “public science,” “public engagement with science,” and “scientific citizenship” out of critiques of the “public understanding of science” movement. Having established these concepts, I proceed to outline how they fit within broader contemporary concerns surrounding the “democratization of science.” Indeed, I follow Latour’s argument that the dual democratization of science and politics entails innovating methods to gather disparate publics around the contested, unstable, hybrid actors that inhabit both political and scientific worlds.

This is a crucial conceptual framework for my study, because Maya hieroglyph experts have prominently allied and engaged with several different publics, and these relations are mediated by mutating actants called hieroglyphs, the driving themes of Chapters 5 and 6. This section presents the concept-work necessary to better grasp the politics of encounters between “expert” and “lay” epigraphers. Since the public engagement with science literature from which I draw has focused principally on biotechnologies and biomedicine (e.g. Epstein 1996; Hagendijk 2004; Horst 2007; Irwin 2001; Michael 1998; Michael and Brown 2005; Wynne 2001, 2005), there remain questions surrounding the utility of this framework for historical knowledge production. After I present contemporary approaches to apprehending public science here, the following section and chapter more directly address the problem of historical science as public science.

Concerns with the authority popularly attributed to empirical knowledge date to the seventeenth-century emergence of institutions and practices recognizable today as

“science” (Haraway 1997; Shapin and Schaffer 1985). However, the construction of science-society relations as an object of research itself did not develop until relatively recently. The concepts of “public engagement with science” and “public science” that I employ in this study have their roots in 1980s research into the public understanding of science (PUS). The designation “PUS” brushes over two very divergent orientations to science-public interfaces. On the one hand, PUS was institutionalized through a report issued by the English Royal Society (1985), reflecting scientists’ and politicians’ concerns that public apathy or antipathy towards scientific innovations and technical knowledge reflected misunderstanding or ignorance of science. At this same time, however, scholars associated with the SSK movement came to examine how the institutions and propositions of science played important, even “prescriptive,” roles in ostensibly non-scientific domains of society (Wynne 1995:362).

PUS has always been characterized by a tension between researchers promoting the authority of science and researchers seeking to understand science as an authoritarian social system hiding the risks of scientific research (e.g. nuclear engineering and genetically modified organisms). This opposition is apparent, for example, in the opening issue of the journal *Public Understanding of Science*. The first author of the journal’s initial article was Sir Walter Bodmer, the chair of the working party that authored the Royal Society report. Bodmer and Janice Wilkins (1992) present the program of the UK Committee on the Public Understanding of Science (COPUS), questioning how to improve public appreciation of science by reaching demographics with minimal scientific awareness. However, some of the issue’s articles by authors involved in the new sociologies of science reflect a very different orientation.

Historian Steven Shapin (1992b) wishes to invite the general public into the messy world of the laboratory to become lay ethnographers of science-in-the-making, demystifying scientific practice and subjecting it to sustained public scrutiny. Likewise, sociologist Brian Wynne teases out how “public identification with science” is no effective response to “public lack of understanding of science,” and casts Bodmer and his colleagues’ rhetoric of public ignorance as “the scientific establishment’s anxious response to a legitimation vacuum which threatened the well-being and social standing of science” (Wynne 1992:38).

Following from my adoption of the basic premises of a Latourian ontology, my orientation towards PUS accepts the “critical” stance, assuming that science is not an ethically and epistemologically unproblematic source of either authority or knowledge for popular consumption. I find inspiration for this position in Wynne’s myriad publications (see also Irwin and Michael 2003; Jasanoff 2004; Nowotny et al. 2001), some of which integrate the respective propositions of ANT and critical PUS. In an assessment of PUS published in 1995, Wynne most explicitly discusses the emergent field’s relation with ANT. Here, he hints at how close attention by critical PUS practitioners to public-science relations provides a necessary corrective to ANT-style analyses (Wynne 1995:383-4). Specifically discussing Callon’s (1986) classic study of negotiations among scallops, fishermen, and scientists in St. Brieuc Bay,<sup>18</sup> Wynne reaffirms Callon’s point that engagement of the scallop fishermen (a kind of public) in the scientific system

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<sup>18</sup> Callon’s (1986) case study has become a model for the utility of ANT’s expansion of the social to the nonhuman realm. The chapter traces a series of steps through which the actions of scallops, fishermen, and scientists become coordinated in a way that enables the scientists to represent both the interests’ of the fishermen and the actions of the scallops (particularly whether or not they anchor to the seabed). Callon’s argument emphasizes how ANT provides resources not solely to describe natural and social process in the same vocabulary, but also to trace how a few actors – here, scientists – achieve the authorization to represent (or translate) the multitude of unspeaking actors with which they have come into contact.

entails a renegotiation of the science itself. That the fishermen are best understood as active players in constructing knowledge of scallops' actions resonates with what we might call Wynne's basic axiom of public science, that publics are integral actors in all stages of scientific research. However, critical PUS scholars' attention to the constitutive interrelations of sciences and publics also aids in shedding light on how scientific systems cannot be understood as isolates. Wynne (1995:383-4) makes this point by objecting to Callon's characterization of the scallop fishermen's sudden withdrawal from the scientific research project as a "betrayal." For Wynne, the term "betrayal" implies that the participants in a scientific actor-network must be fully enrolled, lacking simultaneous enrollment in intersecting or disparate assemblages. Instead of seeing the fishermen's withdrawal as a monolithic betrayal, Wynne suggests that it may reveal this public's "ambivalence" based in actors' competing alliances.

So critical PUS perspectives compel us to examine the constitutive roles of publics in strengthening or legitimating scientific actors, and they remind us that systems of scientific alliances intersect with nonscientific networks. As Wynne puts in:

I also propose...to reconsider the assumed object of science studies itself, namely 'science', and look more closely at *scientific knowledge in its implicit public roles* as an essential element of modern science, not just an extra domain of post-science 'applications' (Wynne 2005:68, emphasis in original).

I talk of *public science*, in the sense of scientific knowledge in which we may identify such implicit human-public dimensions as part of the science itself (Wynne 2005:68-9, emphasis in original).

Such perspectives have the effect of challenging the ideological, institutional, and material bases of *expertise* (see also Collins and Evans 2002; Durant 2008; Selinger and Crease 2006; Wynne 2008). While they may have domain-specific knowledge rooted in trained material practices, the scientific authority of experts also depends

integrally on the participation of non-experts, lay scientists and knowledge consumers most prominently. Thus, the critical PUS orientation strongly disavows the Royal Society's cultivation of popular deference to scientific authority, and reaffirms the established science studies argument that contemporary sciences are integral sites of cultural production thoroughly embroiled in other cultural domains (Franklin 1995; Haraway 1997; Harding 1998; Knorr Cetina 1999; Martin 1998; Traweek 1988). Indeed, popular understanding of specialized knowledge is a less interesting contemporary problem than the various means by which "science" and "society" (including publics) issue from the same material and epistemic practices (Gibbons 1999; Jasanoff 2004; Latour 2004a; Nowotny et al. 2001; Wynne 2005). Following from Latour's flat ontology, grand concepts like "science," "nature," "culture," "modernity," "ideology," and – I might add – "history" emerge as performative *effects* of materially hybrid transactions or alliances; they are objects that demand explanation rather than resources that (social or natural) scientists can use to explain observed phenomena (Latour 1993, 2005b; see also Ong and Collier 2005).

Of course, the concept of *public science* not only challenges the superficial notion of science as a domain of action outside of culture, a "culture of no culture" as anthropologist of science Sharon Traweek (1988) put it, but it also invokes important questions about what we mean by the word *public*. Thus, I close this section by reflecting on contemporary conceptualizations of the public that draw together the ANT and public engagement with science literatures. First of all, Latour has innovatively explored concepts of the public in his recent work. Perhaps his primary goal in engaging questions of the public has been repositioning things as active players in

political and scientific deliberations. Latour's recent work on what he calls "cosmopolitics" or "*Dingpolitik*" (Latour 2004a, b, 2005a, 2007) can be read as a set of attempts to explore how practices deemed scientific, artistic, or political entail ontologically indistinct gatherings or actor-networks that weave together disparate human and nonhuman actors to innovate new collectivities. Thus, while marked by Latour's own idiosyncratic style, this project clearly contributes to arguments that scientists imagine, produce, and depend on publics (Wynne 2005), and that the humans comprising these publics are produced as "scientific citizens" subjected to science's control of their bodies, environments, and foods while they also shape the contours and dynamics of science itself (Elam and Bertilsson 2003; Horst 2007; Irwin 2001; Michael 1998; Michael and Brown 2005; Rose and Novas 2005).

Latour's conceptualization of publics entails reorienting political philosophy to the objects that make up the world (Latour 2005a). Humans are awash among multitudes of other actors depicted in this political scenography. A more realist and realistic political philosophy is one that does not shy away from the heterogeneous materialities or actor-networks that we inhabit and that inhabit us. The image of the "public sphere" as a kind of European coffeehouse where we naked human subjects can negotiate the terms of our governance through dialogue and debate (e.g. Habermas 1989[1962]) fails not solely because of its insistence on rationality, as Michel Foucault argued (see Kelly 1994), but because it limits the enrolled political actors to purified humans, a very small subset of all the things that collectively participate in politics and thus perform contemporary worlds:

By the German neologism *Dingpolitik*, we wish to designate a risky and tentative set of experiments in probing just what it could mean for political

thought to turn “things” around and to become slightly more *realistic* than has been attempted up to now. A few years ago, computer scientists invented the marvelous expression “object-oriented” software to describe a new way to program their computers. We wish to use this metaphor to ask the question: “What would an *object-oriented* democracy look like?”

The general hypothesis is so simple that it might sound trivial – but being trivial might be part of what it is to become a “realist” in politics. We might be more connected to each other by our worries, our matters of concern, the issues we care for, than by any other set of values, opinions, attitudes or principles (Latour 2005a:14, emphasis in original).

Each “issue” is an uncertain thing/assembly (German: *ding*) that gathers disparate humans and nonhumans: “the entry of Turkey into the European Union, the Islamic veil in France, the spread of genetically modified organisms in Brazil, the pollution of the river near your home” (Latour 2005a:14). This vision of the public – adding the *res*, or thing, back to the *respublica* – importantly clarifies much of the literature on scientific citizenship by showing that our rights and obligations as scientific citizens do not play out in the abstract but through tangible, material connections with each other and the varied objects that compose our worlds. We are not citizens of Science, but of multiple specific, local sciences.

I trace such tangible material connections at work in a single local science in my treatment of Austin, Texas and Antigua, Guatemala workshops on Maya hieroglyphic writing in Chapters 5 and 6. Maya hieroglyphs are strange, thought-provoking, even wild objects that have actively drawn together internally heterogeneous communities whose innovations have shaped the ways in which the past can be known. We have to examine simultaneously the literary, aesthetic, and material practices that have preconditioned such gatherings by constructing hieroglyphs as objects and the role of the attending publics as allied actors whose presence helps to validate historical

interpretations with contemporary consequences for indigenous Mayas, whose very status as *indigenes* rests in part on such public science.

Anthropologist of science and activism Kim Fortun (2001) has developed a framework that may aid in grasping publics, such as Maya Meetings attendees and Maya activists, as collectives emergent out of material practices. She terms such collectives “enunciatory communities.” Discussing new collective subject positions and systems of signification that formed in response to the environmentally-disastrous release of toxic gas from a pesticide plant in 1984, in Bhopal, India, Fortun (2001:13) formulates the “enunciatory community” as an alternative to the “stakeholder” concept common in recent social analysis:

Enunciatory communities are different from stakeholder communities in a number of ways. First, their identity cannot be divorced from context. If citizens are irrational, it is because they are reacting to an irrational context. Second, the identity of enunciatory communities is always assumed to be fissured within, even when members themselves insist otherwise. Enunciatory communities are, by definition, morphologically complex. Third, enunciatory communities are not expected to devolve into benign pluralism. They cannot be aggregated into a harmonious whole. Their differences are considered a resource, rather than the problem.

Most important are the discontinuities. Enunciatory communities do not remain the same across time or across space. They are chameleonlike, morphing in response to the interplays in which they find themselves, learning as they go – developing new strategies at every turn.

Fortun’s concept of enunciatory communities resonates strongly with Latour’s presentation of networks as internally heterogeneous and relationally dynamic forms (discussed further in the next section of this chapter) elaborated out of material “issues,” *dings*. This provides a very important clarification for the concept of the public that inhabits any analysis of public science. Scientific publics do not emerge as homogenous groups of people whose relations to their material world results from

scientific innovations. Rather they are internally heterogeneous, mutating, and fissured emergences gathered around the many different objects that shape contemporary collectivities. Most importantly, internal differences do not fragment enunciatory communities. They provide the resources requisite for creative action.

Perhaps the most crucial form of this creative action is the practice of collective governance, or world-making, which Latour and Isabelle Stengers (2005) term “cosmopolitics.” Cosmopolitics is the constant, laborious project of trying to develop democratic means to collaborate across difference in constructing common worlds (Latour 2004a), and hopefully more peaceful common worlds. It could be thought in Latour’s older vocabulary as the labor of alliance, though, in my reading, it is now heavily inflected by ethico-moral imperatives to *care* for the objects with which we become associated (e.g. Haraway 2008), a problem that requires clear recognition that all actors are embroiled in incongruous networks (Wynne 1995).

Thus, publics emerge out of attempts to collectively, materially craft new and contingent ways to live together. One of the most prominent sites of emergence for such publics – and we could clarify the concept by using “enunciatory publics” – is scientific practice, due to the proliferation of scientific propositions and technologies in the performance of contemporary life. Such a formulation of the public, however, depends on the capacity to define ethical parameters that govern which alliances we should extend and which we should evade when there are conflicts or “ambivalences.” The following section begins to define these parameters by pushing object-oriented studies of public science into engagement with the historiographical critiques formulated by postcolonial theorists. My intent is to extend science studies’ analytical object and

introduce a critique of ANT with reverberating ethical and epistemological consequences.

### **Postcolonial Science Studies**

Thus far in this chapter, I have presented Latourian metaphysics as the basic conceptual orientation of my study and then tailored this orientation to the distinct phenomenon of *public science*. Having established these conceptual guidelines, this section sketches the framework for a Latourian analysis of public *historical* sciences. This entails addressing the question of how history differs as an analytical object from the sciences more conventionally subjected to science studies inquiries, such as physics, genomics, and health sciences. I argue that engaging the general research program of “postcolonial science studies” facilitates this extension.

“Postcolonial science studies” scholars have borrowed heavily from postcolonial theory in conceptualizing how subjects negotiate encounters among distinct, conflicting knowledge systems (often in colonial contexts). I should note up front that while postcolonial theory and science studies may seem like ready allies in challenging truth-claims, their critiques have frequently issued from disparate motives. For postcolonial critics, decentering Western truth-claims has served to denaturalize the colonial order and thereby legitimize the knowledges of marginalized subjects in the effort to “decolonize the mind,” as Ngũgĩ wa Thiong'o (1986) famously put it, cultivating oppositional consciousness, politics, and literature. In contrast, the efforts of many science studies scholars to decenter, historicize, or relativize scientific truth-claims are less directly tied to issues of historically-produced social inequalities.

Indeed, science studies scholars approaches to knowledge production have been marked by distinctions between philosophical and activist orientations (Fuller 1993).<sup>19</sup> The philosophy-oriented scholars who have been most intent on determining the historicity of scientific knowledge claims inherit heavily from SSK's efforts to rectify a perceived asymmetry in the explanation of scientific knowledge, the assertion that "social" factors contributed solely to scientific error and not positive scientific knowledge (Sismondo 2008). As an alternative, SSK vitalized the notion that all scientific knowledge responds to social and political interests (e.g. Barnes and Bloor 1982; Bloor 1976). Thus, part of the reason that science studies scholars have challenged the veracity, legitimacy, and transcendence of scientific knowledge derives from philosophical concerns surrounding scientists' asymmetry in their forms of explanation. The "activist" scholars and those involved more generally in cultural studies of science, on the other hand, tend to share a desire to "decolonize the mind" and expose the workings of colonialism, racism, and sexism through technological and scientific practices (while typically also recognizing the asymmetry addressed by the sociologists of scientific knowledge). As elaborated further in this section, "postcolonial science studies" scholars tend to embrace the critiques elaborated within the "philosophical" orientation (including ANT), while also critiquing the roles of sciences in producing inequalities, in "activist" fashion.

To further complicate matters, I position this analysis of historical knowledge production as an exercise in postcolonial science studies that requires engaging with postcolonial studies and subaltern historiography in a style that differs from existing postcolonial science studies accounts. As described below, science studies scholars

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<sup>19</sup> Though, as discussed in Chapter 3, this divide has become more permeable (Sismondo 2008).

have conceptualized their work as “postcolonial” when analyzing encounters between subjects of different knowledge systems. I hold that “postcolonial science studies” should also entail the adoption of the *historical sciences* as objects of analysis, given the prominent attention to negotiations over historical narrative emphasized by postcolonialist historiographers and cultural critics (e.g. Chakrabarty 2000; Chatterjee 1993; Dirks 1990; Guha 1988[1982], 2002; Prakash 1995; Said 1993; Spivak 1999; Stoler 2002). While conventional approaches to postcolonial science studies attend to confrontations between colonialist and subaltern *technoscientific* knowledges (e.g. Anderson 2002b; Anderson and Adams 2008), I hold that such a research framework must also closely attend to *historical* knowledges. I further examine how engaging a subalternist ethics of history affirms important critiques of Latour’s ontology, explored through the concept of “cuts.”

To show how the form of postcolonial science studies that I advocate contrasts somewhat with the studies that have defined the category so far, it is necessary to present the existing definitions of the field. Though questions about the political geography of scientific knowledge have a longer genealogy, feminist science studies scholar Sandra Harding deserves credit for innovating a program of scholarship under the name “postcolonial science studies.” She used the term as early as 1992 to refer to the growing body of scholarship produced by anticolonial and postcolonial intellectuals that challenged the universal application and utility of Euroamerican science: “These critics show how Western sciences are...just one kind of culturally specific ‘ethnoscience’ among the many that have existed” (Harding 1993a:311).<sup>20</sup> Harding

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<sup>20</sup> In this statement, Harding is referring to the following scholarly works: Michael Adas (1989), Susantha Goonatilake (1984), Sandra Harding (1993b), Charles Moraze (1979), Ashis Nandy (1990), Patrick

proceeds to explain that for such critics this provincialization was less a negative project of challenging truth-claims than a positive project of strengthening local sciences in the face of the possible imperialism of “Western” ethnoscience dispersing into “Third World” contexts. Thus, at the core of self-defined “postcolonial science studies” (Adams et al. 2005; Anderson 2002a; Anderson and Adams 2008; Harding 1993a, 1998, 2008; Hayden 2005; Lowe 2004) and closely allied projects (e.g. Agrawal 2005; Choy 2005; Cruikshank 2005; Langwick 2007, 2008; Mitchell 2002; Redfield 2000; Turnbull 2000; Verran 2002), we find careful examinations of encounters between culturally disparate knowledge practices in sites characterized by power differentials, what Mary Louise Pratt (1992a) famously called “contact zones.” These disparate knowledge practices have tended to group into the natural sciences of the colonial West and the indigenous knowledges of the colonial “margins” that make up most of the territorial world.

Harding’s investment in the project developed as a confluence of the feminist theory emergent out of the women’s movement, the rise of African and African-American philosophy, and the new forms of post-Kuhnian science and technology studies emerging in the 1970s and 1980s, discussed above (Harding 1998). In general, Harding effectively organizes disparate currents of cultural theory into a multifaceted critique of science as a set of cultural institutions. Her most established conceptual program involves exploring an epistemology for apprehending different ways of knowing, the premise that “different cultures’ knowledge systems have different resources and limitations for producing knowledge; they are not all ‘equal,’ but there is no single possible perfect one, either” (Harding 1998:19; see also Harding 2008:114-

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Petitjean et al. (1992), Ziauddin Sardar (1988), Ivan Van Sertima (1983), and Jack Weatherford (1988). She also points to the work of Donna Haraway (1989) and Vandana Shiva (1989) as attempts to draw from both feminist and postcolonial theory in critiquing science.

22). Given this difference, what do studies of encounters between knowledge systems look like?

Helen Verran and David Turnbull count among the more influential scholars who have provided an answer to this question, examining the encounter between “scientific” and “indigenous” knowledge systems with specificity and depth from an anthropological perspective (Turnbull 2000, 2007; Verran 2001, 2002; Watson-Verran and Turnbull 1995). Drawing from postcolonial theory and actor-network approaches to science studies, Verran and Turnbull question how Australian Aboriginal knowledge in particular and “Western” science syncretise for specific local ends, as in the example of Aboriginals and scientists developing complementary “firing regimes” for preventing forest fires and maintaining the productivity of lands (Verran 2002). Verran (2002:729) frames the often-uneasy collaboration between Aboriginals and scientists as an example illustrating Stuart Hall’s (1992:293) concept of “postcolonial moments,” which she describes as “occasions for theorizing, for telling differences and samenesses in new ways,” and proceeds to define further throughout the article:

Postcolonial moments are made where disparate knowledge traditions abut and abrade, enmeshed, indeed often stuck fast, in power relations characteristic of colonizing, where sciences usually line up on the side of the rich and powerful. Postcolonial moments interrupt those power relations, redistributing authority in hope of transformed contexts for the exercise of power. A postcolonial moment is not about retrieving a lost purity by overthrowing and uprooting an alien knowledge tradition. Rather, it might effect an opening up and loosening. Increasing possibilities for cooperation while respecting difference, postcolonial moments can lead to making amends for past injustice. Elaborating a postcolonial moment involves both making separations, and connecting by identifying sameness. But ‘sameness’ here is not a dominating universalizing. On the contrary, sameness in a postcolonial moment enables difference to be collectively enacted (Verran 2002:730).

A postcolonial moment offers ways to exit the relativist game of pitting one definitive metaphysics against another. Instead it points to possibilities for

trust in a common sense of embodied certainty in practice (Verran 2002:757).

Exemplary of the postcolonial moment is a workshop discussed by Verran (2002) in which ecologists attempt to discern Aboriginal knowledge of the landscape in their effort to improve the efficacy of firing regimes. I find this use of postcolonial orientations within a science studies framework productive because it attends to the encounter between knowledge practices through a lens that clarifies the constant irruption of historical inequality in such collaborations. Further, the example of *workshops* that assemble differentially interested sets of participants, or “enunciatory communities,” proves directly relevant to my case studies of Maya hieroglyphic workshops in Chapters 5 and 6.

At the same time, the concept of “postcolonial moments” exemplifies what many postcolonial science studies scholars have borrowed from postcolonial theory more generally: emphasis on the forms of relationality crafted in colonial encounters (including racialized collectives). Advocates of postcolonial science studies have made significant inroads into tracing the formation of such relationalities, and occasionally the inequalities that they perform, with a general cognizance of the dangers of slipping into the overdetermined and depoliticized semiotics that sometimes characterize poststructuralist expressions of postcolonial theory (which I describe further below). In the introduction to a Special Issue of *Social Studies of Science* (Anderson 2002b), Warwick Anderson (2002a:643) poses the value of postcolonial theory to science studies (and vice-versa) in these terms:

At the most basic level, a postcolonial perspective would mean that metropole and post-colony are examined in the same ‘analytic frame’ [Stoler and Cooper 1997]. But we would go beyond a recommendation of analytic symmetry and inclusion, and seek to understand the ways in which

technoscience is implicated in the postcolonial provincializing of 'universal' reason, the description of 'alternative modernities', and the recognition of hybridities, borderlands and in-between conditions. We would, moreover, argue that the study of science and technology has much to offer a postcolonial critique that has hitherto concentrated on literary representations, a 'textualism' that often has the effect of erasing the materiality and specificity of neocolonial encounters [Dirlik 1994; During 1998].

Anderson's formulation has clearly discerned and influenced a set of key problems for postcolonial sciences studies as a set of research projects within cultural studies of science. At the same time, I wish to caution that, while focused primarily on literary forms, the postcolonial critique has not uniformly elided the "materiality and specificity of neocolonial encounters" (though, to be fair, Anderson does say "often").

Indeed, in reflecting on the utility of postcolonial theory for the critical and careful analysis of science as a set of cultural formations, it is valuable to discern the degree to which specific postcolonialists build from the traditions of poststructuralism or subaltern studies. Whereas poststructuralism clearly tends towards "textualism," the historiography of the subaltern studies movement had a strong foundation in political economy and the critique of inequality, as they pertain to the valorization of historical narratives and consciousness. From my perspective, the value of subalternist historiography for contemporary science studies and empire studies has outlived the strain of postcolonial theory that draws from poststructuralism in celebrating transgression, hybridity, and disjuncture as modes of resistance to colonialism. Posed differently, this is an affirmation of the continued utility of what Malini Johar Schueller (2009) – extending the work of Ranajit Guha (1989) and Vijay Mishra and Bob Hodge (1994) – calls "resistance postcolonialism" as opposed to "collaborative postcolonialism." Schueller (2009:14) points to how the "resistance postcolonialism" of

scholars such as Franz Fanon (1967; 2004[1961]), Anne McClintock (1995), and Dipesh Chakrabarty (2000) builds on a legacy of anticolonial struggle and acknowledges the material conditions of colonialism and imperialism as partially determinant of power structures, whereas the “collaborative postcolonialism” of scholars such as Homi Bhabha (1994) and Kwame Anthony Appiah (2006) employs a language-based semiotic philosophy to theorize transcultural encounters.<sup>21</sup>

The legacy of the subaltern studies movement’s strong historiographical focus has shaped the resistance postcolonialists’ continued attention to the politics of history (e.g. Chakrabarty 2000, 2002; Guha 1989, 2002). Collaborative postcolonialists, on the other hand, have paid less attention to how control over historical materials and discourses shapes forms of colonial and imperial inequalities. I suggest that articulating science studies and resistance postcolonialism – as Anderson (2002a) seems to advocate – should entail a broadening of science studies’ analytical purchase to historical knowledge practices. I frame this problem with greater specificity in the following chapter through elaboration of the reasons that we should consider history a *scientific* semiotic-material domain in Latin American contexts. In the meanwhile, here I explore how subalternist attention to the violence of (or “cuts” entailed in) historical knowledge production helps to refine the analytical lens of this Latourian study of public historical science.

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<sup>21</sup> I should clarify that, from my perspective, the pervasive influence of both poststructuralism and subaltern studies throughout the field of postcolonial studies renders the contrast between “resistance” and “collaborative” orientations a gradient distinction rather than an absolute distinction.

## Conceptualizing “Cuts”

Cultural studies of science produced new critical methods for understanding sciences as culturally-imbricated, worldly, and local sets of sociomaterial practices (e.g. Rouse 1993). By the 1990s, Donna Haraway could critique the failure of “mainstream” science studies scholars such as Latour to consult the work of “oppositional” scholars such as herself and Harding. Pointing specifically to how Latour (1987) mimetically adopts naïve expressions of disembodied realism, outsider’s objectivity, and militarist rhetoric (e.g. “allies,” “trials of strength”), Haraway (1997:35) claims that such “mainstream” scholars rely on the same philosophical narratives that they aim to critique and that they fail to account for how gender, race, and class form through the actor-networks that they trace:

First, failing to draw from the understandings of semiotics, visual culture, and narrative practice coming specifically from feminist, postcolonial, and multicultural oppositional theory, many science studies scholars insufficiently examine their basic narratives and tropes.... Second, many science studies scholars, like Latour, in their energizing refusal to appeal to society to explain nature, or vice versa, have mistaken other narratives of action about scientific knowledge production as functionalist accounts appealing in the tired old way to preformed categories of the social such as gender, race, and class.... It is past time to end the failure of mainstream and oppositional science studies scholars to engage each other’s work. Immodestly, I think the failure to engage has not been symmetrical.

In her commentary on Latour, Haraway renders explicit how in the absence of a clearly defined set of political objectives, the critique of scientific truth claims has little value, and ultimately displaces one form of epistemological absolutism with another.

I propose that the concept of “cuts” may effectively serve as the ground for such an ethico-political positionality. By “cuts,” I mean the processes through which actor-networks enact exclusions that enable them to be experienced as singular, bounded objects (see also Star and Griesemer 1989). As discussed further below, my use of the

term “cuts” integrates the ontological sense elaborated in Strathern’s (1996) critique of ANT and the ontological, epistemological, and ethical senses elaborated by Barad (2007) in her feminist history of quantum physics. I hold that the term has special value in its ability to locate an ontological isomorphism between the production of coherent scientific objects and the racialization, or “marking,” of social collectives. When used to reconceptualize the consequences of historical knowledge practices, the concept of “cuts” further brings ANT into close alignment with the ethical stance of historiographical subaltern studies, the notion that contemporary forms of historicism fundamentally subject the colonized to narratives that define Europe as the paradigm of modernity and progress (Chakrabarty 2000) and thus perform “epistemic violence” (Spivak 1988) through representation that further entrenches subjects’ marginalization by constituting them as absolutely other, a type of “cutting” (see below).

Strathern articulates her concept of cuts in a 1996 article entitled “Cutting the Network.” Her piece can be read as a critique of the diffusionist character of Latour’s concept of “networks.” In other words, if Latourian actors constantly ally with one another through material mediations, with their properties fully contingent upon this dynamic relationality, and no inherent distinctions between types of objects capable of allying, how do discernibly bounded objects emerge? Strathern builds the critique of this diffusionism through her comparative ethnographic work on concepts of kinship and property in Highland Papua New Guinea and England. Grasping this critique may require a more succinct and direct definition of the “network” than I have provided so far. As Strathern (1996:521) puts it, “The concept of network summons the tracery of heterogeneous elements that constitute an object or event, or string of circumstances,

held together by social [meaning associational, not human] interactions: it is, in short, a hybrid imagined in a socially extended state.” As she takes pains to point out, this “network” concept is supposed to differ fundamentally from conventional sociological “network analysis,” especially studies of kinship networks. The ostensible difference resides in the assumption (though Strathern shows that it is an inaccurate assumption) that kinship networks are fields of constitutively *homogenous* relations, or relations among humans, whereas Latourian actor-networks are, by definition, heterogeneous, or relations among actors attributable to distinct morphological categories, including humans, animals, technologies, and concepts.

Similar to Deleuze and Guattari’s (1987[1980]) “rhizome” (Latour 1999c:19), the network is an assemblage of multiple actors into a discernable singularity. It contains heterogeneous interacting multiplicities, but it is a singularity nonetheless. All actors are networks and all networks are actors. As Strathern (1996:523) puts it, “I shall argue that if we take certain kinds of networks as socially expanded hybrids then we can take hybrids as condensed networks. This condensation works as a summation or stop.” In tension with Latour’s abstract relationism, the very concept of a hybrid network implies that there exist disjunctures or boundaries between different types of actors. Some actors do not interact, *despite* their material contiguity or proximity. Hybridity has its limits.

Examples of such singularities can be found in Latour’s own work. For example, “blackboxes” such as machines in scientific laboratories are “condensed networks” defined by their boundaries. While a mass spectrometer, for example, internalizes and stabilizes the innovations of earlier scientists and technologists, it does not simply

vanish when unused. Instead, it becomes “background” within the laboratory. Further, its use entails an expansion of its actor-network through alliance with a scientist, or other user. But this use does not alter its internal functioning. If this technological actor fails, the blackbox opens, and reveals a multitude of interactants that the scientists otherwise need not acknowledge in their accounts.

For Latour, the notion of “blackboxing” reveals how progressive scientific innovations counterintuitively result in less transparent knowledge production.

“Blackboxing” is

an expression from the sociology of science that refers to the way scientific and technical work is made invisible by its own success. When a machine runs efficiently, when a matter of fact is settled, one need focus only on its inputs and outputs and not on its internal complexity. Thus, paradoxically, the more science and technology succeed, the more opaque and obscure they become (Latour 1999c:304).

This lack of transparency derives from the relative autonomy of certain technologies in the laboratory. Blackboxes are objects cut from their relational, material contiguities, ostensibly allowed to exist independently of their relations. In addition to the technologies emphasized in Latour’s account, Strathern shows readers how sets of relations, such as medical innovations, are objectified, or cut, through sociomaterial practices such as patenting.

Discussing the effects of a specific scientific innovation’s translation from novel object of knowledge to patented commodity, Strathern (1996:524) points out that “forty names to a scientific article became six names to a patent application; the rest did not join in. The long network of scientists that was formerly such an aid to knowledge becomes hastily cut. Ownership thereby curtails relations between persons; owners exclude those who do not belong.” Patents define – we could say “perform” – which

relations matter and which do not as an innovation moves from a laboratory to a wider sphere of application. Legally, they depend on the claim that certain human intervention into the material world has produced a marketable commodity. Scientific innovations entail such complex arrays of human labor that the determination of which actors' interventions should be acknowledged as mediators is necessarily non-democratic. Thus, proprietorship "cuts" networks, defining their limits.

Likewise, in the example of kinship systems, culturally specific knowledge practices determine which actors can become allies and which cannot. For example, Strathern (1996:530) details how English culture defines human-human relations as homogeneous and human-nonhuman relations as heterogeneous, an ontological distinction foreign to Melanesians with whom she has done ethnographic fieldwork. For Melanesians, ownership and kinship are "mixed," in that women may be purchased as goods and flows of commodities or trade-goods may further play direct – rather than indirect or "social" – roles in the constitution of kinship alliances. The Euroamerican system (or, more specifically, what Strathern terms "English bilateralism"), on the other hand, gives a kind of exceptional ontological status to the human body. The human body is a network that connects with other human bodies – flows of substance, sexual intercourse – in ways that are categorically distinct from how it relates with nonhuman actors or with other humans non-sexually. Here, Strathern's critique hints at the broader implications for recognizing network cuts as the necessary inverse of network extensions. Melanesians do not share the seemingly Cartesian English kinship system:

What is interesting about English bilateralism, then is that the basis on which everyone might say they are related (biological and genetic connexion) can be reckoned separately from the traffic of social relations. This gives us both continuities and discontinuities of identity. In so far as

biology and society are taken as distinct domains, we can see why the users of English culture presume an identity of interests in social relations and why they presume heterogeneity in mixes of human and nonhuman. In Melanesian terms I might want to say that these Euro-Americans imagine a boundary to the person that makes internal flows of substance radically different from external ones (interactions with others). That also gives a tenacity to their ideas about race and sexuality: continuities are somehow within and discontinuities somehow outside.

The realization that cutting the network is an important process for the delineation of collectives, including emergent racial and sexual identities, is crucial to the conceptual productivity of the “cut” concept for my study.

Before summarizing the implications of this concept for theorizing subject-formations, it is necessary to proceed into Barad’s sense to help flesh out the epistemological, ethical, and political implications of this concept. In her book, *Meeting the Universe Halfway: Quantum Physics and the Entanglement of Matter and Meaning* (2007), Barad radicalizes the physics-philosophy of Niels Bohr, aligning it with contemporary trends within science studies and feminist theory. Among other insightful and currently relevant arguments made by Bohr, Barad engages his conceptualization of the intrinsic indeterminacy of any boundary between an “object of observation” (i.e. the objects of physical knowledge, such as photons) and the “agencies of observation” (i.e. human physicists). Bohr and Barad both hold that the objects and agencies are mutually coproduced within the specific material assemblage of the laboratory. Different laboratories produce different physical objects and different scientists. Since, as Bohr showed, observations of physical systems have clear effects on their material dynamics, the question of how, or whether, to distinguish objects and agencies has special import for any understanding of the physical properties of quantum mechanical systems (construed inclusively to include systems at all scales).

Barad's solution to this problem invokes what Latour would call "mediators" that enact relations between otherwise independent entities. The specific types of mediators that most pervasively concern Barad are "measurement apparatuses." Barad's (2007:334). invocation of these apparatuses marks her divergence from Bohr: "I propose a crucial shift in understanding the nature of apparatuses: apparatuses are to be understood not as mere laboratory instruments, static instrumental embodiments of human concepts, but as open-ended and dynamic material-discursive practices through which specific 'concepts' and 'things' are articulated." The measurement of quantum physical processes recursively articulates the scientists who seem to – but do not – have complete control over the process of measurement itself. Any scientist who has had to attend to a broken apparatus, or open blackbox (Latour 1987), knows that laboratories are sites where multiple agencies swarm (see Pickering 1995 for examples from the history of elementary-particle physics). When mediators, or apparatuses, break down, nonhuman actors compel scientists to reaction, and the mutually collaborating agencies of the laboratory reveal themselves. Latour sometimes casts this paradox of science-in-action in a near-mystical light; we can often only identify how fact-production succeeds once it fails (hence, the "counterfactual" method).

So, what does the mutual coproduction, or what Barad calls "intra-action," of observers and agencies through the mediation of apparatuses have to do with the concept of "cuts" as an ethical modification or clarification of Latourian ontology? First, it draws what theoretical biologist and systems theorist Howard H. Pattee (2001:15) calls the "primeval epistemic cut" – the subject-object distinction – into the material world of the laboratory and shows how it emerges in practice, rather than exists as an a

*priori* disjuncture.<sup>22</sup> Where Strathern locates her critique of the subject-object cut in Melanesian ontologies, Barad brings us into the laboratory to make the same point, in some of the same vocabulary. Bohr knew already that “there is no inherently determinate Cartesian cut” (Barad 2007:118) but Barad takes us further down this path, revealing that the implications of this observation reverberate through conceptual problems extending beyond classical metaphysics and quantum physics. Namely, I am drawn to the concept of cutting as the process by which disparate (human and nonhuman; it makes no inherent difference now) *bodies* intervene in and thus materially translate, alter, or *mark*, each other:

Since different agential cuts materialize different phenomena – different marks on bodies – our intra-actions do not merely effect what we know and therefore demand an ethics of knowing; rather, our intra-actions contribute to the differential mattering of the world. *Objectivity means being accountable for marks on bodies, that is, the specific materializations in their different mattering.* We are responsible for the cuts that we help enact not because we do the choosing (neither do we escape responsibility because “we” are “chosen” by them), but because we are an agential part of the material becoming of the universe. Cuts are agentially enacted not by willful individuals but by the larger material arrangement of which “we” are a “part.” The cuts that we participate in enacting matter. Indeed, ethics cannot be about responding to the other as if the other is the radical outside to the self. Ethics is not a geometrical calculation; “others” are never very far from “us”; “they” and “we” are co-constituted and entangled through the very cuts “we” help to enact. Cuts cut “things” together and apart. Cuts are not enacted from the outside, nor are they ever enacted once and for all (Barad 2007:178-9, emphasis in original).

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<sup>22</sup> While they are overlapping, I should clarify that Pattee’s and Barad’s senses of “cuts” differ. Barad’s more liberal use of the concept implicitly refutes Pattee’s (2001:15) delimitation of cuts to the epistemological domain (construed, somehow, biologically), and his claim that “cuts” are “an epistemic necessity, not an ontological condition.” For Pattee “cuts” seem to pertain to how subjects can separate themselves sufficiently from objects to represent them externally (a formulation consistent with Barad’s and my usage). However, his concept of the subject-object distinction seems to have little to do with Kantian and Cartesian metaphysics or the conventional use of these terms in the social sciences, as Pattee claims that this epistemic cut emerged in the form of the genotype-phenotype interface (the reason that it is “primeval”). In the case of the observation of physical systems, Pattee resembles Barad in seeing this process as occurring through measuring devices, but diverges by negating the possibility that the agencies of observation are also being measured or cut in the process. It seems that the primacy of genotype-phenotype interrelations for this theoretical biologist inherently rejects Barad’s notion of intra-action and Latour’s flat ontology. In any case, this is a notable terminological convergence.

The twisted becomings and matterings of Barad's cut-concept (which may not be a cut-and-dry concept at all!) sound rather poststructural. But I am willing to accept this position as a useful radicalization of Bohr's physics-philosophy, particularly since it does not depend on recourse to any textualism or immaterial semiotics, and ultimately escapes any phenomenological insistence on the primacy of the human-world interface. Like Latour, Barad presents an irreducibly material (meta)physics, a feminist cousin of what is emerging as "object-oriented ontology" (Harman 2005, 2009). Cuts emerge out of the intra-actions of material systems, or networks, and their defining processual attribute is the performative inter-marking of participating actors. These performative markings include the delineation of matter into objects of scientific analysis as well as the marking – gendering, sexing, racing – of human collectives that may result in violent inter-group subordination.

As in Strathern's critique, aspects of this conceptualization are clearly already present in Latour's concepts of alliance, blackboxing, and mediation. However, this feminist, posthumanist sense of "cuts" adds value to both Latour's concept of the network and Strathern's critique.<sup>23</sup> Particularly, it points out the ontological isomorphism between the act of observing physical entities in laboratory (or laboratory-like, see Chapters 5 and 6) settings and the act of articulating new social categories. This is the reason that the concept of cuts may serve as a powerful source of common purpose for the anthropology of colonialism and empire and the anthropology of science. The

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<sup>23</sup> Barad's definition of posthumanism may differ somewhat from more conventional uses of the term (e.g. Hayles 1999), and therefore demands definition: "Posthumanism, in my account, can be understood as a thoroughgoing critical naturalism, an approach that understands humans as part of nature and practices of knowing as natural processes of engagement with and as part of the world. In particular, the acknowledgement that humans are part of nature entails the simultaneous recognition that our understanding of nature as that which is disclosed through scientific practices entails an appreciation of the fact that scientific practices are natural processes rather than external impositions on the natural world" (Barad 2007:331-2).

remaining question is whether the act of marking physical bodies in the laboratory and the racial, ethnic, and sexual markings of colonialism entail comparable ontological and ethical consequences.

While I am inclined to say that these two categories of cuts do not differ ontologically, broaching the problem from the perspective of ethics is less clear. A useful framing device may be Latour's (2004a, 2004b) concept of "cosmopolitics," or the project of articulating common, shared, peaceful worlds through association with heterogeneous others. Cosmopolitics must attend not just to the question of how our alliances are articulated, but also how we participate in making physical cuts, marks on bodies that perform humans and nonhumans as different, materially constituting them as something other. This is the negative, or violent, inverse of the alliance axiom, and a constitutional argument that subalternists have made against the very possibility of accurate, nonviolent cultural representation (Moreiras 2001; Spivak 1988; see Hayden 2005 and Chapter 3). It is a crucial component of my theoretical repertoire, and bears much in common with the argument among postcolonial theorists that all acts of representation entail "epistemic violence," a negation of the subjects that they attempt to represent (discussed further in Chapter 3). A primary space in the exercise of epistemic and symbolic violence is control over the legitimation of historical narratives. Which historical actors are allied and which are not in the production of Maya hieroglyphic knowledge?

Subaltern studies has as much to offer as science studies in addressing this problem. I turn to this topic in the following chapter, which further situates this conceptual framework within the terrain of Latin American historical knowledge

production. The question becomes, how do acts of historical interpretation perform cuts that mark certain subjects as belonging within categories such as “indigenous,” or “Maya,” and in what ways can scholars address the violence that attends the emergence of such “identity” categories out of the actor-networks that they co-perform?

### **Conclusions**

This chapter has progressed from a presentation of the basic ontology that grounds this study, Latour’s actor-network theory, to a consideration of publics as collectives that emerge out of scientific practices, and, finally, to a critique of the young field of postcolonial science studies. My objective has not been to review these three fields as straightforward conceptual programs that can be applied in this study but, rather, to explore their various convergences and divergences with an eye towards their utility for better understanding the kinds of historical knowledge practices that become my analytical objects, my “objects of observation,” in following chapters.

Further, this discussion of network formations and cuts inflects questions surrounding the role of publics in producing and legitimating scientific knowledges. Namely, through what material assemblages, through what spaces, and with what technologies are publics allied and cut? What are the consequences of including and excluding “lay” scientific actors in the production of scientific/historical knowledges? What kinds of violences take place as these publics act and react to scientific propositions? And how do the public consequences of knowledge production bear on the epistemological value of scientific/historical arguments?

As I proceed to subsequent components of the study, this postcolonial science studies perspective will ground my treatment of hieroglyphs as actors that have played significant roles in the assemblage of Maya culture. Particularly, it sets up

epistemological questions surrounding the value and validity of claims to hieroglyphic decipherment that rest on extricating, or cutting, Maya hieroglyphs from their varied sets of material associations. I position the act of cutting Maya hieroglyphs into texts as the central “issue,” or *ding* (Latour 2005a), of this study, emphasizing how it draws many actors into a coordinated set of material practices that result in reductive, ethically complex, and socially consequential articulations of the Maya.

## CHAPTER 3 THE INTERSECTIONS OF SCIENCE, POLITICS, AND HISTORY IN LATIN AMERICA

### **Introduction**

In the first two chapters of this study, I have periodically touched on how science studies scholars have conceptualized interconnections between science and politics. As I note, the bulk of pertinent science studies research has focused primarily on the interface between science and politics in the United States and Europe. In this chapter, I assess how scholars have understood intersections of science, politics, and history in Latin America (with primary attention to the late twentieth century and early twenty-first century). I conceive this consideration of the geographical pertinence of science studies in light of the argument presented in Chapter 2, that postcolonial science studies should attend to the modes of historical knowledge production complicit in and productive of contemporary forms of imperialism. Here, I help make the case that Latin American historical knowledge has conventionally produced complex cultural effects, including propagating “cuts” (Barad 2007; Strathern 1996; see Chapter 2) that mark bodies as indigenous or non-indigenous. In Chapter 4, I recombine these geographical and topical critiques of science studies with the background presented on Maya hieroglyphic studies to show how this field presents an opportune case to help attune science studies methodologies and metaphysics, such as Bruno Latour’s work, to the field of Latin American historical knowledge practices.

The following section reviews the existing application of science studies methods and concepts to Latin America. In limited scope, I examine work on the history of colonial and postcolonial Latin American sciences before moving to a more thorough review of anthropological and sociological studies of contemporary Latin American

sciences. Tracing the history of colonial and postcolonial science is important for two interconnected reasons: it provides a background for understanding the historical role of sciences in articulating Latin American racial-ethnic cuts; and it identifies the historical processes through which natural and social sciences have contributed to Latin American political formations. Further, in contrasting the relatively meager work on contemporary Latin American science with the relatively abundant work on historical, especially colonial, Latin American science, I hint at the problematic assumption among many Latin Americanist social scientists that sciences no longer factor in the exercise of imperialism.

By reading the contemporary Latin Americanist literature on science studies against the historical formation of sciences in Latin America, I suggest that the field of Latin Americanist postcolonial science studies should attend to contemporary human and social sciences in addition to biological sciences and technosciences. Indeed, by reviewing how perspectives on “public science” figure within this context, I emphasize that Latin American(ist) social sciences have played a very prominent role in constituting publics marked by consequential racial, ethnic, and historical narratives. This expansion of the field’s focus will require forms of engagement between the critical-activist (or “oppositional”) science studies orientations and critical Latin Americanist anthropologists addressing topics such as indigeneity, war, and neoliberalism. Thus, I conclude this chapter by cross-reading the tradition of politically engaged research pursued against the backdrop of the Guatemalan civil war with the critical-activist inclination of postcolonial science studies.

I argue that the political action of much Latin Americanist sociocultural anthropology can fruitfully couple with science studies approaches that attend to science as a form of politics, employ locally relevant concepts of science, and historically situate interpretive and ethical practices, such as “witnessing.” Reading the work of Steven Shapin and Simon Schaffer (1985; see also Shapin 1994) and Donna Haraway (1997) in tandem with that of Karen Barad (2007) discussed in Chapter 2, to “witness” is to become an “agent of observation” that apprehends worldly matters with the objective of cultivating shared understanding of how the world can be known (epistemological witnessing) or how we should live (ethical witnessing). Reference to “witnessing” among Latin Americanist ethnographers (esp. Manz 2004) has turned on the ethical sense of the term. I invoke science studies accounts to show how this ethical sense inevitably entails the epistemological sense as well, implicating a complex history of claims to scientific objectivity and moral certainty. By drawing together the Latin Americanists’ practice of ethical witnessing with the science studies scholars’ critique of the history of epistemological witnessing, I establish a contingent politically- and scientifically-engaged framework (a “cosmopolitics,” see Chapter 2) for evoking the past through practices that attend to their consequences, particularly the cuts that they perform. Subsequent chapters explore the ways in which this framework – an ethico-epistemological program for Latin Americanist postcolonial science studies – helps us reconceptualize the historical knowledge production entailed in Maya hieroglyphic decipherment and Maya studies scholars’ public engagement, processes that have prominently entailed visual practices of witnessing.

## Conceptualizing Latin American Sciences

While science studies is a relatively young field of inquiry with origins elsewhere, it has gradually cultivated a small following of Latin American and Latin Americanist scholars. Much of this research ties into a longer-standing concern with the operation of colonial and imperial rule through scientific methods and institutions. I begin this section with a brief detour into these histories, before turning back to more recent developments in Latin American science. I trace how historians have identified science as a set of projects that contributed to the oppressive colonial economy as well as the bureaucratic administration of the colonies. The heavily historiographical orientation of the history of Latin American science further calls attention to the necessity of addressing the shifting conditions that shape knowledge of the past. Recognizing the critique that English-language scholars have ignored the existing Castilian-language literature (Vázquez León 1996:113), I summarize themes presented by scholars working in both languages.<sup>24</sup> I suggest that the recurrent concern with public participation in scientific and technological projects voiced in Castilian-language publications could productively ally with questions about transnational science articulated in the English-language literature, in order to work towards forms of Latin American science studies that attend to interrelated questions of public engagement with science (PES) and the continuing consequences of scientific knowledge production for the articulation of racial and ethnic cuts.

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<sup>24</sup> By “Castilian” I mean what is colloquially termed “Spanish.” The former term evades the equation of a language and the state of Spain, which is marked by internal linguistic-political conflicts (particularly between speakers of Castilian and Catalan).

## Histories of Latin American Sciences

The existing history of colonial science literature on Latin America is sweeping and thorough compared to the modest work on contemporary Latin American sciences discussed in the following subsection.<sup>25</sup> Biological (primarily botanical) sciences represent a major focus of the literature on Latin American science and empire. However a significant body of work on the social sciences of the post-independence era also now exists. This subsection briefly reviews some of the more important contributions from the history of science literature. In so doing, I trace the historical transition between a colonial science which directly served the utilitarian economic interests of the Latin American elite to a “postcolonial” science emerging through eighteenth-century travel narratives that provided an empirical ground for universalist, historicist natural history. Thus, I affirm and further substantiate that the human sciences of the past two centuries have played a central role in the edifice of science (and, thus, politics) in Latin America, and, further, that “social science is not a bizarre and failed version of science but the quintessential modern scientific effort, a cardinal expression of what the nineteenth century meant by human knowledge; it is thus a yardstick to investigate science as a whole” (Tenorio Trillo 1999:1157). Thus, I argue that a culturally- and historically-attuned postcolonial science studies in Latin America

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<sup>25</sup> See, for example, the wide-ranging articles collected in *Nature and Empire: Science and the Colonial Enterprise*, a volume of *Osiris* guest-edited by Roy MacLeod (2000), as well as the heavily Latin America-focused edited volume, *Mundialización de la ciencia y cultura nacional* (1991). For an historiographical complement to MacLeod’s volume, see the essays collected by Londa Schiebinger (2005) as an *Isis* forum on *Colonial Science*. I should note that while the literature is substantial, in his contribution to this forum, historiographer Jorge Cañizares-Esguerra (2005) notes that the history of colonial science has focused heavily on sixteenth-century colonialism and the eighteenth-century Bourbon revival to the detriment of the seventeenth and nineteenth centuries. This asymmetry seems all the more stunning in light of how centrally Robert Boyle’s seventeenth-century laboratory experimentation has figured in science studies scholars’ arguments for the importance of understanding science as a cultural institution controlled by structures of gentlemanly witnessing and literary persuasion (Shapin 1994; Shapin and Schaffer 1985).

should attend directly and systematically to the politics of social scientific and historical knowledge production.

Setting aside any semantic dispute over the cultural range and historical relevance of “science” as a concept, the European colonists and Creole (American-born Spaniard) ruling class of the sixteenth and seventeenth centuries introduced and maintained European systems of knowledge appropriation, production, and dissemination as part of the colonial enterprise. Of course, the significant political power conferred on those who could understand and manipulate “nature” also led colonized subjects to adopt scientific methods and institutions for their own ends (or vitalize previously existing epistemic practices). In Latin America, this history is further complicated by the powerful Creole naturalists who opposed European antagonists’ assertions that American climes were unsuitable for human and animal health (Glick 1991:311-4). Since the initiation of the very “idea of Latin America” (Mignolo 2005), Latin American science has always served as a contested set of social technologies and practices:

Indeed for colonial scientists, science served as metaphor *and* means of legitimate colonial aspiration. Eventually, both colonizer and colonized came to believe that the promotion of science also promoted the cause of independence. For example, after losing the vast majority of her empire, Spain was not slow to sabotage local attempts to reform and modernize education and scientific institutions in such remaining colonies as Puerto Rico and Cuba. Without a doubt, on both sides of the colonial divide, science was seen to provide a mechanism for increased colonial autonomy and self-sufficiency (Chambers and Gillespie 2000:226).

The sciences of the Spanish colonial territory and subsequent independent Latin American states directly fed the interests of competing political-ethnic collectives. Thus, it should be unsurprising that the original Latin American colonial sciences figured within the Spanish project of extending its sovereignty to the New World by instituting a system of political governance.

In this light, historians of science have paid significant attention to the early botanical pursuits of the Spanish colonists and colonial elite. Cañizares-Esguerra (2005:65) usefully summarizes the means and motives of colonial actors who collected botanical specimens and knowledge to secure Spanish and Creole control of the economy of medical supplies: “metropolitan expeditions, the patriotic impulse of provincial clerical elites within the composite monarchy that was the empire, the private initiative of entrepreneurial settlers and merchants, and the coordinated regional, and even continental campaigns to gather information using the empire’s bureaucracies and the vast network of municipal authorities.” From the perspective of postcolonial science studies, among the most engaging structures of encounter predicated by these botanical pursuits involved the incorporation of indigenous knowledges into the European colonial medical institutions. For example, discussing the literature on the sixteenth century Mexican botanical expedition of Francisco Hernández, Cañizares-Esguerra (2005) calls attention to the prominent contributions of indigenous Nahua intellectuals trained by Franciscan missionaries in creating compendia of American plants. By the mid-sixteenth century, thus, such Nahua knowledges had become ratified contributions to the rising natural histories of the New World, a point that Cañizares-Esguerra (2005:66) employs to remind historians of science to “avoid reductive terms such as ‘European’ and ‘indigenous,’ both based on reified and dichotomous notions of identity.” Additionally, Cañizares-Esguerra proceeds to point out how both “indigenous” and Creole municipal authorities responded to mid-sixteenth century questionnaires circulated by the Crown pertaining to local natural resources, among other topics. Though supported by a weak bureaucracy, these *Relaciones*

*geográficas* (as they are collectively termed) probably represented the most pervasive attempt by the Spanish empire to achieve total knowledge of American natural history, and involved widespread participation across identitarian and linguistic divides.

In other words, the Spanish colonial sciences contemporaneous with (and partially productive of) what is now termed the “scientific revolution”<sup>26</sup> of the sixteenth and seventeenth centuries supported cultural and economic institutions more explicitly in the service of colonialism than did the sciences of the post-Independence period. In addition to the work of the Franciscan missionaries, Creole merchants played major roles in developing Spanish colonial science:

Settlers and merchants were always on the lookout for new natural resources to sell, constantly hyping the economic windfalls that would accrue to those capable of exploiting the various new mineral, pharmaceutical, and agricultural resources found in the Indies. They also sought to introduce new mechanical devices, demanding patents and monopolies. The Crown responded eagerly but cautiously to all these claims by farming out the testing of the new products and devices to experts back home: physicians, pilots, cosmographers, apothecaries, and inventors. By the early sixteenth century, the scale of claims and counterclaims was such that new institutions had to be created, including the Casa de Contratación in Seville, a veritable “Chamber of Knowledge” (Cañizares-Esguerra 2006:19).

Subsumed as a division of the Consejo de Indias, the Casa de Contratación served as the bureaucratic center for compilation and organization of cosmographic and demographic knowledge of the Spanish colonial world. As early in the colonial encounter as the mid-1520s, this largely statistical project required the participation of both encomenderos and local politicians in compiling requisite data, an extension of Spanish networks of control necessary to assimilate New Spain as a territory of the Spanish state (Bustamente 2000). Not simply an informational network, the nascently

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<sup>26</sup> See Steven Shapin’s (1996) *The Scientific Revolution* for a historiographical treatment of this concept.

extended state-form manifest in the colonial bureaucracy preconditioned and relied on the *encomienda* system's expropriation of capital (tribute) from largely indigenized subalterns.

By the eighteenth century, a new vehicle for scientific pursuits in the Spanish colonial New World was emerging: the scientific expedition. Individual actors within the Spanish empire certainly capitalized on the knowledge produced by these new "philosophical travelers" (Cañizares-Esguerra 2001). Yet, the expeditions were primarily comprised of French, Prussian, and British elites who sought to serve as a class of reputable *witnesses*, displacing the purportedly unreliable accounts of the Spanish. Discussing the resulting mid-eighteenth century travel narratives, Mary Louise Pratt (1992a:23) describes the expedition as "one of Europe's most conspicuous instruments of expansion," having utmost importance to the colonial project and the constitution of European collective identity:

In the second half of the eighteenth century, scientific exploration was to become a magnet for the energies and resources of intricate alliances of intellectual and commercial elites all over Europe. Equally important, scientific exploration was to become a focus of intense public interest, and a source of some of the most powerful ideational and ideological apparatuses through which European citizenries related themselves to other parts of the world.

The botanical and cartographic sciences funded by the political patrons of European explorers in the Americas did not serve as a direct instrument of Spanish control over the existing subaltern populations. Yet they did provide the forms of knowledge requisite for maintaining an extractive political economy that instituted forms of labor inequality.

By the eighteenth century, travel narratives also figured among the most important documents circulating in the project of creating an eighteenth-century European

collective self. Compilations of travel narratives became a highly valued source for the reading publics' understandings of the New World, largely displacing the accounts of the Spanish colonists. Analyzing this transition in his masterful *How to Write the History of the New World*, Cañizares-Esguerra (2001:22) builds on Lorraine Daston's (1988) distinction between "external" and "internal" techniques for evaluating the validity or reliability of narrative accounts. Eighteenth-century Northern Europeans adopted "internal" criteria for analyzing travel accounts, which emphasized the narratives' literary and rhetorical coherency, and displaced the "external" criteria that centered witnesses' social status and character, as in the Spanish colonial narratives.

Not only did this transition to "conjectural histories" serve to denigrate the Spanish from a Northern European perspective, it introduced a new epistemological foundation for the evaluation of Amerindian source materials, particularly images categorized as writing:

After conjectural histories maintaining that writing evolved gradually from primitive painting to alphabets began to appear in Europe in the late seventeenth and early eighteenth centuries, Amerindian scripts ceased to be valued as repositories of reliable accounts. Since the sixteenth century, Spanish scholars had characterized nonalphabetical scripts as merely the early stages in the gradual ascent of reason toward the mastery of the visual representation of speech. However, the novelty of the new conjectural histories of writing lay rather in presenting nonalphabetical scripts as untrustworthy. More important, too, conjectural histories deployed systems of writing to demonstrate the evolution of the human mental faculties. Amerindian documents were used to demonstrate the progress of the human spirit through various mental stages rather than as reliable records (Cañizares-Esguerra 2001:95-6).

This marks an expansion of Latin American science beyond its role as an instrument of extractive colonialism. The text-production oriented Latin American science of imperial travel offered early empirical fodder to the social evolutionist universalism that Dipesh Chakrabarty (2000) and Ranajit Guha (2002) challenge in their subalternist critiques of

historicism, the idea that Europe presents the culmination of cultural and political progress, an unattainable stage of development for the rest of the world. While the human status of the colonized American subjects had been a contentious topic in the early colonial period, most prominently in the Valladolid controversy,<sup>27</sup> this new appropriation of the image of the indigene served distinctively teleological ends. The conjectural historians did not pose the question in the binary terms of Las Casas and Sepúlveda (whether or not the colonized were human), but, rather, in terms of a spectrum: precisely how human (read: European) were they? While Spanish writers developed “patriotic epistemologies” that defended the character of sixteenth-century colonists and the coherence of their narratives, as well as the value of indigenous sources as historical documents (Cañizares-Esguerra 2001), the Enlightenment historiography clearly established the perduring framing devices.<sup>28</sup>

In the nineteenth century, the philosophical travelers continued to present visions of the Spanish American territories and newly independent states that contrasted directly with the imaginaries embraced by their Spanish American Creole and mestizo elite contemporaries. The influence of the Prussian travel writer Alexander von

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<sup>27</sup> The Valladolid controversy entailed a debate between the Dominicans Bartolomé de Las Casas and Juan Ginés de Sepúlveda concerning the legitimacy of the Spanish conquest and *encomienda* system. Whereas Las Casas argued that the colonized were humans equivalent in spirit to the Spaniards, Sepúlveda claimed the colonized to be inferior natural slaves (Losada 1970). While the debate had no clear victor, it inspired the *Legislación de Indias*, regulating the *encomienda* system’s labor practices and establishing some of the initial discursive parameters for modern international human rights law (Martin 1995).

<sup>28</sup> While Maya hieroglyph experts have not positioned their work within the historical context of this debate, decipherment (and especially the exhortations of decipherments’ advocates) can be read as a vindication of Spanish American perspectives opposing Enlightenment historicism, tinged with the twentieth century-politics of voice and history. Indeed, epigraphers’ active use of the sixteenth-century analysis of Maya writing included in the *Relación de las cosas de Yucatan*, a document conventionally attributed to friar Diego de Landa (1994[c. 1566]), fits well with this reading (see Restall and Chuchiak IV 2002 for a reconsideration of the authorship and provenance of the *Relación*).

Humboldt has served as a crux of debate in analyses of this historical field.<sup>29</sup> Pratt (1992a) positions Humboldt as a figure who deeply shaped the nineteenth-century Latin American bourgeoisie's vision of the American territory. On the other hand, Cañizares-Esguerra (2006:153-62) has argued against Pratt's interpretation, showing that the (central) Mexican elite embraced a vision of their nation-state as a populated urban topography, the roots of the distinction marking the indigene as a rural subject (see Farriss 1984; Lockhart 1992), rather than as an empty tropical landscape ripe for scientific study, the colonial image further propagated by Humboldt.

According to Cañizares-Esguerra's (2006:163-4) well-argued position, nineteenth-century Mexican intellectuals, such as the landscape painter José María Velasco, rendered an evolutionary narrative of the nation that embraced, rather than rejected, the indigenous and colonial past:

The nation, these intellectuals suggested, needed to stop running away from its indigenous and European roots. In their eyes, the nation was neither "Indian" nor "Spanish"; rather it belonged to a new historical stage. According to this discourse, *mestizaje* was a new (third) historical phase in an evolutionary teleology of progress. *Mestizaje* was a synonym for "modernity": a willingness to bring (and critically embrace) new forms of political organization, technologies, and economies into the fold of a community whose contours, as much as its landscapes, had been shaped by distinct historical stages and events.

While those who embraced *mestizaje*, or racial mixture, within the nineteenth century historicist framework remained skeptical of North American fears of racial miscegenation (Cañizares-Esguerra 2006; Tenorio Trillo 1996), the class of Mexican

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<sup>29</sup> See Nicolaas A. Rupke's (2005) work on Humboldt for a comprehensive treatment of the figure as the subject of continual historical re-imagining.

intellectuals also became deeply involved in collaborations with North American scholars.<sup>30</sup>

Indeed, as the social sciences became professionalized disciplines supported by academic institutions in the late nineteenth century, U.S. intellectuals came to exercise significant control over concepts such as “modernity,” “race,” “nation,” and “development” as they figured within Latin American state and national discourses (Tenorio Trillo 1999). Historian Mauricio Tenorio Trillo (1999) presents the emergence of the social sciences, considered a set of disciplines oriented towards the creation of effectively organized and administered territories, as the outcome of close collaborations among U.S. and Mexican scholars between the 1880s and the 1940s. In Mexico, a small group of educated elite, the *científicos*, were gaining strong political control. This control took shape through techniques of governance innovated by U.S. social scientists, including maps, statistics, and anthropological studies. The production of such knowledge was a deeply political act, an attempt to force Mexican society to escape its colonial, agricultural, racially-polarized roots and to “catch up to” the societal and national model presented by the U.S., in the imperial and teleological terms of the era (Tenorio Trillo 1999:1169).

By the 1930s, the *científicos*' political institutionalization of stagist Marxian thought after the 1910 Mexican Revolution – a positioning of the Mexican nation within a schema of unilinear societal development – had given way to a more conservative national discourse characterized by *mestizaje* and *indigenismo*. In this case, *mestizaje*

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<sup>30</sup> Further, through the nineteenth and twentieth centuries, gentlemanly philosophical travelers continued to explore Mesoamerica, and focused prominently on antiquarian concerns, providing literary material for these historical constructions (Aguirre 2004; Evans 2004; Harvey 2001). As I discuss in Chapter 4, some of the more influential Mayanists of the early and mid-twentieth century, such as Sylvanus Griswold Morley and Sir J. Eric S. Thompson, continued in the tradition of the philosophical travelers.

entailed the exaltation of cultural hybridity, and *indigenismo* involved the often-romantic glorification of the achievements of indigenous cultures (Hernández Castillo 2001; Klor de Alva 1995; Tenorio Trillo 1999). Such concepts became formalized and widely recognized through the work of scholars such as Manuel Gamio (1916), a student of pioneering U.S. anthropologist Franz Boas. The anthropological work of Gamio explicitly served the political purpose of integrating indigenous populations into the *mestizo* nation and thus erasing the colonial and racial distinctions that he perceived to prevent Mexico's national progress. But this collaboration was not one-sided: "Boas needed Gamio's anthropology to support the study of tradition and primitivism and to achieve a truly professional science, universal and cosmopolitan" (Tenorio Trillo 1999:1179). Thus, Boas and Gamio exemplify a collaboration across difference that helped define early social sciences. Whereas Boas had the institutional resources to claim that his work was purely academic, Gamio put his social scientific research specifically into the service of government policy.

After World War II the U.S. came to exercise greater political and economic influence throughout the world. The more historical and pragmatist orientation of scholars such as Boas began to give way to a style of positivist social science that "sought to banish uncertainty" provoked by the Great Depression, soviet Russia, Nazi Germany, and World War II (Tenorio Trillo 1999:1184). Throughout Latin America, banishing uncertainty entailed enforcing development projects rooted in social scientific ideologies that advocated the complete restructuring of "traditional" sociocultural orders, forcing rural and indigenized communities to shed their cultural, religious, and economic systems in favor of something that looked more urban, even more U.S. American

(Escobar 1995). Such imperial development projects found national support in Mexico and other Latin American states through the discourses of *indigenismo* and *mestizaje*.

Whereas the Latin American natural sciences and technologies of the post-World War era have had too little institutional support to shape public imaginings of bodies and environments in the ways that they have in Europe and North America (Vessuri 1987, 2003), the social sciences imbricated in racial discourses of *mestizaje* have had major impacts on both governmental policies and public perceptions. In states such as Mexico and Guatemala, the historicist discourse of *mestizaje* has had extraordinary significance for contemporary forms of governance, as well as cultural and racial identities. Clearly, *mestizaje* is the outcome of a progressivist historical imaginary that has relied on the forms of knowledge produced by social sciences since the nineteenth century. Despite the political dimensions of this knowledge production, science studies approaches to Latin American contexts have tended to shy away from the social and human sciences, a problem that I continue to address in the following subsection.

### **The Rise of Latin American Science and Technology Studies**

As I discuss in Chapter 2, since the 1990s, science studies scholars such as Donna Haraway (1997) have distinguished between “oppositional” research that seeks to trace the role of science in reproducing social inequalities and “mainstream” research that seeks merely to illustrate the cultural imbrication of science. It is, of course, the “oppositional” scholars who initially drew attention to this distinction, and continue to do so, as in Sandra Harding’s (2008) powerful recent characterization and critique of Latour (1993; 2004a), Ulrich Beck (1992[1986]; 1997; 1999), and the team of Helga Nowotny, Peter Scott, and Michael Gibbons (Gibbons et al. 1994; Nowotny et al. 2001) all as “Northern science studies” scholars who have largely failed to attend to feminist

and postcolonial critiques of science. Along similar lines, science (and technology) studies scholars have distinguished between research that emphasizes critique and research that strives towards instrumental, “applied” ends. Philosopher of science Steve Fuller (1993) terms these differing research programs “High Church” and “Low Church” orientations respectively. Critical, or High Church, STS (with the acronym generally signifying “Science and Technology Studies”) has sought to address traditional philosophical and sociological questions surrounding the epistemology and practice of science, whereas instrumental, or Low Church, STS (with the acronym generally signifying “Science, Technology, and Society”) has foregrounded science reform, policy, and activism. Notably, the increasing intercalation of scientific and political problems (such as environmental and genetically modified organism, or GMO, policies), especially in the European context, has resulted in significant increases in the interests and practices shared by the two orientations (Sismondo 2008). By remaining cognizant of the Low Church concerns voiced in the Castilian-language literature while also addressing epistemological problems, I advocate a further rapprochement for the analysis of Latin American sciences.

Neither the critical nor the instrumental variant of STS has traditionally had a strong presence in Latin American universities. In 2003, the journal *Technology in Society* addressed this situation in an issue dedicated to “Studies in Science, Technology, and Society (STS) North and South,” guest-edited by the Spanish scholar José A. López Cerezo and the Mexican scholar Carlos Verdadero. The issue effectively surveys the transformation of STS perspectives developed primarily in France, England, and the U.S. for contexts in Spain, Portugal, and Latin America. In the introduction to

their edited issue, López Cerezo and Verdadero (2003:157) attribute the absence of systematic work in this direction to three factors: social and political conditions which make aspects of the STS movements irrelevant, such as low literacy and non-democratic governance; poor infrastructure for maintaining scientific research and relevant interdisciplinary projects; and restrictions on the formation of academic networks. Given this backdrop, López Cerezo and Verdadero support the development of Latin American expressions of STS that achieve a highly practical value in opening science and technology to public understanding and participation, in the service of improving the social and economic conditions of an impoverished populace. I think that expanding the analytical focus of Latin Americanist science studies explicitly to the social sciences may help provide the field with a more substantial disciplinary form and intellectual tradition.

The two most relevant Castilian-language journals reflect this orientation in their attention to STS analyses of Latin American developments: *Redes*, published by the Argentinean Universidad Nacional de Quilmes; and *Revista Iberoamericana de Ciencia, Tecnología y Sociedad*, published by the Spanish Universidad de Salamanca. Tellingly, several recent articles published by Spanish researchers in *Revista Iberoamericana* have sought to address public participation in technoscientific policy and practice. José Antonio Méndez Sanz (2008) and Ana Cuevas (2008) work with different philosophical repertoires to deepen the possibilities for public participation in the formation of science policy. Recognizing the limited successes of facilitating public participation in such policy domains, Francisco Javier Gómez González and colleagues (2008) argue that public participation in science and technology has been located primarily on the scale of

policy- and program-creation that feeds into research and development. They present the extension of public participation in scientific and technological practices as the appropriate next step, and offer a practical discussion of methods to facilitate such engagement.<sup>31</sup>

In the electronic journal that later developed into *Revista Iberoamericana*, Argentinean scholars Carmelo Polino and colleagues (2003) more directly address the public understanding of science in Latin American nation-states, a necessary precondition for any serious effort to stage public participation. In this article, Polino and colleagues resemble López Cerezo and Verdadero in expressing caution concerning the relevance of science studies approaches to public understanding and engagement in Latin American contexts:

Public understanding of science and technology [as an academic field] in developed countries has placed emphasis on how society increasingly controls the development of science. In contrast, in the case of Latin American and Caribbean countries, with less mature democracies sometimes on the edge of collapse, the scene is different. Scientific and technological activity is neither socially institutionalized nor considered a resource for growth. That is why, for the region's countries, it is still premature to consider citizens' direct participation in decisions concerning risk, uncertainty, or definitions of the pertinence and utility of science, though this limitation is simultaneously an incentive and an obstacle (Polino et al. 2003:part 2; my translation).

Addressing this challenge, Polino and colleagues (2003) suggest that inquiry into public understanding of science should help enact the sciences' "new social contract," a program devised at the 1999 UNESCO world conference (see Gibbons 1999) as an affirmation of the increasing necessity to institute political forms that foreground the interrelations of science and society, to guarantee public access to scientific knowledge,

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<sup>31</sup> As I discuss (especially) in Chapter 6, the history of Maya linguistics and epigraphy workshops may offer a model for such engagement.

and to value local forms of knowledge production. Establishing this perspective subsequently leads Polino and colleagues (2003) to follow Brian Wynne (1995) and other students-cum-critics of the “public understanding of science” movement in arguing against a conceptualization of the public as comprised of subjects ultimately incapable of understanding the nuance, complexity, and uncertainty of scientific practice and knowledge, a position often called the “public deficit model” (Marris et al. 2001; Wynne 2005; discussed further below).

Thus, we can see that one rising trend in Castilian-language Latin Americanist science studies research focuses on the possible utility of examining the science-public interface, with necessary attention to the precariousness characteristic of Latin American democracies. Since “public understanding of science” and “public engagement with science” are intellectual approaches developed within European contexts, where there is significant public discourse surrounding technological interventions into life (GMOs, cloning, etc.), it becomes very important to question the relevance of this research program to Latin America. In the following sub-subsection, I follow Polino and colleagues (2003) in addressing science’s lack of social institutionalization as both “an incentive and an obstacle” (see above).

### **The Relevance of “Public Science” Research to Latin America**

While Latin America has not traditionally hosted the kinds of “high-tech” sciences that concern many European sociologists of science, the history of science literature clearly demonstrates the significance of social, historical, and natural knowledges in constituting Latin American states and nations. The sociomaterial cuts enacted by these forms of knowledge lead me to question the historical, cultural, and political conditions through which elite sectors of the population, as well as U.S. and European

citizens, have achieved control over Latin American knowledge economies. Taking the argument of Polino and colleagues (2003) as a starting point, I suggest that an ideological form categorizable as a “public deficit model” has increasingly controlled both the distribution of knowledge and access to institutions of knowledge production in Latin America.

While it remains a topic of some debate within the European sociology of science community, I follow Wynne (2005) in defending the existence of social/scientific forms that we can grasp systematically as a discourse of “public deficit,” or scientists’ characterization of publics as incapable of understanding science. Wynne presents this rhetorical distantiating of publics from sciences as both an institutionalized habit and a method for deferring attention from contemporary scientific inquiries’ simultaneous and necessarily contradictory invocations of complexity and reduction. As Wynne (2005:70-71) puts it, the “public deficit model,”

involves promulgating the myth that publics are incapable of living with the provisionality of scientific knowledge (when the evidence, e.g. Marris et al., 2001, suggests that typical publics routinely accept endemic lack of predictive control); therefore, so this myth continues, because those publics mistakenly expect certainty and zero-risk from science, science as public knowledge is obliged to delete any open reference to unpredictability and unknowns – only instead to (known and controllable) uncertainties and risks (Wynne, 2001).<sup>32</sup>

As part of his response to public deficit approaches, Wynne advocates the concept of “public science” as the key critical term for analyses of “public engagement with science,” a successor project for the “public understanding of science” movement. By “public science,” Wynne (2005) means institutions charged with creating knowledge that

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<sup>32</sup> In turning to arguments grounded in specialized linguistic knowledge Mayanist epigraphers of the past decade have increasingly framed their public engagement through rhetorical and intersubjective strategies characteristic of public deficit models (e.g. Houston 2000:141).

depends on scientists' imaginings of the knowledge's public uses or consequences as it is produced (see Chapter 2).

Wynne's (e.g. 2005) contributions to the emerging science studies literature on how "the public" or "publics" participate in science itself reveal that even in the "complexity"-based sciences such of genomics and post-genomics,<sup>33</sup> engagement with publics may still be governed by a strong Enlightenment distinction between science, the general public, and the state.<sup>34</sup> Rather than considering scientific engagement with publics a secondary process, external to the real work of producing knowledge, Wynne illustrates the pervasive presence of an imagining of the sciences' public effects and consumers *within* the epistemic culture of post-genomics research. Wynne shows how post-genomics science's recent incorporation of a discourse of "complexity" has been attended throughout with a corresponding, and contradictory, public discourse of "simplicity," strategically employed to control popular understanding of scientific unknowns and unpredictability. In addition to public control, he sees this reductionism as a method of institutional legitimation, in its prominent framing of genomics research within a standard Enlightenment idiom of rationality (deferring or cutting publics' capacities to speak back to the sciences that govern understandings of bodies, foods, and environments). In Wynne's (2001, 2005) account, genomics scientists' public

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<sup>33</sup> Post-genomics is characteristic of sciences defined by "complexity," in that post-genomics researchers attempt to articulate linkages among actors operating through ostensibly different scales, modalities, or materialities (e.g. the "complex," systemic interrelations of genes, genomes, organisms, and environments). Thus, "complexity" here entails a sense of how objects of ostensibly different ontological – or, to be more accurate, "ontic" – status may enter into relations. See Wynne's article (2005) on post-genomics as well as John Law and Anne-Marie Mol's (2002) edited volume *Complexities: Social Studies of Knowledge Practices* for more complete elaborations of "complexity" as an analytical device.

<sup>34</sup> Important Anglophone contributions to the emerging literature on the science-public-politics interfaces include: Massimiano Bucchi and Federico Neresini (2008), Steven Epstein (1996), Alan Irwin (2001), Alan Irwin and Mike Michael (2003), Alan Irwin and Brian Wynne (1996) Sheila Jasanoff (2004), Bruno Latour (2005a), Mike Michael (1998), Mike Michael and Nik Brown (2005), Helga Nowotny, Peter Scott and Michael Gibbons (2001), and Steven Shapin (1990). See also Chapter 2.

discourse reduced the newly conceptualized unpredictability of gene-genome-organism-environment systems to controllable risks in order to assuage European public fears motivating controversy over GMOs. Thus, the sciences' increasing control over public definitions of citizenship, health, and – in the case of this study – history raises serious ethical and moral issues of responsibility and accountability. The key point that I draw from Wynne's and others' work on the public engagement with science is that scientists' concepts of disciplinary self and significance rely not solely on cultivating communities of witnesses (Shapin and Schaffer 1985) but also on imagining public worlds shaped, even constituted, by scientific knowledge practices (see Chapter 2). These imaginings have real political consequences.

To employ such a perspective on the mutual constitution of scientific knowledges and scientific publics from the perspective of Latin American science studies, we will have to address at least two major issues pertinent to scholars such as López Cerezo and Verdadero (2003) and Polino and colleagues (2003). Namely, what kinds of sciences exist in Latin America, and through what existing political channels do Latin American sciences engage publics? Clearly, the kinds of publics constituted through scientific research depend on the kind of scientific research itself. Indeed, because of the crucial importance of racial, ethnic, and geographical distinctions institutionalized by Latin American social sciences, these sciences seem more public in their societal effects. Further, speaking to the kinds of sciences that wield influence in Latin America and the manner in which they wield it involves specifying the political and economic conditions that have cultivated some sciences at the expense of others, a question that I address throughout this chapter.

The overarching conditions that have shaped the terrain of contemporary science in Latin America derive from a geopolitical field wherein Latin America has historically served as a politically marginalized resource provider for the economic centers in Europe and the U.S. Thus, the transformations of political economy, and therefore science, that have marked Europe and the U.S. have also affected Latin America. Perhaps the most obvious recent political economic and ideological transformation that has shaped the terrain of Latin American science is the end of the Cold War. In Central America, the end of the Cold War entailed the decline of socialist movements in Nicaragua, Guatemala, and El Salvador, and the rise of neoliberal reforms and associated oppositional movements based explicitly on ethnic and indigenous identities (Jackson and Warren 2005), crafted in part through mobilizations and appropriations of social scientific categories. As noted above, Latin Americanist science studies scholars such as López Cerezo and Verdadero (2003) and Polino and colleagues (2003) have called attention to the instability of “democratic” governance in post-Cold War Latin America, leading to skepticism surrounding the possibility (or necessity) of democratizing the comparatively weak sciences. So, what tools does science studies offer for conceptualizing the rise of post-Cold War science?

To address this question and consider the relevance of science studies analysis to Latin America, it is important to locate how scholars have justified the value of such analysis in conventional European contexts. Referring primarily to Europe, Mark Elam and Margareta Bertillson (2003:239) have theorized the changing relation between science, society, and the state in these terms:

In the Cold War era, science living relatively apart and invisible to society could be understood as necessary for the overall protection of society, but

not so in the new era. With the passing of bipolarity, the Enemy has been largely replaced by Competitors and an expanding landscape of sizeable opportunities and emergent risks that science must now be clearly seen to be addressing and acting upon.

Elam and Bertillon (2003) conceive this general post-Cold War shift to the democratic governance of science (“science’s new social contract”) and refashioning of citizenship in scientific terms as a substantial divergence from Enlightenment ideals, in which Science was held to serve the interests of the state, and received little or no direct feedback from the general public. Building on the work of Steven Shapin (1990), Elam and Bertillon describe how the European public could not directly address science until the end of the Cold War and the decline of the European Welfare State. While science might have occasionally circumvented the state to speak directly to the public, public concerns with science had to be sifted through the correct government screens. After the Cold War, this “Enlightenment model” declined and science became directly *accountable* to citizens, acting on and addressing clearly articulated public problems.

I am not yet convinced that the lives of marginalized Latin American subjects are characterized by “biological citizenship” (Rose and Novas 2005), or a subject- and body-constituting sense of collective belonging enabled by the dispersed, micropolitical operations of the medical and biological sciences. Yet, it seems clear that often-biologized categories of identity such as “indigenous” have become more central in political movements since the end of the Cold War. In other words, through development and tourism programs, Latin American states such as Guatemala increasingly employ what Aihwa Ong (2006:87-8) calls graduated sovereignty and citizenship: “Graduated sovereignty...refers to the differential treatment of populations in relation to ethno-racial differences, and the dictates of development programs.

Segments of the population are differently disciplined and given different privileges and protections in relation to their varying participation in globalized market activities.” In sites of cultural production such as tourist markets and linguistic nongovernmental organizations (see Chapter 6), discourses of history play major roles in Latin American articulations of graduated citizenship.

Such graduated forms of contemporary population management create complex ethical problems. In anthropologist Michael M.J. Fischer’s (2007b:540) view, the post-Cold War rise of a multicultural world defined by biomedical, ecological, technological, and informational systems unbound and unregulated by existing political forms has introduced “unprecedented ethical dilemmas.” Rather than framing the response to such ethical dilemmas in the policy-oriented terms of Elam and Bertillon’s varieties of democracy, Fischer advocates the innovation of open and reflexive social institutions as democratically responsive processes that respond to uncertainty by facilitating collective decision-making. Such institutions may serve as “on the ground” manifestations of science’s democratization. They seem to exemplify the sciences’ “new social contract,” or what science studies scholars have called “Mode 2” knowledge production (Gibbons 1999; Nowotny et al. 2001). Based on this new, post-Cold War social contract that depends less on the state and creates a greater contiguity of science and society, such knowledge production is transdisciplinary and publicly oriented, as opposed to the closed, authoritarian “Mode 1” European and North American science of the Cold War era. Such open institutions would require an understanding of the public neither as a secondary audience for scientific research nor as a symmetrical but separate participant in the process, but as an integral and heterogeneous component of all research stages

(see Chapter 2). In Chapters 5 and 6, I question the degree to which workshops on Maya hieroglyphic writing conform to such conceptualizations of open, democratic institutions, with their own modes of constituting publics.

While the contributions of Elam and Bertillon (2003) and Fischer (2007b) indicate real trends within the recent attempt to democratize European (and to a lesser extent, North American) sciences, they clearly do not apply in straightforward ways to Latin American contexts where the discursive, political, and economic antagonisms of the Cold War did not play out as directly through competing scientific and technological programs. Thus, in Cold War-era Latin America, the public perception of science did not hinge on nation-constituting governmental performances that demonstrated the state's control over expertise and technologies that could protect society from external threats, as in the case of the "radioactive nation building" (Masco 2006) of nuclear weapons programs. Assessed in very general scope, during the Cold War, Latin American discursive constructions of natural science and technology seem to have been largely determined by necessary political and economic alliances with these major Cold War state actors (unlike public constructions of social and historical sciences). Clearly, these alliances took simultaneously economic and political forms. Therefore, it seems likely that Latin American public perception of natural science and technology during the Cold War was mediated less by states than by the forms of economic imperialism that constructed Latin America as a vast territory of resources – including human resources – for extraction by Europe and (especially) the United States.

Thus, it is not surprising that in his literary critical analysis of the role of science and technology in Latin American narrative, Jerry Hoeg (2000) calls attention to the

tendency of Latin American authors to identify science and technology with colonialism and the economic inequalities instituted by transnational corporations.<sup>35</sup> Hoeg locates this critique in several prominent Latin American novels, including Gabriel García Márquez's (1971[1967]) *One Hundred Years of Solitude*. In García Márquez's acclaimed novel, local, natural magic ultimately triumphs over the rationalist profiteering of the bearer of technology, the banana corporation that the author based on the United Fruit Company. In his rather unconventional reading, Hoeg (2000:32) goes as far as to deem *One Hundred Years of Solitude* "a kind of historiography of scientific 'progress' in Latin America, [in which] technology plays a key role." Hoeg sees this same negative position reiterated in the critical literature on Latin American science, including the important anthropological study by Jacqueline Fortes and Larissa Adler Lomnitz (1994), *Becoming a Scientist in Mexico: The Challenge of Creating a Scientific Community in an Underdeveloped Country* (originally published in 1991 as *La formación del científico en México: Adquiriendo una nueva identidad*, a different title with somewhat different framing devices [see Vázquez León 1996]).

In his reading, Hoeg warns against a naïve antipathy towards the cultural and political value of science and technology in Latin America, cautioning against adopting a "return to Eden" that reproduces classic colonial tropes, as in Humboldt's narratives discussed above. Indeed, Hoeg reminds readers that "science" itself is not a simplistically European invention that colonialism foisted upon a pure and empty

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<sup>35</sup> Hoeg's work fits within a body of critical analyses of technology in Latin American literature (e.g. Brown 2007; Hoeg and Larsen 2006; Robinett 1994). While informed by the posthumanist critiques of technology voiced by authors such as Haraway (1997) and N. Katherine Hayles (1999), these authors have not engaged directly with Latin Americanist sociologists and anthropologists of science (and vice versa). Thus, in a very limited way, here I am advocating a dialogue that takes the critique of technology as a companion project helping to open spaces for further integration of Castilian-language and English-language science studies themes.

territory.<sup>36</sup> In the conclusion to his chapter on “The Technological Other,” Hoeg frames this point as a powerful rebuke of Fortes and Lomnitz’s positions:

This chapter began with Fortes and Lomnitz’s assertion (1994) that in Latin America “science did not result from internal development; it is an imported cultural product.” This assertion simply perpetuates the myth of idealized origins, of the way it never was. The truth of the matter is, from the Inca, Maya, and Aztec empires to Perón and Castro, domination in Latin America has always been achieved through science and technology. The challenge is not one of “Creating a scientific community in an underdeveloped country,” but rather one of creating a nondominating scientific community, of creating a technocounterculture. It is incumbent on writers, theorists, artists, activists, scholars, and critics alike to produce a viable vision of this type of community, one of democratic technologies of empowerment rather than colonizing, monopolistic technologies of domination. Constructing imaginary pasts that never were is part and parcel of the dominating discourse of science.

Interrogating the forms and consequences of scientific knowledges and technologies without succumbing to colonial discursive constructions that presume *a priori* either the necessarily external “origins” of Latin American sciences or the natural, edenic purity of the colony itself could serve well as a central methodological imperative of Latin Americanist postcolonial science studies. Of course, we must also attend to whether potential “technologies of empowerment” prove complicit with the public deficit models that very much define antidemocratic control over many fields of knowledge in Latin America today. In other words, should we really innovate alternative “technocountercultures” or should we look to the existing scientific institutions as sites of cultural politics that already stage internal democratic and antidemocratic structures of

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<sup>36</sup> James Maffie (2002, 2003) makes similar points about philosophy in his excellent studies of colonial Nahua epistemology. There is also a growing body of ethnographic literature on American indigenous groups’ ontologies and epistemologies that details important contrasts between Euroamerican and American indigenous philosophies without resorting to (and, indeed, exposing the limitations of) “relativist” epistemological pluralism. I am thinking particularly of Eduardo Viveiros de Castro’s (1998; 2004) expositions of the Amazonian indigenous understanding of an underlying singularity of human spirit or “culture” hidden by a multiplicity of objects or “natures.” Such cosmological frameworks clearly have significant distribution throughout the Americas.

knowledge production, perhaps assemblages characterized by “friction,” meaning “the awkward, unequal, unstable, and creative qualities of interconnection across difference” (Tsing 2005:4)?

Would a “technology of empowerment” look like Fischer’s open, reflexive social institutions or other instantiations of Mode 2 science, and is this politically and ethically sufficient? This is an important question, to which I return, most centrally in my discussion of Maya hieroglyphic workshops (Chapters 5-6). First, I must turn to the existing Anglophone science studies literature to question how this body of research contributes to the problem of improving public engagement with science in Latin America, and locating these sciences’ forms of imperial complicity. In the process, I begin to highlight the types of contemporary sciences that have found themselves beneath the Latin Americanist science studies lens.

### **Transnational Science (Studies) and Democratization**

In recent years, a number of (especially U.S.) anthropologists working on Latin American issues have contributed to science studies research on the science-politics interface. By the 1990s, the critique of technoscience among cultural studies scholars such as Haraway had influenced a few Latin American ethnographers, notably Mayanist Diane Nelson (in addition to Latin Americanist literary critics such as Hoeg, discussed above). By the 2000s, anthropologists and sociologists of science increasingly pursued research outside the laboratory. Especially because Latin America, as I discussed in the preceding subsection, has long served as a scientific field site (and one not coincidentally troped as a laboratory), an increasing number of science critics have turned their attention to the conditions of knowledge production in this area. Here, I review several works that have sought to contribute directly to postcolonial science

studies (Hayden 2003, 2005; Redfield 2000, 2002).<sup>37</sup> Cori Hayden's and Peter Redfield's works represent significant contributions to the post-laboratory studies dispersion of the field. I seek to coax the Castilian-language literature's emphasis on public engagement and participation into productive tension with the English-language literature's emphasis on the latent and manifest forms of imperialism in transnational scientific research.

Hayden and Redfield have explicitly presented anthropological approaches to Latin American sciences within the conceptual rubric of postcolonial science studies. Both scholars are concerned primarily with the forms of science that have emerged through transnational (or trans-spatial) circuits of knowledge or commodity production. Further, both scholars have used approaches from science studies to grasp fields of science in Latin America that occupy science studies scholars' attention around the world. In Hayden's case, the science is bioprospecting (in Mexico) for the purpose of producing pharmaceuticals. Hayden follows science and empire scholars (discussed above) in examining the operation of colonialism and imperialism through biological – specifically botanical – science. Redfield, in turn, has attended to the use of French

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<sup>37</sup> Though I do not discuss their work directly in this subsection, I should also acknowledge the significant research of anthropologists and anthropology-oriented scholars who have contributed to critical analyses of scientific knowledge in Latin America outside the explicit space of postcolonial science studies, particularly Arturo Escobar (1998; 2008), Diane Nelson (1999; 2006; 2009), Hugh Raffles (2002b), and Luis Vázquez León (2003). Latour (1999a) has also published an important ethnographic essay on technologies and practices of reference employed in the production of Amazonian botanical knowledge during a research expedition. Unfortunately, Latour overtly extracts colonialism from science in this essay: "I will not deal at length with the politics surrounding this expedition, since in this chapter I wish to concentrate on scientific reference as a philosopher, not on its 'context' as a sociologist. (I apologize in advance to the reader, because I am going to omit many aspects of this field trip that pertain to the colonial situation)." Latour presents this delimitation as necessary to his project of reworking the concepts of reference and context. While I tend to read Latour's work as a kind of companion project, I see this delimitation as an ethically, politically, and epistemologically debilitating elision, itself an act of cutting the network (Strathern 1996) that risks complicity in its refusal to attend to the violences that have made Amazonian botanical research possible in the first place (for a discussion of this deferral, see Mallavarapu and Prasad 2006:192-3). As Mallavarapu and Prasad note, however, Latour does demonstrate more direct awareness of this deferral here than in other work (see also Harding 2008).

Guiana as a staging ground for the French government's efforts to launch commercial satellites.

Hayden's work on bioprospecting (her 2003 book *When Nature Goes Public: The Making and Unmaking of Bioprospecting in Mexico* and her 2005 article "Bioprospecting's Representational Dilemma") merits prominent attention in my project primarily because it addresses the use of Mexican "indigenous knowledge" in transnational science, as well as how sciences constitute publics. Further, Hayden attends to these problems through a framework that integrates analyses of the possibilities of and labor entailed in "representation" (in aesthetic, political, and scientific senses) presented in science studies and postcolonial studies literatures. In *When Nature Goes Public*, Hayden presents a rounded empirical and conceptual inquiry into a system of Mexican-U.S. pharmaceutical knowledge production. Indeed, in line with my study, Hayden traces a kind of actor-network or system of linkages among U.S.-based pharmaceutical companies, Mexican university chemistry laboratories, Highland Chiapas NGOs, and roadsides subjected to the scouring attention of scientists evaluating plant diversity. Hayden pays careful attention to the circulation and transformation of nonhuman entities, adding to the substantial body of literature within science studies on the emergence and transformation of scientific objects and, thus, objectivity (e.g. Callon 1986; Daston and Galison 2007; Fujimora 1992; Latour 1999d; Law 2002; Rheinberger 1997). She concentrates in one chapter, for example, on how scientists assay brine shrimp in chemistry laboratories, transforming them from crustaceans into cancer medicines. Such a focus contributes to rising attentiveness to transspecies ethics among ethnographers working on the "anthropology of life," a

rethinking of the category of the “human” in relation to its others (Franklin et al. 2000; Haraway 2003, 2008; Kohn 2007; Raffles 2002a). Throughout, Hayden questions how different stages in the network of material translations or transformations that define drug development recognize and elide the forms of “indigenous knowledge” that have become embedded in the plants and drugs themselves.

Thus, in her own words, Hayden (2005:186-7) is “interested in the work – simultaneously biochemical and political – that ‘local’ or ‘ethnobotanical’ knowledge is expected or asked to do as it travels into drug discovery circuits, and, ostensibly, back out again in the form of benefits-to-be-shared.” In locating this simultaneously biochemical and political work, Hayden asks how the repertoire of representational concerns entailed in actor-network approaches to science studies (where science is envisioned as a practice of producing complex networks of allied humans and nonhumans; see Chapter 2) conform with the representational concerns of postcolonial studies (where scholars such as Gayatri Chakravorty Spivak emphasize the complicity of representational(ist) knowledge practices in the marginalization of the subaltern). Indeed, her careful integration of the same bodies of literature to which I constantly return merits quotation at length:

We might say, then, that actor-network theory shades towards the positivities if not the positivist dimensions of knowledge production as an act of representation, while much postcolonial studies in South Asia and Latin America (specifically, the work of subaltern studies scholars) asks us to attend to its negativities – its violences and elisions. As such, ANT and the kind of postcolonial/subaltern studies developed by Spivak [1999], [Alberto] Moreiras [2001], and others seem implicated in two fundamentally distinct, if not opposing, ways of thinking about the political/social/epistemological work that knowledge does. ‘It’ (knowledge) includes, and shores up; it elides, and does violence. Where and how might these divergent theoretical perspectives take us with regard to bioprospecting?

One route would certainly be to address the historiographic and epistemological questions about the effacements enacted through the 'translation' of traditional knowledge into pharmacological value.... But we could also take our inquiries into bioprospecting's representational project in some other directions. We could think about questions of representation here in the dual mode flagged by actor-network theory – the depictive doubling as the political, in an 'affirmative' and indeed incredibly explicit sense. Here I think of the ways in which engaged ethnobotanists and plant chemists have long used their field studies and laboratories precisely like courtrooms; that is as staging grounds for proving the veracity of 'indigenous knowledge'. In this sense, ethnobotany has been figured by some activist practitioners as a form of what I call 'epistemological advocacy' – a commitment to translate 'indigenous knowledge' into the language of biochemical efficacy, and to use these assertions of the scientific rationality of indigenous knowledge as an axis of political mobilization and even court-room defense (Hayden 2005:188).

In "Bioprospecting's Representational Dilemma," Hayden pursues this legalistic positivities-oriented approach to biochemical knowledge production to its limits, before finding that the network itself constructs necessary sorts of exclusions and elisions – Strathern's (1996) sense of "cuts." In this case, allied research institutions in the U.S. and Mexico intentionally strip plant extracts of any identifying information that might allow the companies that use these extracts to know precisely whose "local" or "indigenous" knowledges contributed to the initial scientific identification of the plant as a useful biochemical agent. This negative work of rendering the plants anonymous serves to prevent corporations from evading their agreements with the original bioprospecting researchers by pursuing or producing the chemical compounds elsewhere.

Hayden (2005:196) calls this system a "technics of counter-representation" in which the liberal rights and material compensation of knowledge "providers," such as indigenous informants, can only be protected through a representational deferral, a negation of their constitutive contributions to the system. In other words, Hayden

follows a very Latourian methodological path, yet ultimately finds herself in a network characterized by uneven, contradictory, and ethically complex internal elisions, rather than the wildly rhizomatic or stiffly structural (Latour 1999a) expansions characteristic of more conventional ANT analyses. This network looks like what a subalternist might expect.<sup>38</sup> Hayden presents how the protection of property rights and compensation requires researchers to cut the network. Of course, the political radicalism of Hayden's own project is also limited or cut here, as she is prevented access to the local knowledge producers. As Spivak (1988) might anticipate, Hayden's capacity as a political/epistemological spokesperson for the subjects of local knowledge gives way beneath an logic that seeks to protect subaltern subjects' financial compensation by failing to represent, or identify, them, because their identification would enable corporations to pursue the botanical materials without their mediation.

I question whether the representational failure entailed in the pharmaceutical technics of counter-representation constitutes the original knowledge providers' subaltern subject positions. In other words, is it viable to assume that the subjects providing local knowledge who have been cut from the network in Strathern's (1996) sense have also been subjected to the performative epistemic violence of categorization, in Barad's (2007) sense of being "cut?" Answering affirmatively supports

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<sup>38</sup> While I am following through with Hayden's reading of the relevance of ANT and postcolonialism for bioprospecting, I do not think that, besides Latour, the innovators of ANT have only portrayed network-formation in a positivistic-oriented expansionist rhetoric. As a counterexample, consider John Law's (2002:99) poststructuralist reading of Latourian "translation:" "Translation: the pun works best in the Romance languages: *traduttore-tradittore*, *traduction-trahison*, to translate is to betray. To translate is to connect, to move to shift from one place, one modality, one form, to another while retaining something. Only something. Not everything. While therefore losing something. Betraying whatever is not carried over." Law clearly emphasizes the negativities of network-formation/material semiotics/translation/association/rhizomatics, though he does not attend clearly to how such negativities enact epistemic and material violences. Of course, this is a substantial elision. The attention to violence is what makes postcolonial readings of the poststructuralist literature an absolutely necessary corrective to the latter (see Chapter 2).

Spivak's (1988) position that the subaltern cannot be represented. Yet it seems that this negative position depends on the existence of a body of historical and contemporary documents, a kind of "positive" knowledge that testifies to the violence inherent in colonial encounters and subsequently emergent political economic systems and representational forms. In other words, the very possibility of locating a technics of counter-representation that performs epistemic violences relies on some presumption that scholars can represent the subaltern in some limited capacity (setting aside questions of such representations' complicities).

Hayden's political sympathies clearly reside with the oppositional approaches to science studies that seek to expose such violences of knowledge production. Yet, in this case, we have no apparent technologies of vision or voice to enable the researcher to see, let alone listen to, the subaltern. Thus, in the case of bioprospecting, is there no alternative to Hoeg's (2000) advocacy for a "technocounterculture?" Perhaps, though I fear that this hybridizing appropriation is a bit too likely to take the cyborg, that "illegitimate offspring of militarism and patriarchal capitalism" (Haraway 1985:68) as its figurehead. Today it seems that such advocacy may achieve greater returns through an irreducible localism, a commitment to "attachment sites" that cultivate subjects who recognize that "[t]he point is not to celebrate complexity but to become worldly and to respond" (Haraway 2008:41). As Wynne's (2005) work makes clear, exalting the non-hierarchical, rhizomatic qualities of "complexity"-based sciences (and other forms of cultural production) is no effective substitute for a neighborly and responsive care for how specific knowledges serves local, pragmatic ends. In this kind of neoliberal science, the only viable "oppositional" stance may be to ensure that activist

ethnographers or other types of political representatives become “enrolled” (Callon 1986) participants themselves. Perhaps such participation can produce forms of representation and worldly attachment that chart alternatives to the public/private distinction, and preserve marginalized subjects’ access to locally produced forms of the “common” as Michael Hardt (2009; see Hardt and Negri 2009) advocates.<sup>39</sup>

This problem brings us back to the questions of public participation and engagement foregrounded in the Castilian-language literature discussed above. In the case of contemporary botanical sciences, the problem is not a clear-cut public deficit model that constructs local subjects as ignorant of the complexities of scientific knowledge production. Instead, the quasi-open system negates any possibility for responsiveness between actors operating on the different scales of (and recursively constructed by) scientific translation. Phrased differently, the scientists enable the subaltern to participate but not engage. This problem clearly results from the trans-state imperialism of a network performing cuts – geographical as well as institutional – that distance the initial knowledge producer from the ultimate product. In other words the logic of colonialism – that “[t]he West is now everywhere, within the West and outside; in structures and in minds” (Nandy 1983:xi) – continues to inhabit and animate this trans-state science.

While Hayden attends deftly to the necessarily anti-democratic system embedded in the mutual production of scientific knowledge and corporate profit, she does not always address the forms of history and historicity that enter into friction with

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<sup>39</sup> Hardt is presenting an alternative to the competing regimes of “public” and “private” ownership that inhabit contemporary capitalism, taking the “common,” or earthly resources and the outcomes of human creativity, as forms of production that cannot be translated into either collective or individual property without violence.

contemporary scientific/imperial knowledge. Engagingly, Hayden does explicitly reference the history of colonial botanical knowledge in her chapter on collecting specimens “by the side of the road,” where both the constant circulation of vehicles incidentally moving botanical materials and the material barrier of the road itself create a distinct ecological niche. I mention this example for two reasons: to highlight the type of historical narratives that inform Hayden’s work as an example of (Mexican-U.S.) postcolonial science studies; and because roads occupy a similar empirical, aesthetic, and political position in Redfield’s account of the Ariane space station in French Guiana, to which I return momentarily. Riffing on Kathleen Stewart’s pathbreaking work on affect, place, and the representational limits of ethnography – *A Space on the Side of the Road: Cultural Poetics in an “Other” America* (1996) – Hayden considers roadside specimen collection as an epistemologically generative “suture zone” (2003:187) revealing the forms of contemporary interest and encounter that define bioprospecting within a naturalcultural landscape materializing histories: “In northern Mexico, as in many places, one cannot dwell for long on the paths traced by previous generations of botanists without also tracing the exploratory endeavors of loggers, miners, and missionaries – and in turn, the oft-erased indigenous interlocutors who helped guide those explorations” (Hayden 2003:172).

Unlike Hayden, Redfield constantly attends to the colonial forms that have conditioned contemporary science. Like this study and Hayden’s work on bioprospecting, Redfield’s research into the establishment of a space station in French Guiana works together the methodological and political convictions of science studies and postcolonial studies (in his 2000 book, *Space in the Tropics: From Convicts to*

*Rockets in French Guiana* and his 2002 article, “The Half-Life of Empire in Outer Space”). Indeed, Redfield (2002) poses his work as a project that inherits equally from Chakrabarty’s provincialization of Europe and Latour’s demonstration of scientific networks’ internal hybridities. Attentive to productions of both place and space, Redfield emphasizes how Chakrabarty and Latour each employ spatial frames for thinking about knowledges’ productions and consequences:

Distinguishing Chakrabarty’s and Latour’s patterns of thought is a concern for the enduring legacy of empire and the imbalances of human history. To take both authors seriously poses a riddle: we may never have been modern, or at least not in some of the ways we like to think. But some of us have certainly been more colonized than others, marked by race, language and other artifacts of historical difference. What are we to make of these combined insights (Redfield 2002:795)?

Redfield (2002:795) demonstrates the productivity of this frame through an argument that “outer space reflects a practical shadow of empire” in a twofold sense: the exploration of space has always been figured as a technological extension of colonialism, a scalar shift that has served to provincialize the globalizing pretensions of (Western) humanism itself; and the verticality of space travel introduces a new (third) dimension to the operation of empire that requires an expanding sociotechnical assemblage.

Redfield would like his readers to follow in seeing the vertical turn of rocket launches as a scalar shift that extends rather than deviates from European colonial projects. Unlike Hayden’s work, Redfield’s project requires an intricate reading of the colonial institutions that preconditioned the contemporary extension of colonial surveillance to an extraterrestrial scale. French Guiana, France’s lone *département* in South America, has represented both an outpost and a limit of European power and sovereignty since the establishment of French plantations in the late seventeenth

century. An object of both desire (the El Dorado of Sir Walter Raleigh [1928(1596)])<sup>40</sup> and disgust (the paranoia of racial climatology), French Guiana has staged fantasies of technological futurity on the boards of colonial contamination (to use a theatrical metaphor). This staging, in/of/as the material network that has launched about half of the existing commercial satellites (Redfield 2002:800), has implicated the historicity of a dehumanizing colonization in the futurity of a posthumanizing expansion. Despite the spacey play, Redfield follows through with the interlocking empirical commitments of (contemporary open systems) ethnography and science studies in asking how the institution of a rocket launching center has brought subjects representing distinct interests into productive and destructive friction.

Among the most tangible of encounters between representatives of these interests involves the *Centre Spatial Guyanais*' (CSG) attempt to redirect local traffic away from the space center by closing the relevant stretch of the single paved Guyanais road and building a deviation that lengthened the local route (Redfield 2000:234-6, 2002:802-11). When the CSG moved to close the road in 1994 – citing security concerns – a group of local politicians mobilized to inspire citizens' opposition to the project, calling attention to the failure of the space station to respond to local interests. Both the local politicians and, later, a political organization named the Movement for Decolonization and Social Emancipation (MDES) registered their opposition to the project in terms of the continued internal colonization of French Guiana. They desired to open a regime of vision in which CSG representatives might see the contradictions of a space station constructed in the middle of an economically destitute territory. Further, the object of this political dispute, a road, indexically references competing understandings of the past itself:

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<sup>40</sup> While "Raleigh" is often spelled "Raleigh," here I retain the spelling employed by Redfield (2000).

Though all recognize the road to be a legacy of the past, the ‘past’ involved likewise varies widely, from long-failed slave plantations and a consistently neglected colony, to an earlier phase of a successful enterprise and state investment, one now superseded by modified security considerations. Behind these spatial-temporal frames lie the asymmetries surrounding modernity and Europe invoked by Chakrabarty. For space officials, their own modernity is never in question, providing they kept pace with rival rocket consortia and the national entities involved in them. For at least some of those protesting the road closure, their own modernity is potentially in question and directly at stake in the road. For the elected officials of French Guiana, appropriately positioned in a mediating position, the *calculation* of modernity is strategically in question, understood to be ‘development’, as measured in European terms and supported by state appropriations (Redfield 2002:803).

From my point of view, such a conflict is a kind of “postcolonial moment” (Hall 1992; Verran 2002; see Chapter 2) that illustrates precisely how framing devices that differentiate domains of Latin American science, politics, and history should have no claim to pre- or a-political status. The CSG representatives sustain the inequalities articulated by internal colonialism by defining the institution’s expanding sovereignty as a technical (read: apolitical) extension.

Redfield would like readers to push analysis of this metapolitical disjuncture one step further, calling attention to the political parties’ differential positioning within the network performed as space rockets, satellites, roads, development projects, and their human subjects. In Latourian terms, the space center “could not *hide the length of its network*” (Redfield 2002:807; emphasis in original). The MDES’ invocation of colonialism sought to expose precisely how the CSG exercised its own political will by invoking both France and outer space as allied actors in a long and coordinated network that could quash the MDES’ localized interests. In turn, the MDES assembled a network – or, perhaps, a counter-network – that linked the contemporary struggle to historical injustice. As in Hayden’s work, roads thus figure as sites of cultural

production, reproduction, and destructive transformation. Symbols of modernity, development – and now science – in Latin America, roads also carry the past into the present and beyond.<sup>41</sup>

Thus, in Redfield's work, I find an example of how contemporary postcolonial science studies scholars must be willing to "follow the actors themselves" (Latour 2005b:12) through networks of association innovating forms of life, relationality, and knowledge that do not simply expand spatially, but also "expand" temporally. Such concept-work entails a "posthumanist" definition of history as a political form of human/nonhuman association that can be narrativized through reference to past events. The importance of such a concept becomes even more apparent in light of Redfield's illustration of a contest over neocolonial science where localized subjects must turn to the past as a political ally to build a counterhegemonic network. Redfield's concern with the "length" of these competing, co-constituting networks reflects how they serve, in Tsing's (2005:57) phrase, as "scale-making projects."<sup>42</sup> Without reducing time to space, it has become crucially important in the contemporary post-globalist episteme to recognize that scale-making is as much a historical project as a geographical one. This is a political position; need I point out that such scale-making invocations of the past –

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<sup>41</sup> Roads strongly symbolized the onset of "modernity" for modernist-developmental Mesoamerican anthropologists of the mid-twentieth century. See, especially, the work of Robert Redfield and Alfonso Villa Rojas (1971[1934]) on the 1930s construction of a road to the Yucatec Maya village of Chan Kom. Engagingly, where Robert Redfield and Villa Rojas imagine roads as symbols of progress and futurity, both Hayden and Peter Redfield draw us into the contradictory historicities that mark these landscape features. It seems almost as if the vertical turn of the space age left the symbolic charge of roads in the dust.

<sup>42</sup> Tsing develops the concept of "scale-making projects" to describe the kinds of performance and conjuring that have shaped productions of international finance, linking local, regional, and global imaginations and interests in the post-Cold War era. As she puts it, "In these times of heightened attention to the space and scale of human undertakings, economic projects cannot limit themselves to conjuring at different scales – they must conjure the scales themselves... Scale is the spatial dimensionality necessary for a particular kind of view, whether up close or from a distance, microscopic or planetary. I argue that scale is not just a neutral frame for viewing the world; scale must be brought into being: proposed, practiced, and evaded, as well as taken for granted" (Tsing 2005:57-8).

the mediation of historicized actors – conform effectively with subaltern studies scholars' conviction to represent history from the position of marginalized subjects?

Of course, this is simultaneously a political statement on the research orientations of science studies itself. As science studies scholars have recognized at least since Traweek's (1988) ethnography of physics laboratories, and the subsequent 1990s rise of concern with biomedicine (Fischer 2007b), the presumption of a hierarchy of sciences ranking knowledge from soft-social-human to hard-physical-nonhuman has functioned as an ideological barrier that defers an empirical, "positive" knowledge of the roles of all sciences as institutional sites of cultural (or naturalcultural) production. In light of the *temporal* extension of networks – hegemonic, counterhegemonic, or otherwise – in contexts marked by colonial and imperial racial-ethnic cuts, I think that it is now time for a more thoroughgoing and systematic science studies inquiry into the historical sciences, and the human sciences more broadly. In Latin America, such a project would benefit tremendously through thicker engagement with the existing expressions of critical ethnography, to which I turn briefly in the following section.

### **Engaging Critical Ethnography**

The literature that I have examined thus far in Chapter 3 has mostly occupied the traditional focus of science studies on the physical sciences, biosciences, and technosciences. While Redfield's subjects clearly enroll historical actors into their networks, the neocolonial contest centers on the technologies for launching rockets. It is, of course, reasonable to assume that the subject areas of science studies analysis in Europe and the U.S. would also be the subject areas for the field's Latin American development. As Latin American states have increasingly adopted democratic forms of governance and emphasized technological innovation in economic programs, this

consistent application of science studies is certainly merited. Yet, I advocate that the use of science studies concepts and practices in Latin America (particularly Mexico and Guatemala) should remain open to analyzing fields of knowledge production outside the conventional terrains of critical and instrumental STS. As discussed in the subsection on Latin American colonial science, the disciplines of history and anthropology have contributed directly to the formation of government policy, as well as both statist and anti-statist nationalisms (Lomnitz 2001; Tenorio Trillo 1999; Vázquez León 1987, 2003). Thus, while disciplines such as history and anthropology are not “hard” sciences, they have contributed to public discourses, practices, and forms of self-identity that make them fertile ground for examining their politics of knowledge production.

Questions of governmentality, nationalism, indigeneity, neoliberalism, and war have inhabited central positions in contemporary critical Latin Americanist ethnography (e.g. Hale 2005; Jackson and Warren 2005; Lomnitz 2001; Nash 2001; Nelson 1999, 2009; Stephen 2002; Taussig 2003; Warren; Watanabe and Fischer 2004). Scientific knowledge, including social scientific knowledge, has received markedly less attention. This section promotes historical knowledge as a fundamental set of analytical artifacts for understanding contemporary formations of capital, identity, and politics. I travel down this path in order to locate the spaces in which an already fertile analytical terrain might be enriched by attention to another prominent set of cultural practices. For me, this is not a question of reflexivity, of “self-” or any other varieties. Rather this is a political – and, I hope, activist – attempt to render more effective interventions into forms of graduated citizenship (Ong 2006; see above).

The consequences of historical interpretation for contemporary forms of Maya cultural politics is not a new topic (see Breglia 2006; Castañeda 1996; Fischer and Brown 1996; Nelson 1999, 2009; Warren 1998). Some of these authors, such as Diane Nelson, have drawn from the science studies literature, while others, such as Kay Warren, have conducted ethnographic research on workshops linked to those that I discuss here. Still others, such as Jan Rus (2004), have presented insightful historiographical re-interpretations of Mesoamerican ethnographic practices and institutions, such as the Harvard Chiapas Project.<sup>43</sup> However, perhaps with the exception of some of Nelson's work, these authors have not framed their research as contributions to the anthropology of science.

This leads me to ask how a science studies-inspired attention to public science may contribute to activist critiques of the contemporary racial-linguistic cuts in Guatemala, the outcome of colonial forms that continue to exercise tangible influence. In the case of Guatemala, postwar neoliberalization has shifted some governmental power and authority into a dispersed field of non-state institutions, providing an opening for nongovernmental organizations (NGOs), including linguistic institutes, to emerge as representative powers (Hale 2005). However, members of new research and activist organizations who have achieved significant agency over the terms of their identities

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<sup>43</sup> The Harvard Chiapas Project is an additional example of the sorts of collaborations across difference that have marked the application of social scientific knowledge to Mexican governance (Tenorio Trillo 1999). Originally conceived and enabled in the 1950s by the Mexican anthropologists Alfonso Caso and Alfonso Villa Rojas, leaders of the Mexican *Institución Nacional Indigenista* (National Indigenist Institute), the project was an extended series of community studies conducted almost exclusively in Zinacantan, a Tzotzil-speaking Maya community near San Cristobal de Las Casas, Chiapas. The Mexican intellectuals supported the project as an attempt to discern effective policies for integrating indigenes into the Mexican *mestizo* nation. Supervised and administrated by Harvard cultural anthropologist Evon Vogt, between 1957 and 1977 the project likely produced the most extensive ethnographic documentation of any community in the Americas, hundreds of publications describing a town with a population that never exceeded ten thousand people (Rus 2004).

and histories have simultaneously found themselves subject to a transnational knowledge economy built by a 500-year history of colonial and imperial science. The messy contradictions of this inheritance led some to describe Maya activists' efforts, in Nelson's (1999) titular phrase, as *A Finger in the Wound*. Indeed, especially for the state, though also activist NGOs, the channels through which historical sciences can become accountable to and thus constitutive of publics remains uncertain. As in Hayden's analysis of knowledge producers for bioprospecting, some sectors of the public remain excluded from recognition and response by the scholarly and governmental institutions that dictate the terms of their "scientific citizenship" (Michael and Brown 2005).

Cultural and linguistic analyses, including those presented by Maya activists, inscribe our bodies, scientifically translating them into source material for some of the most prominent political classificatory systems (Bowker and Star 1999), cutting them up to make them intelligible to states and their private allies. To more effectively address such problems, the oppositional critique of scientific knowledge as a set of naturalcultural institutions that produce and maintain colonial, racial, and gender inequalities should come into more intimate dialogue with the existing critiques of neoliberalism, war, indigeneity, and history that animate Latin Americanist anthropology. Such a convergence will require significant local care and attachment. Before concluding this chapter, here I elaborate one possible point of productive friction between science studies and critical Latin Americanist work: the practice/analysis of witnessing. Witnessing plays an important role in the historical scientific knowledge production that I describe in subsequent chapters. Thus, here I am initiating a recurrent

question concerning the imbrication of the epistemological, political, and ethical in a practice that helps constitute Maya hieroglyphic decipherment.

In her thoughtful, careful, and evocative ethnography *Paradise in Ashes: A Guatemalan Journey of Courage, Terror, and Hope*, Beatriz Manz (2004) draws readers into the violent world of the highland Guatemalan department (or political district) of El Quiché during the course of the civil war. Introducing her own positionality, grounded in long-term political and ethnographic engagements in El Quiché, and building on Nancy Scheper-Hughes' (1995) insistence that ethics should be central to activist ethnography, Manz frames her activism in the human rights discourse of witnessing:

It [activist ethnography] is also a recognition that being a witness at times demands action, and that failing to witness in these situations is amoral or perhaps immoral. The communities where we immerse ourselves are generally far less able than we ourselves to expose human rights violations, abuses of power, and repression. The choice is really not between ethnography and activism. Certain circumstances call for an ethnography that is aware of the broader social conditions in which ethnographer and subject find themselves (Manz 2004:6-7).

The political and epistemological contributions of science studies could directly aid ethnographers in achieving such action from a historically informed position. For example, in an article on the production of truth by medical humanitarians, Peter Redfield (2006) has turned precisely to the broad assemblage of ethico-cultural practices termed "witnessing" (within which Manz's concept of "witnessing" fits), asking how the humanitarian organization Doctors Without Borders employs discourses of witnessing to put the technical, fact-production-oriented act of observation into the service of explicitly political value-oriented ends. Redfield presents "witnessing" as a conflicted space of internal debate within European branches of Doctors Without

Borders, mixing medical practices and moral convictions with differing degrees of utility in reducing suffering.

The type of witnessing that Manz advocates directly resembles the moral discourse of European doctors working in postcolonial contexts, often genocidal zones like the Guatemalan civil war during the early 1980s. While these doctors resemble the “modest witnesses” emergent out of Enlightenment science, whose freedom from personal, subjective interests ostensibly permitted them to autonomously represent the external world (Haraway 1997; Shapin 1994; Shapin and Schaffer 1985), they are not simply neutral spokespersons. Redfield (2006:17) describes their practices of witnessing in these terms:

Collectively, then, MSF [Doctors Without Borders] operates as a modestly immodest witness, a mirror of reality that reinforces its own unmotivated veracity even as it transmits a distinctly moral framing vision. This vision retains elements of the generative moral economy of early modern European science, the complex combination of self-restraint and self-effacement that produced “those key aspects of science we somewhat grossly term quantification, empiricism, and objectivity” (Daston 1995:23). But it also extends beyond such dictates of personal and professional conduct to ask the question of who suffers in any given situation and what “common decency” would demand in response.

Manz (2004) would like more Latin Americanist ethnographers to resemble these medical doctors in using their institutional and sociopolitical positioning to expose the acts of violence that cut bodies, families, and communities. Such is the post-relativist standpoint that Scheper-Hughes (1995) insists that we must adopt. What the politically-committed, activist, “oppositional” science studies scholars such as Haraway (1997) have to offer to this discussion begins from the observation that scientific – including ethnographic – forms of scientific witnessing take place within regimes of vision that have historically marginalized – performatively cut – non-male, non-Europeans as

contaminated and contaminating subjects incapable of speaking authoritatively about an external world. Thus, scientific witnessing itself has been a violent act. This observation does not automatically damn the moral and ethico-political valence, indeed necessity, of witnessing. Rather it serves to further flesh out the ways in which we, as ethnographers, must be “aware of the broader social conditions in which ethnographer and subject find themselves” (Manz 2004:7).

The acknowledgement of knowledge practices’ complicities in reproducing imperial forms is, however, a somewhat unsatisfactory endpoint today, especially if it leads to textualist negativities, such as those of the collaborative postcolonialists (Schueller 2009; see Chapter 2), without any recourse to the actual positive possibilities of representation (however partial and situated). Perhaps Haraway’s (2008) more recent exploration of the types of local attachments that we must craft with care as we consider how our lives impinge on those humans and nonhumans with whom we co-construct and cohabit worlds will serve as a more generous analytical modality. Personally, I wish to trace how such an ethic of local attachment helps us, as scholars and activists, to evoke the past and give it manifest presence through practices of both scientific and humanitarian witnessing. I want a non-innocent but optimistic historical consequentialism in Latin America. Such a position will help clarify the ongoing significance of historical science in Latin America, determining which consequences get to count in judging historical truth claims.

When it comes to the significance of historical narratives and objects, this may be a messier political and ethical standpoint that Manz seems to desire:

Many people – both within and outside academia – have been more fascinated with the ancient Mayan civilization than with the living Mayas.

Some archaeologists conducting research in Guatemala at the height of the terror seemed oblivious to what was happening around them. In 1984 archaeologists discovered a well-preserved tomb in northern Guatemala dating back fifteen hundred years. It garnered prominent coverage in the major newspapers in the United States. The *New York Times* ran a front-page article entitled “Untouched Mayan Tomb Is Discovered,” which reported on the find and included a photograph and an artist’s representation of the tomb [Glueck 1984]. The article related that Guatemalan guards were protecting the extraordinary and valuable site with rifles and machetes. I sent a letter to the *Times* the following day [Manz 1984] that was published under the title “Mayas Celebrated and Mayas Persecuted.” I wrote that “the impression is given that these brilliant pre-Hispanic people left only fossilized heritage” and questioned, “How will the four million Mayas in Guatemala receive the news?” I wanted to provide a sense of the conditions in which contemporary Mayas find themselves. “Too many are grieving over fresh tombs of kinsman recently killed. Tens of thousands hiding in the mountains and jungles will not hear of the discovery for some time.” I found it ironic that the same army that was littering the countryside with fresh bones was treating ancient bones with such respect and concern (Manz 2004:7).

I read this passage as Manz’s reflection on her attempt to intervene into a network that employs the scientificity of archaeology to cut away the very real political and bodily violence that helped sustain such historical research. Manz expresses necessary dismay over the science-media assemblage’s failure to attenuate the contradiction of romanticizing the (ostensibly absent) past Maya while ignoring (or negating) the tangible living and suffering Maya of the present.

A science studies-inspired attentiveness to the constant presence of nonhumans, including “dead” – or, perhaps more accurately, “former” – humans as political actors sometimes allied and sometimes cut provides a necessary corrective to the assumption that that ancient Maya tomb should not receive the attention of those scholars working against the political hegemony and bodily violence of war. The problem is not just how or whether the living Maya will care about the objects expropriated by the state as patrimony. The problem is how the network that enrolls those historical objects is

alternately extended and cut in representational practices that do or do not conform with our political standpoints. The politics of history is no simple, deferrable bourgeois pursuit that stands in the way of serious opposition to violence. Whether we like it or not, control over history, including histories fifteen hundred years past, is control over violence, life, and death.

Unlike physics or the biosciences, there is no obvious reason to suspect that the particular fields of Mayanist archaeology and epigraphy contributed significantly to Cold War competitions and post-Cold War democratization. While the historical interpretations resulting from archaeological and epigraphic research lacked direct application to the arms race or the violence in Guatemala, the resulting interpretations did serve as (largely unintended) resources for constructing and controlling forms of political solidarity and historical consciousness. The most obvious such application has been the “Pan-Maya Movement” beginning in the late-1980s, whose protagonists have employed discourses of precolonial history crafted by archaeologists and epigraphers to construct or strengthen a widely shared indigenous identity transecting linguistic differences (Fischer and Brown 1996; Nash 2001; Warren 1998; Watanabe and Fischer 2004). Nevertheless, the armed guards standing over an ancient Maya tomb clearly materialized state claims to control of the past, an issue that demands more nuanced attention from both mass media and ethnographers.

That archaeological histories attracted public interest and were endowed with political capital was obviously not new to the final years of the Guatemalan civil war and the postwar era. Indeed, archaeology has provided material for state constructions of the nation since shortly after Guatemalan independence from Spain (Mazariegos 1998).

Yet, the end of the civil war did mark a shift in the public relevance of epigraphic research into the Maya past. Guatemalan democracy emerged on shaky ground in part because political-ideological conflicts also had long-standing foundations in ethnic-linguistic tensions. The intense violence of the civil war did not smoothly give way to a peaceful democratic republic (Cleary 2002). Thus the “bipolarity” which Elam and Bertillon (2003) associate with the Cold War has never fully dissolved in Guatemala. Neither the political-economic bipolarity of capitalism (or oligarchy) versus socialism (or local autonomy), nor the identitarian bipolarity of the Ladino versus the indigenous disappeared. But the *significance* of an indigenous-Ladino opposition did shift somewhat, and, by the 1990s, expressly manifesting an indigenous, non-Ladino self-identity could be simultaneously an opportunity and a risk, rather than solely the latter. In this neoliberalizing era, the risks still far outweighed the opportunities. Yet, sciences such as Maya epigraphy could be envisioned as resources for both the state and activist organizations to shape the opportunities and risks of Maya identities.

### **Conclusions**

This chapter has offered an extensive review of the intersection of science, politics, and history in Latin American contexts. I have argued that an effective Latin Americanist postcolonial science studies should attend directly to the forms and consequences of historical knowledge production in Latin America. I have elaborated this perspective through a series of three sections. In the first section, I review the contemporary literature on the history of science in Latin America. This review provides the necessary groundwork for grasping how science has contributed directly to the operation of both colonial and post-independence governance in Latin America. Indeed, science, in many different manifestations, has served to provide these state

apparatuses with the necessary knowledge to achieve forms of population control through the production of historical subjects and, thus, life itself. My historical review focuses on the transition between a colonial science that directly fed into the violence of the *encomienda* system to a “postcolonial” science conducted by gentlemanly travelers who served as international witnesses to the empty terrain of Latin America, a discourse contested by nineteenth-century Mexican elites who cultivated a progressivist historicism exalting *mestizaje*.

While such historical sciences seem to have exercised direct control over Latin American populations, racializing linguistic and geographic distinctions, and constructing indigenized subjects as remnants of the past, the bulk of Latin Americanist science studies research, in both the Castillian-language and Anglophone literatures, has focused primarily on the cultural positioning, policy implications, and public perceptions of biosciences and technosciences. Thus, given the importance of social and historical sciences inheriting from nineteenth century modes of historicism, I have advocated that a locally attentive postcolonial science studies should consider historical discourses and objects as crucially important sites of naturalcultural production. Thus, here I substantiate the importance of historical narrative production that I introduced in attending briefly to subaltern studies and postcolonial theory in Chapter 2.

The final section of this chapter turns to an attempt to expand dialogue between science studies and other existing spaces of inquiry within critical Latin Americanist ethnography. As will become clearer through the remainder of this study, it is implausible and politically irresponsible to consider the historical and contemporary practices of Maya hieroglyphic studies as politically neutral. I begin to flesh out this

impossibility in the third section by foregrounding the politics of historical interpretation during the Guatemalan civil war. By attending to the example of humanitarian and scientific dimensions of “witnessing” as a social/scientific practice, I have drawn out how science studies may offer useful conceptual and activist tools to engaged Latin Americanist ethnography. At the same time, I recognize how locally oriented ethnographers have presented very valuable and historically attuned forms of empirical attachment that are absolutely necessary to effectively represent marginalized humans/nonhumans. I continue this project in the following chapter, as I turn to the rise of phonetic interpretations as a key transitional moment in Maya hieroglyphic decipherment. This analysis of decipherment stands on the political, ethical, and epistemological foundation that I have elaborated thus far through my critique and extension of postcolonial science studies.

## CHAPTER 4 PHONETICISM AND THE RISE OF MAYA HIEROGLYPHIC DECIPHERMENT

### Introduction

This chapter continues to establish the significance of an object-oriented postcolonial science studies perspective (that emphasizes concepts of “cuts” and “witnessing”) for the analysis of historical knowledge production. I analyze Maya hieroglyphic decipherment as a complex assemblage of human and nonhuman actors entering into and dissolving performative interrelations. Specifically, I cast the enrollment of philologist and epigrapher Yuri Knorosov into the decipherment network as an act with significant consequences for how hieroglyphic elements, or actors, were “cut...together and apart” (Barad 2007:179; see Chapter 2). Beginning in the 1950s, Knorosov argued that Maya hieroglyphs amounted to a fully functional writing system partially comprised of graphemic signs with phonetic equivalents. This development is an apt object of my concept-work because Knorosov’s innovation effectively articulated with techniques of structural analysis and Tatiana Proskouriakoff’s (1960) subsequent claim that hieroglyphs had historical content, establishing the basis for inroads into Palenque’s dynastic sequence initiated at the 1973 Palenque *Mesa Redonda*. Thus, I devote a section of the chapter to important innovations made during the early *Mesas Redondas*, treating the initial decipherment of Palenque’s dynastic list as a relational effect produced by the convergence of structural analysis, phoneticism, and Proskouriakoff’s argument for the existence of historical content in the hieroglyphs.

I suggest that Knorosov’s claim for phoneticism in Maya hieroglyphs had a pervasive restructuring effect on the alliances that Maya hieroglyphs could enact. In contrasting their positions, it becomes evident that Knorosov’s (1952, 1958) interpretive

divergence from his predecessors, such as J. Eric S. Thompson (1950), altered how hieroglyphic inscriptions were dissected into distinct units of meaning. But my object-oriented position further reveals how this divergence entailed Maya hieroglyphs' collective retreat into a closed semantic system, as they cut alliances with a non-semantic material world populated by other actors, including past and present Maya subjects, cosmologies, and the architecture and objects into which the hieroglyphs were originally inscribed.

As explained in Chapter 2, a foundation of the object-oriented ontology that I adopt holds that the movement of actors (here, hieroglyphs) through novel relational assemblages necessarily alters the objects themselves. Thus, by tracing hieroglyphs' movements into a closed system, I am working to establish the position that decipherment – here meaning the process by which Maya hieroglyphs are rendered sensible within contemporary textual logics – depends integrally on a series of material and semiotic alterations of the hieroglyphs. Decipherment, in other words, requires the substitution of new material contexts for hieroglyphs' original associational fields. I am putting forth the claim that an important and unavoidable – but not totally unbridgeable – gap exists between contemporary readings of hieroglyphs and their original meanings.

In tracing this process of abstraction and alteration, I present Knorosov's foundational transition for contemporary methods of hieroglyphic decipherment as an epistemological and ethical trade-off, an excision of hieroglyphs' alliances with actors comprising real people and places in order to witness how they associated among themselves (and articulated with experts such as Knorosov) in a controlled environment. In something beyond a metaphorical sense, Knorosov initiated an antiseptic textual

laboratory for experimenting upon the emergent Maya writing system with a high degree of control over the scientific objects, Maya hieroglyphs. Thus, while apparently increasing hieroglyphs' significance by virtue of stabilizing their linkages with delimited phonetic values, Knorosov's mediation actually decreased the quantity of hieroglyphs' alliances, *reducing* them to objects that could be understood as writing. Such a position entails the more general assumption that hieroglyphs can be cut and read independently of any particular indexical meanings. As I explain in the first section below, such a position seems heavily conditioned by contemporary concepts of language and writing which are, I maintain, largely inapplicable to the Precolumbian Maya.

The reductive, decontextualizing, structural scientificity established by scholars such as Knorosov (and others including Thompson) has not, however, irreversibly extracted hieroglyphs from the extra-laboratory world. It is possible to save hieroglyphs from their reduction to writing, to mediate a renewed proliferation of hieroglyphs' associations, and to do so without critically debunking the argument that they have semantic and phonetic values (though it does remind us that their semantic values only exist through relations with humans). To deploy hieroglyphs back into the real, dirty world in Latourian style is to help democratize historical knowledge production by opening up these objects to articulation with a wide range of otherwise inaccessible actors.

While, in recent years, Mayanist epigraphers have paid greater attention to the context-specificity, or indexicality, of hieroglyphs' meanings (e.g. Houston and Taube 2000, discussed below; see also Inomata 2006 for the role of what he calls "theatrical

performance” in elite Maya rulership), they have not adequately reflected on how such a semantic localization has recursive consequences for the premises of the (“laboratory”) decipherment articulated by Knorosov and his intellectual inheritors. Nor have they adequately addressed a dual-paradox of historical interpretation, that the actor-networks enabling hieroglyphs’ indexical significance in their original sites of production and use are further cut by the largely distinct actor-networks that condition the meanings of these text-artifacts today.

An object-oriented metaphysics and methodology helps rescue hieroglyphs from their reduction to texts and restore the extra-semantic associations that condition and issue from interpretive propositions concerning hieroglyphs’ meanings. In particular, I emphasize how the enrollment of Knorosov into the hieroglyphs’ actor-network effected a further dissolution of alliances between hieroglyphs and “Maya” people who rendered these signs, engaged them in their original contexts, or were ascribed epistemically as the descendants of the signs’ producers. Since Maya actors have been systematically subjected to (and partially defined by) forms of colonial and postcolonial socioeconomic marginalization, this interpretive, or onto-epistemological, cut also raises ethical questions. Indeed, as in Cori Hayden’s (2005, see Chapter 3) example of the “protection” through anonymization of indigenous subjects who developed plant compounds subsequently patented for medical use, the scientific validation of a cultural innovation – here, the invention of a writing system – takes place through a paradoxical negation of the subjects who made the innovation and the material contexts in which the innovation occurred. While less absolute than Hayden’s example, this negation still has ethical implications.

That Maya people themselves were partially cut away as a precondition for decipherment thus rings true with postcolonialist claims to the violence of representation and the Eurocentrism entailed in ascribing “history” to the subaltern (Chakrabarty 2000; Guha 2002; see Chapter 3). Longstanding subordination clearly cannot be undone simply through the progressive, historicist casting of the Maya writing system as a civilizational achievement. Though, if we can situate and attach this articulation more effectively, the achievement may still have utility for democratic activist ends (and some non-Maya Mayanists have definitely contributed to these ends; see Chapter 6).

I develop my argument here through three sections. The first section offers necessary background for apprehending how contemporary Mayanist epigraphers apprehend the structure and content of Maya hieroglyphic writing. In order to establish the utility of a science studies-perspective for explaining how Maya hieroglyphs transformed into legible, literal signs, it is useful to first offer a brief sketch of the contemporary scholarly consensus concerning the structure of these semiotic objects. Thus, I provide an account of how contemporary epigraphers understand hieroglyphs, with an eye to how these rationalizations instantiate contemporary non-democratic scientific practices. The following section explores two contrasting stages of decipherment that have heavily conditioned the contemporary epistemic reduction of Maya hieroglyphs into transparently legible, literal writing: the twentieth century position that hieroglyphs were not a fully-functional writing system, which was elaborated by Mayanists such as Sylvanus Griswold Morley and J. Eric S. Thompson, and the rise of Knorosov’s claim for the existence of phonetic values in the hieroglyphs.

This is merely one example of how an object-oriented approach alters conventional understandings of hieroglyphic decipherment. Thus, to provide readers with a more comprehensive understanding of the range of innovations that preconditioned contemporary ideologies and practices of decipherment, I turn in the final section to an overview of three additional phenomena that preconditioned current hieroglyphic knowledge practices: structural analysis, Proskouriakoff's argument for the hieroglyphs' historical content, and the breakthroughs achieved at the early *Mesas Redondas*. Structural analysis has been a vital knowledge practice for cutting Maya hieroglyphs into component parts that make sense within contemporary language ideologies. In turn, Proskouriakoff established conventions for understanding hieroglyphs as literal texts that could be understood within positivist historical logics. Finally, the early *Mesas Redondas* marked a coalescence of these emergent phenomena within Maya studies, mediating the articulation of structural analysis, Knorosov's techniques for phonetic interpretations, and Proskouriakoff's argument for historical content.

Since it is of crucial importance to the current epistemic positioning of Maya hieroglyphs as a writing system, I devote the most thorough treatment to Knorosov's intervention into the existing disciplinary assemblage, contrasting his position against Thompson's (and Morley's). I focus particularly on the shifts in hieroglyphs' alliances evidenced in and enacted by Knorosov's significant article, "The Problem of the Study of the Maya Hieroglyphic Writing," which was translated by Sophie Coe and published in the flagship English-language archaeology journal *American Antiquity* in 1958. My analysis emphasizes how a postcolonial science studies perspective foregrounds the

epistemological and ethical implications of this reduction of Maya hieroglyphs to writing, a necessary precursor and bridge to the analyses of laboratory-like decipherment in workshops discussed in the subsequent two chapters.

### **The Structure of Maya Hieroglyphs**

Grasping precisely how a science studies vantage usefully diffracts and elucidates hieroglyphic writing requires a review of epigraphers' running assumptions. This subsection thus reviews contemporary hieroglyph experts' understanding of what Maya hieroglyphs are and how they function as "writing" or "texts," leaving these abstract concepts momentarily unparsed. Readers who desire a more complete background on the structure of Maya hieroglyphic writing should directly consult either current textbooks designed to teach novices how to read the script (e.g. Coe and Van Stone 2001; Montgomery 2002b) or reviews of the state of the field (Houston 2000; Houston and Lacadena García-Gallo 2004; Wichmann 2004). Additionally, the workbooks printed for the Austin, Texas Maya Meetings, one of the workshops discussed in this study (Chapter 5), have served traditionally as annual "state of the art" introductions to hieroglyphic writing.

Though it stands as a relatively recent development in Maya studies, hieroglyphs are now considered a fully functional writing system, endowed with a complete representational capacity. While earlier Maya inscriptions exist and have some identifiable components, it is apparent to contemporary epigraphers that Maya inscriptions became fully syntactic linguistic forms by 150 CE, the end of what Mesoamericanists term the late Preclassic period (Houston 2000:145). The writing system remained in use into the early colonial period. This is evident from both Bishop Diego de Landa's (1994[c. 1566]) attempt to produce an "alphabet" with a native

informant and the transfer of Maya hieroglyphic writing conventions into Latin script in the post-Conquest Books of Chilam Balam of Chumayel and Chan Kan (Bricker 1989). Subjects elaborated texts through diverse techniques and employed diverse material media: bas-relief and alto-relief inscriptions into architecture and stone monuments (wall panels, lintels, and stelae); painting on fold-out bark-paper manuscripts misnamed “codices” by early writers; painting on and occasional molding of or engraving into ceramic pots; inscription into bone, jade, and shell; and painting on wood and stone; as well as painting or inscriptions on media such as clothing and gourds, which have not preserved to the present. The heyday of Maya hieroglyphic writing helps to define the Classic period (250 – 900 CE), an era in which major Maya archaeological sites are characterized by significant quantities of hieroglyphs as monumental inscriptions.

The notion that the hieroglyphs were writing dates to the widely circulated claims made by nineteenth century U.S. explorer John L. Stephens (1963[1843]; 1969[1841]), who could be considered a quintessential “philosophical traveler” (Cañizares-Esguerra 2001; see Chapter 3). Mayanists narrating the history of their field prominently invoke Stephens and his travel companion Frederick Catherwood, an English artist who produced the realist drawings of hieroglyphs printed in Stephens’ publications. Stephens’ (1963[1843], 1969[1841]) travel accounts and Catherwood’s drawings established an aesthetic and ideological frame for understanding Maya hieroglyphs that endures to the present. Stephens’ aestheticization of Maya hieroglyphs diverged sharply from predecessor antiquarians such as the Prussian travel writer Alexander von Humboldt (Leask 2006). While Humboldt treated the Maya inscriptions as historical artifacts revealing a degree of civilizational achievement, he denied them aesthetic

status as “art” and semiotic status as “writing,” reserving such treatment for Western accomplishments. As literary critic Nigel Leask (2006:131) puts it, “recognition of the cultural achievements of pre-hispanic America was...still tied to a privileging of Eurocentric global history.” Stephens challenged this global history rooted in a Greco-Roman classicism through his ideological appropriation of Maya accomplishments as the American equivalent of European antiquities.

In his readiness to appropriate Maya art and architecture as an accomplishment that could be embraced by his readers in the westward-bound Jacksonian United States, Stephens heralded Maya hieroglyphs as *writing* that would make the ostensible disappearance of the ancient Maya intelligible. Stephens (1969[1841]:159-160) framed the inscriptions at the Maya site of Copan as legible writing: “One thing I believe, [is] that its history is graven on its monuments. No Champollion has yet brought to them the energies of his inquiring mind. Who shall read them?” Conjuring the classicist and philologist Jean-François Champollion, who had deciphered the Rosetta Stone 17 years before the publication of Stephens’ 1841 *Incidents of Travel in Central America, Chiapas, and Yucatan*, Stephens posed a resounding challenge to future Mayanists. He also helped establish the framing device for a linguistic ideology (and ontology) that would treat Maya inscriptions as potential historical records.

However, the influence of such arguments had largely faded by the mid-twentieth century. Mayanists working in the 1950s and 1960s understood only some of the inscriptions as readable signs, particularly numbers and the calendrical notations that often introduce monumental texts. Decipherment of the numbers was initially grounded in accounts of Maya informants’ knowledge of a dot-and-bar number system that

appeared in eighteenth-century documents collected by the nineteenth-century Mexican scholar Juan Pio Pérez (Brinton 1882), as well as the independent decipherment proposed by Constantine Samuel Rafinesque-Schmaltz (2001[1832]). In turn, the meanings of many calendrical hieroglyphs were also effectively discerned by scholars in various disciplines during the late nineteenth and early twentieth centuries (e.g. Förstemann 1880, 1886, 1887-1894, 1893; Goodman 1905; Martínez Hernández 1928; Morley 1915, 1920, 1938, 1946; Seler 1887, 1888; Thomas 1893; Thompson 1950, 1962). Morley and Thompson were particularly influential in positioning these calendrical inscriptions as crucial indicators of Maya cultural practices and beliefs, as they published a number of widely read and used texts (esp. Morley 1946; Thompson 1962, 1971[1950]).

More recent scholars have reconsidered the interpretations of Morley and Thompson as reflections of the wider social ideologies and concerns of their era. Morley (1946) and Thompson (1971[1950]) seem to have situated ancient Maya leaders in a primitive utopian alternative to the competing hegemonies of the World-War era (Clancy 2006:823; Schele and Miller 1986:24), particularly in their claims that the ancient Maya were a peaceful civilization ruled by priests obsessed with the worship of time. These criticisms could be reframed in a more critical vocabulary as arguments for Morley's and Thompson's ideological construction of the Maya as an alterity. I do not consider such contemporary influences wholly damning for the value and consequentiality of their interpretations that Classic period calendrical monumental inscriptions are artifacts of divinatory rites and rituals surrounding passage of time. Yet, it is important to note that Thompson's and Morley's claims often adopted an Orientalist

rhetoric (following Said 1978), in that they constructed the Maya as the inverse image of the violent, political, individualistic, materialist West:

The Maya inscriptions treat primarily of chronology, astronomy – perhaps one might better say astrology – and religious matters. They are in no sense records of personal glorification and self-laudation.... They tell no story of kingly conquests, recount no deeds of imperial achievement; they neither praise nor exalt, glorify nor aggrandize, indeed they are so utterly impersonal, so completely nonindividualistic, that it is probable that the name-glyphs of specific men and women were never recorded on the Maya monuments (Morley 1946:262)

With very rare exceptions the inscriptions deal with the passage of time, with particular reference to the end of the period which was approaching when each inscription was carved. As we have seen, the unending journey of time was of transcendental importance to the Maya; it was the main theme of their philosophy of life, but, because of its effect on human life, it was much more than the focal point of a cult of mysticism. The powers wielded by the succeeding rulers of the divisions of time affected life to its very roots: the ruler of one day was a beneficent deity who brought happiness and prosperity with him; the lord of another day was a malevolent power, whose passage was fraught with misery and evil (Thompson 1971[1950]:64).

Thus, for Morley and Thompson the Maya (elites) were too concerned with spiritual matters surrounding astrology and the passage of time to care about recording other matters. This ascribed primacy of the spiritual was clearly based in the historically specific understanding of the hieroglyphs possessed by these authors. Their inability to grasp the non-calendrical components of the inscriptions had led them to emphasize the centrality of time to Maya cosmology.

So what hieroglyphic knowledge provided the foundation for this particular interpretation? Morley and Thompson knew that many monumental inscriptions began with a date. Mayanist Alfred P. Maudslay (1897) had established this in the late nineteenth century, naming such dates “Initial Series.” Maudslay’s contemporary, J. Thomas Goodman (1897:148), had speculated that these Initial Series recorded the

date of each monument's dedication. The Initial Series dates were part of a calendrical system known as the "Long Count." The Long Count measures the number of days elapsed since a mythological starting date, corresponding to August 13, 3114 BCE in the Gregorian calendar. It is complemented by another more pervasive Mesoamerican calendrical system termed the "Calendar Round" by contemporary scholars. The Calendar Round is comprised of two separate day-naming systems, one comprised of a 260-day interval (known as the tzolk'in) and the other comprised of a 365-day interval (known as the haab'). When paired, the tzolk'in and the haab' produce an intersecting pattern of paired day names that repeat every 18980 days, or 52 (365-day) years. While the Calendar Round repeats at 52-year intervals, the Long Count provides absolute dates, offering Mayanists an exceptional degree of precision in their ability to specify the dates on which events recorded in the hieroglyphs occurred.

It is also important to note that apprehending calendrical components of the inscriptions also depended on an understanding of the reading order of the hieroglyphs. This basic understanding continues to inform hieroglyphic interpretation today. I will momentarily describe individual hieroglyphs in more detail, but it is necessary to note here that Maya hieroglyphs are typically rounded quadrangular signs (sometimes termed "glyph blocks"). In texts, the glyph blocks are most frequently positioned within a grid pattern and read from left to right, top to bottom, in vertical double columns. Thus, in Figure 4-1, the established reading order is as follows: A1, B1, A2, B2, etc. At position B4 the reader returns to the top row: C1, D1, etc.

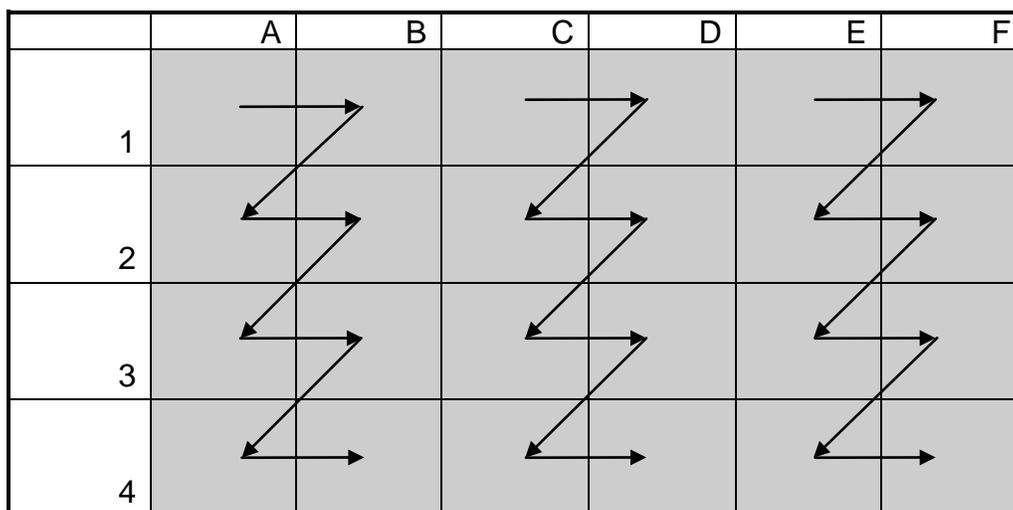


Figure 4-1. The reading order for Maya hieroglyphic writing.

Mayanists have positioned two early innovations as foundational events in the shift away from such emphasis of non-calendrical components and toward the emergence of a persuasive argument for hieroglyphs as writing (Coe 1999[1992]; Houston et al. 2001; Stuart 1992): the identification of phonetic signs (Knorosov 1952, 1958); and the identification of patterns in hieroglyphic dates that indicated the texts' historical content (Proskouriakoff 1960). These events signaled the beginning of the end for a dominant early twentieth century assumption that the hieroglyphs were largely icons, rebuses, and logographs (or word signs) that served limited purposes for religious acts of divination and lacked a full capacity for signification, as Morley (1946) and Thompson (1950) had maintained. The rise of the "phonetic" and "historical" approaches also led to a shift in interpretations of representational imagery associated with the writing. Mayanist such as Morley and Thompson considered such images representations of religious or cosmological figures and events, and worked within an epistemology that distinguished rigidly between religious figures and "historical" figures, meaning immanent human beings. By the 1970s, the emerging community of hieroglyph

scholars had begun to make claims that the hieroglyphs were “more” than icons, rebuses, and logographs, and in fact comprised texts with signs that combined logographic and phonetic components amounting to a completely syntactic script (e.g. Mathews and Schele 1974; Schele 1982a). In turn, the accompanying images could no longer be grasped as primarily or entirely mythological. Many such images came to be understood as realist historical representations of elite Maya figures (Schele and Miller 1986).

A crucial unit of analysis for hieroglyphic interpretation is the “glyph block,” or “glyph collocation.” Each square in Figure 4-1 above represents a single glyph block. Glyph blocks do not simply equate to words or sentences in alphabetic scripts, and are not clearly segregated by grammatical parts of speech. Glyph blocks typically contain multiple signs. The reading order of signs internal to individual glyph blocks follows the same general pattern as the reading order of glyph blocks: left to right, top to bottom. These signs combine to form vocally realizable morphemes, or basic units of meaning.

Epigraphers have categorically divided the signs that comprise each glyph block into functional forms: main signs and affixes (including diacritical signs and semantic determinatives that clarify or modify other signs’ meanings). Main signs, of which there are more than 700 currently identified, are often the largest component of each glyph block and are commonly logograms (e.g. Figure 4-2c, d; Montgomery 2002b:41). In addition to logograms, glyph blocks frequently incorporate, or are composed of, phonetic signs, termed “syllabograms,” of which there are approximately 200 in the script (Kettunen and Helmke 2008:6). Syllabograms sometimes combine with logograms as particular types of affixes, including “phonetic complements” that modify

or clarify the phonetic value of main signs with more than one interpretable meaning (e.g. Figure 4-2b, d) and “semantic complements” that modify or clarify its meaning. Alternatively, syllabograms can be used instead of logograms, combined to form morphemes out of multiple syllables (e.g. Figure 4-2a). That the Maya system combines logograms and syllabograms rather than using one or the other is sometimes cited as a reason for the initial difficulty in deciphering the script (e.g. Coe 1999[1992]). The availability of both syllabic and logographic signs enabled scribes to write some morphemes with many different “spellings,” or combinations of signs (Figure 4-2), variations that may reflect regional differences as well as scribal preferences (Houston et al. 2000).

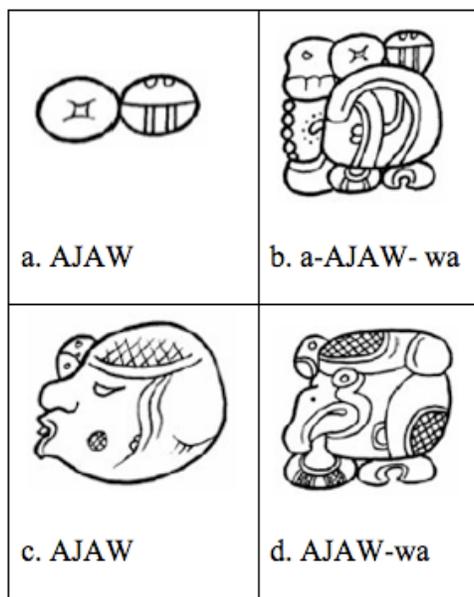


Figure 4-2. Four “spellings” of *ajaw*, lord (also spelled *ahau*). Logograms are transliterated as uppercase letters and syllabograms as lowercase letters. The figure is reproduced and the hieroglyphs are copied from Harri Kettunen and Christopher Helmke (2008:16). © Harri Kettunen, reproduced with permission.

Exactly how scholars deduced this graphemic system from the available evidence requires more than a technical overview of the structure of hieroglyphic writing. Indeed,

I hold that the deduced internal structure of hieroglyphs (as well as its “technical overview”) is inextricable from both the cultural conventions of Maya scribes and the interpretive conventions of those who labor over deciphering hieroglyphs today. I provide my own intensive analysis of these conventions, particularly aesthetic transformations of the hieroglyphs and shifting media through which collaborations have taken place, in Chapters 7 and 8. Here, I proceed to turn to an analysis of two critical stages in the formation of hieroglyphs’ contemporary epistemic positioning, focusing most closely on the emergence of arguments for the signs’ phonetic values.

### **The Rise of Decipherment**

A primary objective of this study is to reconsider the past and present significance of Maya hieroglyphs, as part of an effort to democratize contemporary epigraphic research through a treatment of the hieroglyphs as legitimate actors in and of themselves. Thus, in this section, I narrate the development of ancient Maya studies vis-à-vis the transforming roles of the hieroglyphs. This entails a departure from the conventional narrative techniques adopted by Maya hieroglyph experts who have sought to recount the history of their field (e.g. Coe 1999[1992]; Houston 1989, 2000; Houston et al. 2001; Stuart 1992). These experts position human interpreters, or scholars, as the central actors who motivated advances in decipherment. By exploring how Maya hieroglyphs played active roles in their transformation into writing, I am intentionally downplaying the somewhat romanticized and quite hyped roles of purified human interpreters.

I indulge this inversion in order to set up a substantial argument concerning the politics of decipherment. Namely, the hype is misplaced. Maya hieroglyphic decipherment is a *reduction* of complex aesthetic, material objects to a semiotic script.

This reduction was initially conditioned by what linguistic anthropologists call a “language ideology,” meaning “the situated, partial, and interested character of conceptions and uses of language” (Errington 2001:110; see also Irvine and Gal 2000; Kroskrity 2004; Schieffelin et al. 1998; Silverstein 1979). The particular language ideology governing decipherment treats hieroglyphs as objects that independently and transparently convey the literal, interior thoughts of ancient Maya subjects, a quintessentially modernist idea rendered thinkable by what anthropologist Webb Keane (2002:66) has termed the “convergence of Puritan morality and scientific objectivity.” While it is a pragmatically useful reduction, and one that makes hieroglyphs into properly scientific objects, decipherment also problematically defers both hieroglyphs’ original material contexts and their alternative contemporary functions and meanings.

In apparent tension with this ideological framing, contemporary epigraphers, particularly Houston, have noted that these texts had highly context-dependent communicative functions, and were situated within a language ideology that negated the possibility that the texts could be abstracted from their producers:

Mesoamerican writing [Maya and Mixtec] was not so much an inert or passive record, but a device thought to “speak” or “sing” through vocal readings or performance [see Monaghan 1990 for an account of the performance of Mixtec codices]. As a form of communication, writing was inseparably bonded to the language that it recorded. The view that script was an abstract, isolable text was most likely unthinkable, since, to quote Sennett [1994:43] on ancient Greek writing, the “reader would have thought he heard the voices of real people speaking even on the page, and to revise a written text was like interrupting someone talking” (Houston and Taube 2000:263-4).

Yet, while this passage seems to acknowledge that the contemporary language ideology governing the decipherment of Maya hieroglyphs differs starkly from the ideology enacted by the scribes, Houston and Taube (2000) do not address the far-

reaching ramifications for this ostensible epistemic discontinuity. They have ultimately ignored the more radical consequences of their claims to the context-specificity of hieroglyphs' meanings, as their ostensible interpretive progress depends on the claim that "certain readings [of hieroglyphs] are now set, regardless of personal whim and inclination, as reconstructed confidently by us and as intended explicitly by the Maya" (Houston 2000:128).

Reconsidering the position put forth by Houston requires more than an analysis of the linguistic ideologies in play. Indeed, it requires the postcolonial science studies perspective that I have developed thus far in this study, an object-oriented perspective that takes the hieroglyphs as consequential actors in their own worlds. Thus, in the next section I provide the requisite background for more intensive analyses in my fieldwork-based chapters. I offer an account of hieroglyphs' nineteenth- and twentieth-century extraction from their material contexts and translation into scientific objects with phonetic values. I proceed from an analysis of Morley's partial extrication of monumental inscriptions from their environmental contexts and, finally, to my important claim that Knorosov radicalized this scientific extrication and purification, radically transforming how hieroglyphs would be known in the process.

Morley's work provides a useful starting point for this analysis, because his philosophical travels marked an initial transition in hieroglyphic studies away from the prioritization of codices and toward the emphasis of monumental inscriptions (though Thompson and Knorosov continued to devote significant attention to the codices). Furthermore, analysis of Morley's writings can offer a number of suggestive cues for

postcolonial science studies, as he active characterized the Maya area in particular as a “laboratory” (see below).

### **Morley’s Hieroglyphic Assemblage**

While colonists, explorers, and antiquarians have interpreted Maya hieroglyphs since the sixteenth-century work attributed to Landa (1994[c. 1566]), a series of events in the twentieth century shaped the contemporary epistemic field much more thoroughly than earlier efforts. In his influential stagist history of epigraphic research, George Stuart (1992) classifies the initial set of these events as the “period of the institutions,” beginning with the founding of the Mexican Escuela Internacional de Arqueología y Etnología in 1910 and ending with Knorosov’s 1958 argument for phoneticism. This section focuses on the interpretations of hieroglyphs that dominated this period, as articulated principally by Thompson and Morley. The objective of this section is to trace some of the contours of the actor-network that Knorosov’s intervention significantly transformed, to which I turn in the following section. Subsequent critics, such as Michael Coe (1999[1992]), seem to consider the addition of more explicitly acknowledged actors – such as World War era utopianism – as a means of undermining the facticity of interpretations such as Morley’s. On the contrary, in a Latourian vein, I consider the acknowledgement of these mediating actors as a process that confers greater reality on Morley’s (and Thompson’s) propositions, and hold that their epistemological substantiality depends more on the consequences that they effect than their isolation from purportedly contaminating subjective, human processes.

Among the most prominent institutions shaping the epistemic terrain of Maya studies during the first half of the twentieth century was the Carnegie Institution of Washington (CIW), which employed Morley and, for a brief period, Thompson. In 1913,

Morley proposed the initial CIW project in Mesoamerica, an excavation and reconstruction of the site of Chichen Itza in Yucatan. While the Chichen Itza project was slated to begin under Morley's supervision in 1914, the sociopolitical instability instigated by the Mexican Revolution delayed the project indefinitely (Brunhouse 1971). During the project's delay Morley trekked to Petén (in Guatemala) with a mandate from his employers at the CIW to construct a chronology of Maya civilization. Notably, during this period Morley also used his archaeological credentials as a cover for U.S. government espionage work, attempting to document southern Mexican and Central American opposition to U.S. political and economic interests during World War I (Harris III and Sadler 2003).

Morley's (1920, 1938, 1943) early work on calendrical inscriptions at sites such as Copan, Uaxactun, and – especially – Chichen Itza shaped much twentieth-century thought on calendrical components of the hieroglyphs (even though many of his specific interpretations are now rejected). While Morley seems to have entertained the possibility of systematic phonetic interpretations of the hieroglyphic system early in his career (Brunhouse 1971), he abandoned this line of thought in favor of arguments that hieroglyphs solely comprised a calendar system based on astronomical observations. As Stephen Houston and colleagues (2001:11) put it, "This is a time when 'calendrical dogma' – the firm belief that calendrical and astronomical matters dominated Maya texts – became entrenched in epigraphic minds." Stuart (1992:38) summarizes it this way:

In the nearly half century that elapsed between 1910 and 1958, Carnegie and other institutions brought a vast amount of new material to light. In terms of major trends of research, we have seen how the decades before World War II witnessed an almost exclusive concern by Morley and others with calendrical, astronomical, and chronological matters. These new data

were derived largely from monumental inscriptions, which generally replaced the codices as the primary focus of attention.

Here Stuart also provides a useful addendum to the association of World War-era CIW-sponsored research with “calendrical dogma.” He presents a set of material actors linked integrally to the narrative of peaceful astronomer-priests: monumental inscriptions (both inscriptions into the walls of temples and inscriptions into stone sculptures termed “stelae”). Thus, in the remainder of the section I focus prominently on how these monumental inscriptions became allies of Thompson’s and, especially, Morley’s emerging interpretations.

Enrolling many of these monumental inscriptions into the Maya studies network entailed cutting them from their existing material associations through Morley’s scientific expeditions (interpretable as the unintended scientific consequences of the Mexican Revolution). Through such expeditions Morley came to inhabit the established subject position of the “philosophical traveler” (Cañizares-Esguerra 2001, see Chapter 3), expanding the U.S. American empire through CIW-sponsored historical knowledge production. For the CIW, such efforts served the purpose of establishing the epistemological legitimacy of historical research. Morley’s original charge from the CIW was to complete a chronology and thus develop a “scientific” basis for understanding the ancient Maya (as opposed to the work of institutions who prioritized antiquities collection) (Brunhouse 1971:78). In contrast with more recent depictions of his interpretations as overdetermined by “subjective,” societal influences (e.g. Coe 1999[1992]), Morley thus represented his research into calendrical chronology as the core foundation for a project with implications extending beyond circumscribed questions concerning the meaning of Maya hieroglyphs (though he primarily left these

implications for other scholars to pursue). The key challenge in achieving this goal was conducting research “under most trying, even perilous, conditions” (Morley 1946:ix), and thus overcoming Nature. In Morley’s writing, Nature is less a Newtonian law-driven machine than a Romantic impediment interceding in Man’s access to Historical Knowledge. Nature had to be cut away to reveal History beneath.

Yet, in his practice of imperial science, Morley did not cleanly cut the hieroglyphs out of their tangled sets of environmental associations. While his drawings and photographs (like those of his predecessors, such as Alfred P. Maudslay) aesthetically enrolled architectural inscriptions at sites such as Uaxactun into the hieroglyphs’ scientific network, their emerging meanings were mediated by Morley’s experience of an oppressive Nature. The environment, in other words, shaped the content of Morley’s interpretations of ancient Maya society. While the mountainous terrain and dense tropical forest served as an impediment to Morley’s philosophical travel and historical inference, he re-imagined the same environment as a condition enabling the ancient Maya society’s freedom from societal conflict, like the European wars and U.S.-American racial strife of the early twentieth century. The ancient Mayas’ environment isolated them from the segmentation and conflict that Morley seems to consider an inevitable outcome of transcultural encounters.

Thus, a racial and scientific rhetoric of Maya civilizational purity emerged, for example, in a history of the CIW’s archaeological research that Morley published in 1943. Here, he maintains that the ancient Maya civilization had developed and declined in the absence of significant contact with other cultures. This presumed lack of intercultural influence and diffusion made the Maya area “an ideal laboratory for the

study of human history” (Morley 1943:205). Indeed, Morley’s writing indicates that he imagined the Maya archaeological area as an “ideal laboratory” to perform experiments on the pure structure of humanity’s historical development. In reflecting on the CIW’s decision to pursue research in Mesoamerica, Morley (1943:205) explicitly contrasts the Maya area with other large ancient civilizations:

The Old World seemed to offer few, if any, examples of relatively high, pure cultures. Egypt had been repeatedly overrun from Asia Minor. The great nations of the Tigris and Euphrates Valleys had been as frequently conquered by Egypt. The Greeks and the Romans have built their own brilliant, distinctive civilizations on earlier cultures; while China has been a veritable melting pot of the Mongols, Tatars, Manchus and latterly the Japanese, each of which has modified an earlier mixture, itself produced by still earlier blends. Old World Civilizations everywhere were of composite rather than pure origin.

In order to find the simplest conditions possible for this complex study, the Carnegie Institution turned to one of the indigenous civilizations of the New World, namely that of the Maya, which developed, flourished, and fell in the Yucatan Peninsula, the latter, however, considered a physiographic rather than a political unit. This peninsula, surrounded by water on three sides and separated from the adjacent continental land mass by high mountains, seemed to offer unusually favorable conditions for the study of civilization in a pure state, relatively free from adulteration by alien cultural elements.

Thus, Morley presents the ancient Maya as an isolated cultural exception, fully distinct from the documented ancient civilizations of Europe and Asia. The exceptionality of the Maya reflects their status a unique civilizational resource for generalizations concerning the existence of underlying patterns in world history. Their exceptionality simultaneously makes field research an extreme “trial of strength” (Latour 1987), in which an actor finds itself subject to new mediations that may dissolve its established alliances. Morley parlayed his subjection to the environmental travails of a tropical philosophical expedition into a stabilized inscription of the ancient Maya as a truly

isolated culture. He marshaled the hieroglyphs' environmental emplacement as a complex of allies for his historical narrative of Maya exceptionalism.

Morley thus did not emerge from his trial of strength unfazed. His quest to produce hieroglyphic re-inscriptions entailed cutting the alliances produced by other actors enrolled in the networks relationally defining him, as well as establishing new alliances. While becoming an interpretive resource, environmental conditions also intervened in Morley's alliance with a properly technological blackbox, the camera, short-circuiting its scientific utility:

[Morley] and [his fellow traveler Joseph] Spinden divided the scientific work along lines of mutual interest. Vay [Morley] sought new inscriptions and copied them, while Joe gave his attention to art and architecture, often drawing complete figures that he found. Both men had planned to make a full photographic record of the monuments, but the moist, hot climate played havoc with the film. Hesitating to trust the camera, they drew furiously day after day to preserve a pictorial record of their discoveries (Brunhouse 1971:85).

ANT-contributor and sociologist of technology Madeleine Akrich (1992, 1994) has produced relevant analyses of how the transfer of technologies to new material contexts may result in their failure, revealing that blackboxes such as cameras are not fully stabilized actors. They require articulations with both "social" and "natural" actors to function effectively. The "technical" status of objects such as cameras obscures their roles as politico-epistemological actors. In Akrich's analyses, we see how the "stability" of such objects confers political strength: "[technical objects] may change social relations, but they also stabilize, naturalize, depoliticize, and translate these into other media." Here we have a case in which the camera, a key actor in the production of "mechanical objectivity" (Daston and Galison 2007), or the ostensibly automatic, unmediated re-presentation of the world as a scientific image, becomes unreliable and

causes the human actor to step back into the scene of historical knowledge production. The environmental condition of humidity, thus, emerges as an actor causing Morley and Spinden to produce what would be considered a subjectively compromised record among proponents of early twentieth-century scientific ideologies.

Yet, the moist, hot climate is not solely an antagonist of the researchers, in their quest to cut Nature away and ally the History beneath:

At [the archaeological site of] Naranjo [Morley] accidentally learned how to work with troublesome carvings. One afternoon as he labored over a stela trying to decipher the difficult lines, a rain shower overtook him; but he was so intent on his task that he kept on working, and at once the inscription, clear and clean-cut, seemed to spring up at him. Thereafter, he applied water to bring out the lines of recalcitrant carvings (Brunhouse 1971:80).

Aided by a multitude of allies (the CIW, U.S. government interests, the Mexican Revolution, the United Fruit Company, *chicleros* and their trails,<sup>44</sup> mules, riverboats, a mayor's letter of recommendation, and tropical rain, among others; see Brunhouse 1971), Morley arrived at the sought out monuments, applied water, and furiously drew the inscriptions relevant to him, combating their intrinsic "recalcitrance," overcoming their alterity and making them comprehensible within the regime of imperial science.

In other words, with the aid of actors such as rain showers, Morley articulated with these inscriptions, and this contiguity effected their extrication from the complex chains of alliance standing behind their sense and meaning as inferred in the present (including the "multitude of allies" just mentioned). This is where Morley's science turned towards

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<sup>44</sup> *Chicleros* harvested *chicle*, the latex of the *sapodilla* tree, which was the primary ingredient of chewing gum through much of the twentieth century. Morley relied on *chicleros*' trails to find archaeological sites in Peten, the northeastern department of Guatemala: "If some satisfactory substitute for *chicle* in chewing-gum should be discovered, it would put an immediate end to the *chicle* business (for *chicle* has no other commercial use than in chewing-gum), which in turn, would be followed by the total disappearance of all *chicle* trails and the consequent loss to knowledge of Many important Maya sites" (Morley 1943:214). *Chicle* production was thus something of a blackboxed intermediary supporting Morley's epigraphic innovations.

the negative, as he became a part of a network cutting away the actors that had entailed much (human and nonhuman) labor to enroll (following Latour 1999a; Strathern 1996). Ultimately, the actor-network must cut Morley *himself* from the picture, as if his renderings fully simulated the monuments from a totalized visual perspective and his hand had no part in this re-inscription. Morley could not control this bodily self-denial; he was performed as incidental to his own aesthetic renderings.

In contrast with Knorosov's account described below, many of those cut away in Morley's labor are environmental and sociopolitical actors ostensibly distinct from the architectural inscriptions. The scientific practices producing Morley also brought architectural inscriptions in abandoned former Maya cities out of their given material-semiotic networks and into association with Maya beliefs and cosmological systems. Morley's fieldwork extricated this subset of Maya inscriptions through a complex process of material translations. These translations relied first on his ability to ally a range of human and nonhuman actors in the process of entering Nature and finding History. The process of transforming these inscriptions into documents that could be materially circulated as "data" resulted in cuts that partially externalized their environmental contexts as well as these allies and Morley's own body.

Disciplinary histories attribute the completion of this labor to Thompson, positioning him as the actor who solidified scholarly and popular images of ancient Maya hieroglyphs as esoteric commentary by priests on astronomical and cosmological concerns (Coe 1999[1992]; Stuart 2006a). Thompson's analyses of these matters entailed alliances with diverse Mesoamerican belief-systems. As a technique for bringing the distinction between the Morley-Thompson position and Knorosov's

arguments into high relief, in the following section I situate my explanation of Thompson's methods through contrast with Knorosov's competing research program.

In this section, I have considered how an assemblage performing epigraphic interpretations in the first half of the twentieth century entailed the partial extrication of Maya monumental inscriptions from their material, environmental associations. Through Morley's mediation, however, these environmental associations retained an altered articulation with the Maya hieroglyphs as they became parlayed into interpretations that framed the Maya as a peaceful, isolated civilization. The figuration of the ancient Maya as a unique, secluded culture, helped to lend scientific credibility to the CIW's expansion into anthropology, affirming the ancient Maya as an optimal culture for scientific experimentation upon underlying laws of historical development. The physical difficulties of philosophical travel – expeditions into mountainous and jungly terrain – became affirmations of the Maya as an exceptional culture. Perhaps, like Stephens before them, Morley's and Thompson's experiences of traveling through and working in areas inhabited by contemporary Mayan-language speakers prevented these researchers from cutting Maya subjects themselves from the network of hieroglyphic knowledge production (as far as they could control such acts of constitutive exclusion). As we will see in the next section, such a negation emerged fully upon Knorosov's intervention into Maya hieroglyphs' alliances.

### **The Hieroglyphs' Fall from Eden**

Contemporary epigraphers' reflections on the history of their field often establish a contrast between the kinds of research conducted by Thompson and Knorosov. For example, concluding a summary of Knorosov's advances into decipherment, Houston and colleagues (2001:144) explicitly counterpose the two researchers: "unlike the

ideographic interpretations then championed by J. E. S. Thompson, Knorosov's approach demonstrated a more sophisticated knowledge of orthographic possibilities." Such arguments attribute the sophistication of Knorosov's intervention to his identification of plural types of graphemes and his recognition that Landa's "alphabet" was actually a roughly-constructed syllabary (Houston et al. 2001:144).

In this section, I read Knorosov's identification of graphemes as a process that acutely intensified his predecessors' reductive extraction of hieroglyphs from their given material associations. Knorosov's divergence from Morley and Thompson was not solely a matter of his more apt training in linguistic and orthographic matters, but his performance of an effective blockage, or cut, between the Maya script and Maya humans. In affirming a phonetic basis for hieroglyphs' semantic values, Knorosov became an important mediator reducing the hieroglyphic assemblage and constituting its boundaries in the making of a closed semiotic, aesthetic, and material system. I suspect that his physical distance from the material contexts in which hieroglyphs achieved their original cultural significance facilitated this paradoxically heralded reduction, decipherment.

According to Coe (1999[1992]) and Harri Kettunen (1998a, 1998b), Knorosov's initial work on hieroglyphic decipherment was inspired by his professor Sergei Aleksandrovich Tokarev, who challenged him to refute the German Mayanist Paul Schellhas' (1945) argument that decipherment was "an unsolvable problem." Much of Knorosov's early work dealt with Landa's (1994[c. 1566]) *Relación de las cosas de Yucatan*. In particular, Knorosov's (1955b) doctoral thesis was an annotated translation of the *Relación* from Spanish to Russian. Knorosov also had access to the three then-

known Precolumbian Maya codices, possibly in the form of the black-and-white volume compiled by J. Antonio Villacorta and Carlos A. Villacorta (1933). The Villacorta volume was the first book to assemble the Dresden Codex, the Paris Codex, and the Tro-Cortesianus (now better known as the Madrid Codex) under one cover. Such sources helped shape the conditions whereby Knorosov could stabilize an alliance with the hieroglyphs to produce a series of texts that outlined viable phonetic interpretations considered methodologically innovative and accurate.

These text-artifacts differed in form and content from the monumental inscriptions that had so fascinated Morley and conditioned his version of the peaceful astronomer-priest figuration. While Morley's undergraduate research on the Maya entailed an examination of the Dresden Codex (Brunhouse 1971:30), his influential image of the Maya clearly emerged as a result of his expeditions. The inscriptions that Morley accessed through his imperial "philosophical travel" were monumental, typically immobile objects situated within a dense assemblage of environmental actors. Morley's re-inscriptions were often the first attempts to simulate specific hieroglyphs within a scientific culture. The codices presented to Knorosov, on the other hand, were mobile objects simulated from the original (and originally mobile) screenfold manuscripts. Furthermore, whereas the monumental inscriptions accessed by Morley were produced during the Classic period (250-900 CE), and typically included exact Long Count dates, the codices were probably artifacts of the Late Postclassic period (1250-1520) and entailed distinct functions and aesthetic conventions, including short texts associated with drawn scenes and the use of dates in the 260-day ritual almanac (Vail 2006). The hieroglyphic objects available to Knorosov were already the products of identifiable cuts.

Of particular note is the grayscale status of the Villacorta and Villacorta (1933) copies. The reduction of the multicolored originals to monochrome simulations may have aided Knorosov's attempt to control variables at play in complex signifying objects, reducing them to straightforwardly semantic texts.

While other scholars had proposed the hieroglyphs' status as a coherent writing system (e.g. Thomas 1893; Whorf 1956[1940]), Knorosov's alliance with these manuscripts enacted a much more consequential set of interpretations in this vein. Such a reading extracted the writing system from the cultural contexts that scholars such as Thompson invoked, and abstractly situated it as an object analogous to other known writing systems. The introductory paragraph of Knorosov's (1958) article "The Problem of the Study of the Maya Hieroglyphic Writing System" makes his interest in positioning the Maya inscriptions within the analytical category of "writing" abundantly clear:

Up to the present time, some specialists have held to the view that on the American continent before the European colonization, there was no writing in the true sense of that word. The various systems of writing used by the Indians were regarded as pictographic or ideographic. However, it is now known with certainty that the civilized peoples of ancient America – the Maya, Zapotec, Olmec, and also the Quechua and Aymara – had hieroglyphic writing of the same type as that of the Old World, of China, Egypt, Sumeria, and so forth (Knorosov 1958:284).

In this passage, Knorosov mediates the alliance of three sets of actors within the actor-network performing his research. First, he invokes other hieroglyphic writing systems as allied actors; the semiotic inscriptions of "the Old World, of China, Egypt, Sumeria, and so forth" become assimilated into a "type" that also includes the writing systems of the New World, including that of the Maya. Their common status as "writing" seems to imply that they perform comparable functions for those who use or engage them.

Second, Knorosov implicitly introduces readers to the scholarly antagonists who resisted this attempt at translating Maya hieroglyphs into a “writing system” that mediates their alliance with analogous scripts. In the opening sentence of the article, readers find themselves confronting unnamed “specialists” who deny the status of Maya hieroglyphs as a true expression of writing.

Having encountered the concept of writing as an actor stabilized by Knorosov’s publication, we can begin to trace how Knorosov and specific allied hieroglyphs collaborated to innovate a style of analysis that contributed directly to transforming the semiotic status of the Maya hieroglyphs. In other words, consider Knorosov the mediator between an uncertain object (for Knorosov), hieroglyphs, and a certain object, writing. I bring this translation into high relief through its contrast with Thompson’s position.

Knorosov objected to Thompson’s approach on both theoretical and empirical/aesthetic grounds. In terms of theoretical disposition, Knorosov (1958) argues against Thompson’s claim that the hieroglyphs were comprised primarily of symbols, rebuses, and ideograms. For Thompson, glyph collocations were units of meaning, irreducible to smaller elements. Knorosov’s (1958) article presents the case that Thompson and others of his orientation – such as Schellhas – had not effectively justified their claims that the Maya signs lacked a major phonetic component. He retains the position that individual hieroglyphs could be reduced to smaller units of meaning. Thus, Knorosov attempts to ally hieroglyphs with “writing” through several phonetic demonstrations, including a reading of a glyph collocation as the Yucatec Maya word *tzul*, meaning “dog” (Figure 4-3).

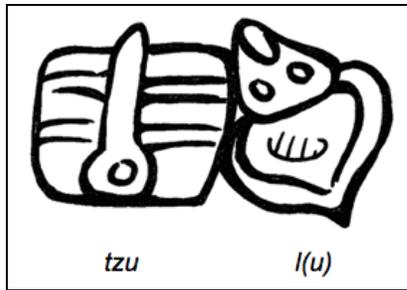


Figure 4-3. Maya glyph collocation interpreted by Knorosov to signify *tzul*, “dog,” with the phonetic values of the component syllabograms specified. Drawing by Lucas Martindale Johnson, after Knorosov (1958:285), reproduced with permission.

Since it serves both as a crux of his divergence from Thompson and a central – indeed, definitive – example of his method of phonetic decipherment, this case merits close scrutiny in determining how Knorosov mediated an alliance between hieroglyphs and the concept of “writing.” Knorosov’s initial discussion of the “dog” hieroglyph appears in a particularly ambitious passage. Here, he both defines “decipherment” – in terms of interpreting specific sets of hieroglyphs – and explicitly positions his previous works as the premier examples of such decipherment:

Although Maya writing has been studied for about a hundred years, it was deciphered only in 1951 (Knorosov 1952, 1955a, 1955[c], 1956). The term “to decipher” is often misunderstood. Some authors consider as decipherment any plausible interpretation of unknown signs. Others consider as decipherment the full finished translation of texts written in an unknown script. Both of these are wrong. By various indirect clues, we can determine the meaning of separate hieroglyphs. If one can determine the meaning of a hieroglyph it does not follow from this that the hieroglyph can be read. For instance, the representation of a dog in Maya manuscripts is accompanied by a hieroglyph made out of 2 symbols [see Figure 4-3]. There is every reason to believe that this hieroglyph transmits the word “dog.” But, in the Maya language there are several synonyms all meaning “dog.” Moreover, it is unknown how far the ancient pronunciation differs from the modern. It is also possible that many words of the ancient language are not in the modern language. The determination of the meaning of a hieroglyph gives one the possibility of indicating the meaning of the word written by that hieroglyph, though it remains unknown exactly what word it is, and how it is to be pronounced. If the meaning of a row of hieroglyphs is correctly determined, it becomes possible to try a reading.

In contrast to the determination of the meaning of separate hieroglyphs by indirect clues, decipherment is the beginning of an exact phonetic reading of words written in hieroglyphic form. As a result of decipherment, the study of texts becomes a branch of philology (Knorosov 1958:286-7).

Knorosov's definition of decipherment sheds some light on his mediation of hieroglyphs' alliances. In terms of the signs' material-semiotic boundaries, Knorosov accepts the position that the objects classified as hieroglyphs are themselves delineable semantic units. In other words, he does not challenge the presumption that hieroglyphs can be understood as signs that maintain relatively stable meanings or references in different contexts. The individual hieroglyphs are bounded, coherent, meaningful objects. His position clearly had predecessors, including the work of Léon Louis Lucien Prunol de Rosny (2001[1876]), who analyzed Landa's manuscript and distinguished between what he called figurative signs, ideographic signs, specific determinants, and polyphonic signs. Knorosov also accepted the stance – held by Cyrus Thomas (1893) and Benjamin Lee Whorf (1956[1940]) before him – that individual hieroglyphs could be reduced to semantic subunits, including phonemic components amounting to one or more morphemes.

In the example of the “dog” hieroglyph (Figure 4-3), Knorosov distinguishes two main signs, reading a sign on the left as *tz(u)* and a sign on the right as *l(u)* (Figure 4-4). Both of the signs in this glyph collocation for “dog,” thus, have a phonetic basis. Knorosov deduced these phonetic readings by applying the phonetic values identified in Landa's flawed “alphabet” and analysis of day and month names to the captions accompanying images of identifiable figures, such as animals, in the Maya codices. Knorosov's basic knowledge of Mayan languages also aided in his identification of morphemes. He notes, for example, that the *tz(u)* sign that initiates the *tzul* hieroglyph

is also employed at the end of the glyph collocation for *cutz* (meaning “turkey”) in the codices. Thus, by repositioning the hieroglyphs as a closed system with some known phonetic values, Knorosov was able to deduce a series of “decipherments,” or precise phonetic readings, through acts of phonetic substitution or “cross-readings.” In Knorosov’s formulation, which was later institutionalized in the practice of decipherment (with some notable counterexamples), the writing system relied on an orthographic practice of “synharmony.” With the assumption of “synharmony,” Knorosov designated scribes’ apparent habit of ending words with consonant-vowel syllables that shared a vowel with the antecedent syllable. The repeated, second vowel would presumably have been omitted in spoken discourse. Thus, the epigraphers could cut away the second vowel in an inscription that seems to read *tzul(u)*, or *tzulu*, reading the sign as *tzul*, a morpheme for “dog.”

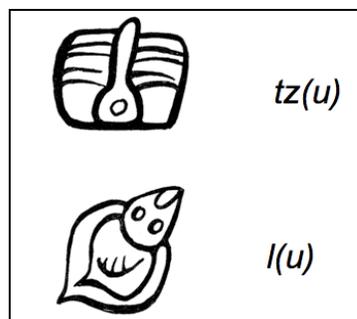


Figure 4-4. The two components of the *tzul* glyph collocation identified by Knorosov. The signs are listed with their phonetic equivalents. Drawings by Lucas Martindale Johnson, after Knorosov (1958:285), reproduced with permission.

Unlike Thompson’s system, described below, Knorosov’s merely requires linguistic knowledge and the ability to identify associated images such as turkeys and dogs in the codices as background for evaluating the hieroglyphs to achieve legitimate interpretations, or decipherments. It requires little non-linguistic cultural knowledge of Maya symbolic systems, cosmological practices, or history itself. The actor-network

performing Knorosov-as-decipherer thus cuts the hieroglyphic system entirely out of its previously associated cultural contexts, rendering hieroglyphs into purified laboratory objects, and initiating a formalist research regime. Such formalism is fairly apparent in Knorosov's most explicit definition of decipherment, presented in the passage from the second paragraph quoted above. There, he reveals his position that determining the "meaning" of a hieroglyph or set of hieroglyphs (by which he means determining the equivalent morphemes in a different language) is insufficient to count as decipherment. As demonstrated above, Knorosov's sense of "decipherment" entails the phonetic rendering of a hieroglyph or set of hieroglyphs as a syntactically coherent series of morphemes spoken in the scribes' language. Implicit in this definition is the sense that the hieroglyphs function as abstract, reproducible units with presumably stable effects and meanings (such as *tzul* and *cutz*) for the community of users, what Latour (1987) – referring to scientific inscriptions such as graphs – calls "immutable mobiles."

As becomes apparent in subsequent passages of the article, Knorosov meant to contrast his definition of decipherment explicitly against Thompson's interpretive techniques. Knorosov did not invoke the hieroglyph representing a Mayan language morpheme for "dog" as an arbitrary example. He counterposed his phonetic reading of the dog hieroglyph (Figure 4-3) against the non-phonetic, symbolic reading posited by Thompson:

Eric Thompson (1950: 78) interprets, for instance, the hieroglyph of the dog in the following fashion. The hieroglyph consists of 2 signs; the first sign depicts the rib of an animal and appears to be the symbol of the dog, while the second sign is the symbol of death. The combination of the symbol of the dog and the symbol of death is explained by the fact that the dog accompanies the shades of the dead to the world beyond the grave. In the opinion of Thompson, such an interpretation is a true decipherment. It is clear that such a "decipherment" cannot help us make sense out of any

other hieroglyph. Thompson (1954: 167) says as much: “the decipherment of new glyphs does not appreciably simplify the task of tackling the remainder, as in a crossword puzzle or in a writing which employs an alphabet” (Knorosov 1958:288).

Knorosov’s objectives in analyzing the dog hieroglyph seem entirely disparate from those reflected in the Thompson passage that he referenced. For Knorosov, the purpose of analyzing the hieroglyph for dog was to produce the exact phonetic rendering of signs in the hieroglyphs, as summarized above. His analysis had little if anything to do with Maya cultural practices or meanings; as he stated, his aims in decipherment were philological (and implicitly not anthropological). Thompson’s passage, on the other hand, invoked the hieroglyph for dog represented in the Dresden Codex as one piece of evidence – one actor – in a much more general analysis of a component of the Maya astrological-cosmological system, the 260-day ritual almanac comprised of day signs, which Thompson understood as a tool for prognostication.

The relational properties of the dog hieroglyph for Knorosov thus differ dramatically from the properties of the “same” hieroglyph for Thompson, because the former allies the graphic inscriptions of the symbol with graphemes represented in sources such as Landa (1994[c. 1566]) whereas the latter invokes the Dresden Codex’s dog hieroglyph (in a single sentence) as an ally in an analysis of the meaning of the tenth day name of the tzolk’in. Thompson’s position invoked the dog hieroglyph as part of an attempt to connect specific day names with specific Maya gods. His concern was not with what Knorosov would call the “meaning” of the hieroglyph. Whereas Knorosov presented a formalist exercise that took Maya hieroglyphs as a closed system, Thompson’s project was a further-reaching attempt to integrate disparate evidence from ethnographic, ethnohistoric, and iconographic studies of both Maya and central Mexican

symbolism. The disparate actor-networks that constituted Knorosov and Thompson positioned hieroglyphs in contradictory relational webs.

While Thompson resembled Knorosov in defining hieroglyphs' boundaries and properties as relatively fixed and stable, he invoked a wide range of non-hieroglyph actors in his interpretations. Knorosov, on the other hand, innovated a formalist exercise that defined certain types of objects as the only legitimate actors in his interpretive network: hieroglyphs, Maya-language morphemes, and Maya-language phonemes (inferred from the hieroglyphic graphemes). Thus, while the hieroglyphs' boundaries and properties were not fixed when the copies of the codices and Landa's *Relación* arrived on Knorosov's desk, his alliance attempted to stabilize the hieroglyphs into clear matters of fact with a constrained number of actors. This alliance repositioned the study of Maya hieroglyphs from an anthropological pursuit to a philological or epigraphic pursuit, abstracting the Maya hieroglyphs from their preceding assemblage comprised of environmental actors, anthropologized artifacts, and contemporary Mayan-language-speaking informants. It further positioned them within a comparative framework in which they could be understood as instantiations of an ostensibly transcultural human practice, "writing."

The reduction of heterogeneous hieroglyphic texts to a singular object – a "writing system" – enabled Knorosov to ignore the historical settings in which particular hieroglyphs were produced. The implicit justification for this reduction turned on Knorosov's assumptions concerning the diachronic uniformity of the speech referenced by the inscriptions. Having established his divergence from Thompson in deciphering

signs such as the “dog” hieroglyph, Knorosov depicted his efforts at decipherment as the reason that he accepted a premise of diachronic uniformity:

The decipherment has given us the opportunity to begin the study of the language of the hieroglyphic texts. There is every reason to believe that the texts are written in the language of the time when hieroglyphic writing was invented (first centuries B.C.). This ancient language became literary and sacred. The priests wrote it until the Spanish conquest, while the spoken language in 1500 years had strongly changed in all respects (Knorosov 1958:289).

Knorosov proceeded to support this assertion for diachronic uniformity, in a rather convoluted way, by insisting that the phonetic rendering of signs used during the early colonial period differed markedly from coeval Maya linguistic forms. This discord seems to emerge as the sole basis of Knorosov’s claim to diachronic uniformity, a very bold (and, by recent standards, inaccurate)<sup>45</sup> assertion given his exclusive focus on texts conventionally attributed to the colonial period and the Late Postclassic period of 1250-1520 CE (Vail 2006).

The real subjects who produced the Maya texts, however, had to be blackboxed, deferred, or cut away in order to render the existing corpus of hieroglyphic inscriptions a singular set of decipherable forms, a “writing system.” While Knorosov resembled other scholars in lacking direct access to the historical contexts of production for these text-artifacts, his geopolitical location in the Soviet Union also distanced him from the material and geographical locations where hieroglyphs had been produced, locations that Morley and Thompson knew through their research in Mesoamerica. Further, his formulation of the argument for phoneticism in the 1950s preceded Proskouriakoff’s (1960) argument for historical content in the hieroglyphs. He could not work fully

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<sup>45</sup> More recent studies have turned to the geographical and historical variation in the Maya script(s) and languages employed in the Preclassic, Classic, and Postclassic periods (Vail and Macri 2000).

outside a rather Thompsonian interpretive regime that assumed the texts' content to be primarily cosmological, a problem complicated by the high degree of astrological content in the codices (compared to monumental and ceramic inscriptions from the Classic period). Thus, the actual people who produced the codices were reduced to a rather spectral figure of the indigenous priest, the embodiment of a "literary and sacred" tradition inaccessible to the masses of the past and the researchers of the present. The identification of a closed, internally self-referential linguistic system inscribed by elite scribes wielding a sacred language led Knorosov to ignore contemporary Maya-speakers or relevant comparisons with other Mesoamerican cultures.

Unlike Thompson, who seems to have generalized wildly in books such as *Maya Hieroglyphic Writing: An Introduction* (1950), where he compares aesthetic forms across Mesoamerican cultural boundaries and historical eras, Knorosov turned the study of hieroglyphic texts into a formalist exercise with very limited scope. His concept of decipherment did not lend itself to producing knowledge of those who inscribed the hieroglyphs. Rather, it turned these subjects into priestly automata ritually reproducing archaist inscriptions that instantiated the phonological forms predominant at the presumed origin of Maya culture. To reduce hieroglyphs to writing, Knorosov had to blackbox or negate the very people who produced them. Knorosov's position did not instantaneously marshal a network of human and nonhuman allies strong enough to overwhelm Thompson's approach, which the latter defended vehemently (Thompson 1959).

By the mid-1970s, the tides turned and the *Mesa Redonda* collaborators began to position Knorosov's formalist phoneticism as a central method for decipherment.

Indeed, Knorosov's textual laboratory became one fundamental basis of the workshop-based decipherment and pedagogy discussed in the following two chapters. In the meantime, it is necessary to outline the emergence of structural analysis and Proskouriakoff's historical method as additional knowledge practices requisite to hieroglyphic decipherment, which began to achieve its contemporary momentum through the early *Mesas Redondas*.

### **Historical Content, Structural Analysis, and the *Mesas Redondas***

Thus far in this chapter I have emphasized the central importance of Knorosov's efforts to apprehend the role of phoneticism in Maya hieroglyphic writing. This section traces the emergence of a set of concepts allied with Knorosov's phoneticism in defining the standards for contemporary Maya hieroglyphic decipherment. I concentrate on accounts of two processes: Proskouriakoff's case for historical content in the hieroglyphs, and the rise of structural analysis. I also briefly discuss how these epistemic techniques became articulated co-actors in the case for Maya hieroglyphs as a fully functional writing system articulated and defended at the early Palenque *Mesas Redondas* (mentioned in Chapter 1). This addendum to my critical treatment of Knorosov's innovation effectively sets up the ethnographic analyses of workshops in the following two chapters. Establishing the hieroglyphs' phoneticism and historical content as matters of fact not subject to challenge on empirical or epistemological grounds was prerequisite for the constitution of workshop publics that have helped to confer decipherment's public and scholarly legitimacy. Moreover, Linda Schele's version of structural analysis has traditionally functioned as the central technique for enrolling workshop attendees as participants in – and not merely witnesses of – the labor of

decipherment. Thus, it is crucially important to review the emergence and imbrication of these processes.

Two years after the appearance of Knorosov's (1958) programmatic case for phoneticism, Proskouriakoff (1960) published an equally consequential article in the same venue, the flagship U.S.-American archaeology journal *American Antiquity*. Proskouriakoff's argument dramatically contradicted the reigning approach to understanding the content of Maya hieroglyphic writing. In stark and explicit opposition to Thompson's (1971[1950]) claims for the pervasively cosmological referents of Classic Maya dates, Proskouriakoff made the case that dates at the site of Piedras Negras appeared in patterns indicating that they designated historical events in the lives of Classic Maya rulers. While Proskouriakoff prominently acknowledged that overcoming difficulties in hieroglyphic interpretation would require more complete decipherment of hieroglyphic content, she made a clear case that patterned dates conformed to expectations for the recording of prominent events in rulers' life histories, such as birth, accession to rule, and death.

In his popular history of the field, Michael Coe (1999[1992]:171) ebulliently claims that Proskouriakoff's interpretation "revolutionized" Maya research, and that she had "cut the Gordian knot of Maya epigraphy, and opened a world of dynastic rivalry, royal marriages, taking of captives, and all the other elite doings which have held the attention of kingdoms around the world since the remotest antiquity." Coe (1999[1992]:171) continues: "The Maya had become real human beings." Such bold rhetoric is suggestive of the regime of historical value that would come to dominate the field of Maya hieroglyphic studies in the era following Knorosov's and Proskouriakoff's

achievements. The ancient Maya were to be valued and understood not as a societal exception revealing “pure,” isolated cultural development, as in Morley’s logic, but as the Americas’ instantiation of a complex civilization structured by the actions of elites.

Knorosov’s case for phoneticism and Proskouriakoff’s argument for historical content emerged more or less simultaneously with Heinrich Berlin’s (1958, 1963) identification of emblem glyphs designating specific sites. These three arguments amounted to significant evidence that the script described earthly, rather than cosmological and astronomical, affairs. Coe’s (1999[1992]) rhetoric is just one popular and recent example of how Mayanists have assessed the importance of these collective shifts in the development of hieroglyphic decipherment. Describing the importance of Berlin’s methodology in their edited compilation of contributions to decipherment, Houston and colleagues (2001:299) state, “It was novel conceptually...and represented, along with Proskouriakoff’s findings, the beginnings of the historicist paradigm in Maya research.” They proceed to strengthen this sense of a major transition in the first sentences of their introduction to Proskouriakoff (1960):

Seldom does a single article change a discipline. But Tatiana Proskouriakoff published one such paper in 1960.... This article led to a sea-change in Maya studies by demonstrating in a clear, meticulous way that the sculptured texts of the Classic period recorded, among other things, the dates and deeds of rulers (Houston et al. 2001:312).

Likewise, in an obituary of Proskouriakoff, Ian Graham (1990:8), the director of a Harvard-sponsored project to compile a Corpus of Maya Hieroglyphic Inscriptions, describes the influence of her 1960 *American Antiquity* article in these terms:

At one blow, this short paper freed the study of Maya writing from lengthy stagnation, which largely was due to the generally accepted idea that monumental inscriptions contained exclusively calendrical and astronomical matter. Proskouriakoff’s demonstration that they contained instead records

of the principal events in the lives of historical personages was truly liberating.

That Proskouriakoff's article signaled an important – if only arguably “liberating” – change of course for Maya hieroglyphic interpretation seems to occupy a clear position in the culture of hieroglyphic studies.

As I adopt a relationist ontology in this study, I cannot fully concur with Houston and colleagues' (2001:312) seeming assessment that Proskouriakoff's innovation was *intrinsically* consequential, that it was a “single article [that] change[d] a discipline” as they put it. But it is apparent that the “historical hypothesis” (Proskouriakoff 1960:454) has a prominent place in Maya studies' collective disciplinary memory. I suggest that this memory has emerged in concert with assessments of subsequent developments, particularly the process of public hieroglyphic decipherment innovated at the 1970s *Mesas Redondas*. While Proskouriakoff did not participate in this subsequent shift towards a phonetic approach, and even had strong reservations concerning the possibility of fully translating hieroglyphic “texts” (Joyce 1993), her work preconditioned these developments and subsequently benefited from them, becoming one “obligatory passage point” (Callon 1986) for assembling the ancient Maya inscriptions as a coherent set of legible texts.

This particular obligatory passage point is of additional relevance to my study because it established the semiological and epistemological contours of “history” for Maya hieroglyphic studies. In this view, “history” is equated with events in the lives of elite human actors, that “world of dynastic rivalry, royal marriages, taking of captives, and...other elite doings” (Coe 1999[1992]:171). This history or – perhaps better-written – “History” emerges through positivist and literalist reading practices requiring the

troubled assumption that hieroglyphs were intended as accounts of events preserved for posterity.<sup>46</sup>

The epistemological legitimacy and interpretive security that has been conferred retrospectively on the positivist positions of Proskouriakoff, Knorosov, and Berlin has also hinged on the establishment of structural analysis as a valuable interpretive and pedagogical technique for hieroglyphic interpretation. While Proskouriakoff's innovations were not based in the method of structural analysis, her work influenced the establishment of this method as a dominant set of tools for hieroglyphic interpretation. Epigrapher David H. Kelley (1962a:323), an advocate of structural analysis, opened an article extending Proskouriakoff's method to inscriptions from the site of Quiriguá with the following statement: "The discovery by Heinrich Berlin (1958) that certain glyphs are directly associated with, and appear to refer to, specific sites, and the evidence offered by Tatiana Proskouriakoff (1960) for the existence of a dynastic sequence at Piedras Negras must inevitably lead to a full reappraisal of most of the Mayan inscriptions." Despite Proskouriakoff's strong reticence to adopt the phonetic approach, the argument that hieroglyphs dealt significantly with immanent, worldly matters helped to incite desires for more thorough understanding of all components of the inscriptions, rather than solely the components with clearly astronomical referents.

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<sup>46</sup> Such a disposition remained largely uncontested in twentieth century historical knowledge production until the 1970s, when postcolonial critiques inspired by decolonization movements in South Asia and Africa challenged the literary "canon" and positivist historical interpretation (Pratt 1992b). Among the many relevant postcolonialist challenges to positivist accounts of the past, one particularly notable claim is Dipesh Chakrabarty's (2000) refusal to exclude "religious" nonhumans from historical accounting practices. After all, nonhuman spirits are pervasive actors in most peoples' – including contemporary Maya peoples' (e.g. Watanabe 1992) – accounts of their worlds and, as such, have deeply material and human consequences. Accepting such claims might force a total reassessment of the conventional disciplinary narrative that opposes Morley's and Thompson's "cosmological" explanans to more recent scholars' "historical" explanans. In other words, I am asking what a post-secular account of ancient Maya history might look like.

Kelley (1976:249-81) has offered the most thorough treatment of how structural analysis enables a systematic interpretation of hieroglyphic inscriptions. While he provides neither a clear definition of structural analysis nor an account of its origins and development, Kelley does briefly review the history of Mayanists' applications of this method. He traces this history to Cyrus Thomas's (1882) analysis of tablets from Palenque, thus revealing that the method of structural analysis employed in hieroglyphic interpretation was not grounded primarily (if at all) in mid-twentieth century structural linguistics and anthropology. In contrast with Kelley's prioritization of Thomas's (1882) work, Houston and colleagues (2001:168) attribute the "methodological breakthrough" of structural analysis to Hermann Beyer, who used the method to examine inscriptions from the site of Chichen Itza. Houston and colleagues (2001:168) converge with Kelley (1976) in noting that Berlin's (1958, 1963) research on Palenque inscriptions and emblem glyphs built on Beyer's structural analysis. In any case, despite the method's ambiguous genealogy, it has become a vital tool for epigraphers.

Structural analysis functions through the identification of patterns of repetition in texts. The structural analyst seeks to identify similarly or equivalently structured units of syntax. The identification of patterned hieroglyphic syntax enables epigraphers to infer the semantic values of incompletely understood or partially destroyed passages through the use of passages with known semantic values as models. In an example demonstrating the utility of structural analysis, Kelley (1976:252-6) carefully examines parallel passages from the Dresden Codex, revealing that the structural method makes it possible to infer both the grammatical content and the semantic value of undeciphered or damaged hieroglyphs. The method capitalizes on the prevalence of parallelism in

hieroglyphic writing in order to apprehend scriptural variation and synonymy (see Figure 4-2).

In this sense, structural analysis functions as a “scale-making practice” (Tsing 2005) subordinating the aesthetic and scriptural specificity of hieroglyphic signs and passages to their semantic functions as parts of a presumably more intricate whole, be it specific texts or the graphical and grammatical totality of all known inscriptions. Of special relevance to my study, this scale-making practice has conventionally entailed the graphic re-transcription of paired hieroglyphic columns into “linear” (left-to-right) syntactic or discursive units. In the vocabulary that I elaborate here (see Chapter 2), this re-transcription is an act of *cutting*, or the translation of actors (hieroglyphs) from one relational field into another, intervening into and altering their properties and values. The aesthetic particularity of the hieroglyphs in their original contexts is elided in order to elucidate their operation at a higher, more abstract scale. This labor of translation, extrication, and abstraction has become one technique for constituting an enrolled public through pedagogical practices. In my Chapter 5 analysis of workshops on hieroglyphic writing, I examine how Schele’s version of structural analysis becomes, quite literally, a material practice of cutting that makes hieroglyphs knowable within ideologies of text and language that are sensible to contemporary publics.

Proskouriakoff’s claim for the existence of historical content in the hieroglyphs and the method of structural analysis employed by Kelley, among others, effectively preconditioned the rise of contemporary epigraphic knowledge practices. However, these interpretive and methodological innovations did not intrinsically constitute a significant break from the preceding epistemic field dominated by the cosmological

fixations of scholars such as Morley and Thompson. While there is a strong argument to be made that the cosmological orientation remains vibrant today, it is certainly clear that the Palenque *Mesas Redondas* that begin in 1973 marked a strengthening set of alliances among claims for phoneticism, the historical hypothesis, and the method of structural analysis. Taken together, these three actors strongly defined the assemblage governing the reduction of hieroglyphs to writing.

While the inaugural *Mesa Redonda* concentrated scholarly attention on Palenque in particular, the consequences of the collaboration transcended that specific locality and history. The workshop became a regular event, attracting a wide range of scholars, including epigraphers, archaeologists, iconographers, art historians, ethnographers, and linguists. Further, the series of Palenque *Mesas Redondas* set in motion a transformative collaboration among Schele, art historian Merle Greene Robertson, archaeologist Peter Mathews (a student of Kelley's at the time), and linguistic anthropologist Floyd Lounsbury. These scholars' interests and fields of expertise differed markedly, and they combined each other's complementary knowledges to answer established questions surrounding the names and biographies of ancient Maya kings encoded in Maya hieroglyphs. An equally diverse nonacademic public, ranging from local tour guides and amateur archaeologists to enthusiasts who traveled to the meetings from the United States, witnessed this breakthrough. In subsequent years, some of these scholars developed workshops in Austin (the Maya Meetings) and Antigua (the Maya Linguistics Workshops), which continued to attract heterogeneous participatory communities: the former composed of enthusiasts drawn to the exotic

allure of the ancient Maya and the latter composed of pan-Maya intellectuals and other Maya. I explore these subsequent workshops series in the following two chapters.

The innovations of the first *Mesa Redonda* were rooted in the interpretations and methods of Berlin, Knorosov, and Proskouriakoff, and they provided a tangible, publicly witnessed legitimation of the arguments for phoneticism and historical context. At the same time, it is interesting to note that Berlin and Proskouriakoff maintained conservative positions on some of the methodology and specific readings of Schele and her colleagues. In a private letter to Schele, Proskouriakoff attributed her reservations to contribute to this rising field to her own lack of knowledge concerning Maya languages.<sup>47</sup> Yet, Schele, Mathews, and Lounsbury's phonetic decipherment of Palenque rulers' names at the first *Mesa Redonda* became a requisite mediator permitting greater attachment between Knorosov's argument that the hieroglyphs were largely phonetic and Proskouriakoff's argument that the hieroglyphs contained historical content, thus strengthening them both. Despite any reservations about the style in which phonetic-historical decipherment was taking place, such a linkage was highly anticipated and desired among advocates of the phonetic and historical positions. For example, Berlin (1965:339) put it this way:

One of the salient features of the present historical approach is the effort to isolate proper names. This effort is of noble ancestry. The correct reading of personal names was the final breakthrough in deciphering Egyptian, ancient Persian cuneiform, and other hieroglyphic script. Hence there is a good chance that it will work once more in the field of Maya hieroglyphs.

The successful isolation of proper names effectively initiated a strong shift away from Morley's (1946:262) position that "it is even probable that the name-glyphs of specific men and women were never recorded upon the Maya monuments." The *Mesa*

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<sup>47</sup> Proskouriakoff to Schele, 28 January 1977.

*Redonda* colleagues innovated a novel image of the ancient Maya which seems somewhat reactionary in its radical reversal to the Morley-Thompson position. So, which local associations enabled the hieroglyphs' new actor-network to solidify? Addressing such a problem must entail entering into the first workshop and tracing how different actors became effectively allied.

In the published proceedings of the 1993 *Mesas Redondas*, Merle Greene Robertson (1996) reflects on two decades of public workshops at Palenque. Contrasting the 1973 and the 1993 Palenque *Mesas Redondas*, Robertson emphasizes the events' increasing scale. While 36 people attended the first Palenque *Mesa Redonda*, 425 registered and attended the 1993 workshop. This sense of a scalar shift in the scholarly and public attention paid to the workshops grounds her presentation of early workshops in a clearly nostalgic style. Robertson describes her family's role in organizing the first meeting and the subsequent mood of collective dwelling. She writes:

Our living room was rearranged for the meetings. One wall was left bare to be used as the screen for the projector. We sat on our red chairs, the beds, or the floor. A coffee pot was always on the stove, and whenever anyone wanted a cup of coffee or a Coke they just went and helped themselves.... Some of the meetings were held in the thatch roof Morales *champa*, where howler monkeys regularly put their "two cents worth." So many Palencaños wanted to come and find out more about the Palenque ruins at their doorstep, that we held regular meetings from 9AM until 12:30PM, and again from 8PM until 10PM. A Spanish only session from 5PM until 6PM was held for all of the village inhabitants. The Palenque guides attended every meeting (Robertson 1996:i-ii).

Such passages celebrate the early meetings as warmly situated in an inclusive place. The material details – the red chairs, brewing coffee, and howler monkeys inserting their own voices – invite the reader to share in nostalgia for the local, the familiar, and the organic. Robertson reveals her primary concern with the early *Mesas Redondas* as

*social* spaces, collaborative and open gatherings. Though not originally designed as a public forum, the first *Mesa Redonda* proved flexible and fluid, open to the “village inhabitants” curiosity.

Robertson’s narrative continues by describing how knowledge of the meeting could not be contained. While she notes at the beginning of the reflection that few Mexican scholars could attend due to short notice and Christmas plans, Robertson goes on to express that many Mexican university students quickly learned of the Roundtable and began to arrive. She emphasizes the priority placed on discussion at the meetings. The academic papers presented become secondary to the elaborate, collective, responses. “Everything was informal” (Robertson 1996:ii).

Flexible, informal, collaborative, and homey, while simultaneously an uncontainable presence that reaches out and calls its participants, Robertson emphasizes the new possibilities for understanding history enabled by the open gathering. In fact, she lends particular emphasis to a set of intersubjective exchanges that would significantly alter the Mayanist Precolumbian discourse. For example, she describes Mayanist scholars Schele, Mathews, and Jeffrey Miller’s collective effort to produce a list of the Palenque kings. Robertson writes,

White paper was taped on the wall, glyphs were drawn on it by Linda who noted the kings. Then came the chore of naming them. We were all thinking in terms of English names when [local guide] Moises Morales spoke up and said “This is a Spanish speaking country, we should give them their names in Spanish.”

Then Fray Facunda Ramirez from Tumbala said “But the Maya people here did not speak Spanish; they spoke Chol. We should give them Chol names” (Robertson 1996:ii).

Morales’ and Ramirez’s amateur interventions into the material practice of collectively determining the names of the Palenque kings momentarily destabilizes epigraphic

knowledge production. Their articulations, as reinscribed in Robertson's account, upset the naturalized scholarly conventions that define the epigraphic community. They reveal that hieroglyphic knowledge has very specific and parochial parameters that exclude sets of interested actors. Their interruption is overtly political, but immediately accepted. This is a critical moment in the reformation of epigraphic knowledge-practices. Simultaneously it reaffirms the necessity of extra-disciplinary collaboration and articulates a new set of connections between epigraphy and Maya linguistics.

However, other accounts of the first *Mesa Redonda* workshop are not so smooth. Both Coe's (1999[1992]) well-crafted narrative description, and Moises Morales' memory of the Roundtable, as conveyed to me in an interview, draw attention to the darker, exclusive dimensions of these early meetings.<sup>48</sup> Coe, for example, emphasizes how Cold War tensions between U.S. epigraphers and Marxist Mexican archaeologists underlay the absence of many Mexican scholars from the first meeting (rather than Christmas-time conflicts, as Robertson states unproblematically).

A more compelling contradiction emerges by juxtaposing Robertson's and Moises Morales' descriptions of the decision to use Chol names for Precolumbian kings. According to Morales, on the last scheduled day of the meeting, he was checking to make sure that dinner was being prepared in the kitchen. When he returned to the dining room, he noticed that several large sheets of paper with glyphs, dates, and locations had been posted on the wall (Figure 4-5). The sheets included important facts, namely the identification of kings with their names in English: "Lord Sun Shield, Mr. Pyramid, and Lord Headache/Toothache/Bundle." Shortly thereafter, perhaps at dinner, Morales requested to address the group. Coe refused him, but linguist Floyd

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<sup>48</sup> Morales, M. Personal Interview. 8 July 2006.

Lounsbury spoke up and told him to be brief. Morales asked why they did not honor the people who made the glyphs by writing the names in a Mayan language rather than in English.



Figure 4-5. Poster presenting the phonetic decipherment of “Pacal” (later changed to “Pakal”), hung on the wall at the first Palenque Round Table meeting. Access to the posters for the purposes of reproduction was provided by Alfonso Morales.

Neither Robertson’s nor Morales’ narrative provides a complete, disinterested account of the shift in scholarly convention at the meetings. The unmistakable contradictions between these two memories, principally *who* suggested that they name the kings in Chol rather than English and how readily this suggestion became incorporated into scholarly practice, hint at the possibility that the first *Mesa Redonda*, and subsequent events were not as seamless and communitarian as they appear in

later reflections. Regardless of the style of collaboration, the important point here is that the successful articulation of a Proskouriakoff-style historical “methodology” with phonetic glosses of the hieroglyphs’ meanings was facilitated through engagement with subjects who were not institutionally affiliated contributors to Maya studies. Explained differently, whereas Morley’s public engagement involved the dissemination of his own knowledge to public audiences, the public engagement of the *Mesa Redonda* group entailed an expansion of the network of human actors participating in decipherment to non-professionals. In this sense, the complex of actors assembled at the *Mesa Redonda* instigated a new ethic of hieroglyphic studies as a necessarily public science. Through the next two chapters, I explore the consequences of this ethic as enacted in series of hieroglyphic workshops.

### **Conclusions**

This chapter has drawn on the postcolonial science studies perspective elaborated in the preceding two chapters to reconceptualize a key event in the decipherment of Maya hieroglyphs: the rise of phoneticism. I prefaced my Latourian analysis of Knorosov’s argument for phoneticism with a summary of the contemporary scholarly consensus surrounding the structure and content of Maya hieroglyphs. I then proceeded to examine how the work of Morley and Thompson in the twentieth century established the conceptual and ideological framework within which the debate over the status of hieroglyphs as writing unfolded. Whereas Morley’s and Thompson’s scientific culture relied on a conceptualization of hieroglyphs as objects acting within specific environmental and cultural contexts, Knorosov abstracted the hieroglyphs entirely from their past and present associations, re-situating them in an abstract laboratory-like framework that limited hieroglyphs’ actions to alliances amongst themselves.

My primary analysis in this chapter suggests that hieroglyphic research rooted in Knorosov's formalist approach unknowingly inherits a highly abstract positioning of Maya aesthetics on a distinctively contemporary and distinctively non-Maya epistemological foundation and language ideology. Knorosov's turn in validating the cultural achievements of the ancient Maya paradoxically deferred the ancient Maya and modeled their writing on contemporary analogies.

Moreover, I have traced how Knorosov's phoneticism articulated with Proskouriakoff's historical hypothesis and the method of structural analysis advocated by scholars such as Kelley. These interpretive actors achieved more thorough relational integration through their material-semiotic imbrication in the context of the *Mesas Redondas*, workshops that helped to further legitimize the premises of decipherment, in part through the constitution of a public of witnesses. The scholarly practice of assembling a public of witnesses around the labor of decipherment became even more integral for the effective operation of the Austin Maya Meetings and the epigraphy sessions of the Antigua Linguistics workshops, the topics discussed in the following two chapters.

The reduction of Maya hieroglyphs to writing became an established position through the staging of public workshops. There is no return to Morley and Thompson's Eden. But it has become ethically imperative to save the hieroglyphs from this imperial, scientific logic of historical appropriation, to return them to their makers. This is a rather impure repatriation, and one that – due to the ostensible absence of these makers – may require the supplement of a re-imagined method for summoning the no-longer-human in deference to contemporary Maya constituted as the descendents of those

nobles and scribes. I touch on this ethical-scientific position in the following four empirical chapters, and elaborate it explicitly in the Conclusion, Chapter 9.

## CHAPTER 5 THE MAYA MEETINGS AS PUBLIC SCIENCE

The workshop rapport cultivated from the late-1970s through the mid-1990s by Mayanist Linda Schele, among others, entailed clear imaginations of a public participating in the creation of Maya history. As I describe in Chapter 1, this recent style of valuing public participation in hieroglyphic research began in 1973 when Mayanist art historian Merle Greene Robertson founded the Palenque *Mesa Redonda* series. The kernels of collaboration planted at the first Round Table and its successors in the 1970s matured into the more thoroughgoing public engagement of the Maya Meetings. The Maya Meetings entangled research, public outreach, and pedagogy, proving successful enough to spawn similar workshops across the United States. This chapter principally demonstrates how we can understand the Maya Meetings as an expression of what Brian Wynne (2005:68; see also Chapter 2) calls “public science,” or a set of institutionalized knowledge practices in which “imaginings of the public world, however that is construed, can be taken as integral to scientific knowledge-generation, not simply as afterthoughts.”

I trace how a small group of innovative scholars in Maya hieroglyphic studies imagined a public as integral to their production of historical knowledge, and created conditions for continuously engaging this public. I pay particular attention to the visual and material practices of witnessing and replicating innovative acts of Maya hieroglyphic decipherment, as the constitutive performance of a contemporary community and a historical imaginary. This work illustrates how historical practices co-produce understandings of the present and the past. Further, the Maya Meetings have functioned partly within the imperial construction of Latin American – especially

Mesoamerican – indigenous cultures as *National Geographic*-style objects of consumption.

The coproduction of the experts and public succeeded most clearly when Schele and other workshop leaders fully deferred how their very presence created the conditions – the stage – necessary for this association. Schele and the workshop leaders assembled and inhabited an environment that substituted the actual presence of ancient Maya scribes and lords for their own mediating subject positions.

I have divided this chapter into three sections. The first provides a basic overview of the inception of the “Workshop on Maya Hieroglyphic Writing,” which was later redesignated the “Maya Meetings.” The following two sections elaborate my analyses of Schele’s workshop lectures and the participants’ material practices of hieroglyphic analysis. I present Schele’s lectures as a site in which the audience was constituted as a public participating in the act of “scientific witnessing” (Haraway 1997; Shapin and Schaffer 1985; see Chapters 2-3). I emphasize how the style of scientific witnessing enacted in the Maya Meetings differed somewhat from that of more conventional scientific spaces, such as laboratories. In addition to witnessing the hieroglyph experts’ equivalent of “nature,” the past, the public was also called to witness actual acts of decipherment. They witnessed the process of decipherment itself, and acquired the tools to imagine themselves as the discoverers. Such practices of witnessing fashioned the public attendees’ dispositions to the workshop leaders and hieroglyphs and provided the conditions in which they could come to perceive themselves as active, if marginal, participants in the process of decipherment. I attend specifically to this transformation, which I call “staged discovery,” in the following section, which traces

how this public was consolidated through material practices that conditioned the collective's firm adherence to the notion that hieroglyphic decipherment provided the tools to directly access ancient Maya voices.

I present the public engagement of the UT-Austin workshops as a necessary site for the production of what Stephen Houston (2000), in a discussion of epigraphy's epistemology, terms "consensus" surrounding interpretations of Maya hieroglyphs. But I diverge somewhat from Houston's concept of "consensus" (which is limited to assent among human experts) by examining hieroglyphic studies as a public science with its material basis in an assemblage of disparate human and nonhuman actors. Thus, I am broadening the range of actors "taken into account" (Latour 2004a) in determining the value and consequentiality of contemporary historical narratives to the material tools of structural analysis and members of the public with differential degrees of interest and expertise in decipherment. Ultimately I trace how this consensus is actually a complex coordination, an actor-network (Latour 2005b), of expert knowledges, popular desires, hieroglyphic forms, and aesthetic and technological practices.

### **The Inception of the Workshops on Maya Hieroglyphic Writing**

Here I provides a requisite overview of the formation of the Maya hieroglyphic workshops held at the University of Texas at Austin from the Fall of 1977 to the present. I begin to describe how the workshops emerged and became a significant zone of regulated contact among Mayanist experts and publics. I begin to emphasize the significant labor that went into the production of the workshops as laboratory-like sites through which the voices of hieroglyphs could be accessed and represented. This series of events also deeply shaped Schele's career, opening up institutional channels that facilitated her complete transition to Maya studies. I cast this transition as the

material and institutional backdrop enabling Schele to become a “spokesperson” (Latour 2004a:62-70) for the hieroglyphs. With this term, Latour breaks down the entire spectrum of representational competencies presumably inhabited by scientists, ranging from their transposition of facts’ “objective” voices to the articulation of their own “subjective” opinions. By assimilating these presumably opposed positions into a single role, Latour emphasizes that all voices emerge out of intricate arrangements of actors and represent through direct partial connections. Here I emphasize how Schele’s role as a spokesperson for hieroglyphic facts was grounded in a complex material-semiotic assemblage. Following from Steven Shapin and Simon Schaffer’s (1985:22) elucidation of the structures of scientific witnessing (to which I turn more directly in the next section), I illustrate how “the experimental production of matters of fact involved an immense amount of labour.” As I describe and further elaborate in the following section, this labor entailed the production of both alliances and cuts, particularly the regulation of a properly assenting audience.

In Fall 1977, Nancy Troike, a Mixtec specialist and research affiliate in the Institute of Latin American Studies at the University of Texas at Austin, invited Linda Schele to present a visiting lecture at the Institute.<sup>49</sup> Schele was then an assistant professor of art at the University of South Alabama in the process of shifting careers from studio art to Mayanist art history. During this same period Troike had facilitated a workshop run by linguistic historian Frances Karttunen that introduced students and members of the general public to Classical Nahuatl writing.<sup>50</sup> Apparently Schele’s initial lecture was

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<sup>49</sup> Troike, N. Personal Interview. 11 July 2007.

<sup>50</sup> Troike, N. Personal Interview. 11 July 2007. Troike to Schele, 6 February 1978.

successful, as Troike invited her to come back to campus that semester to conduct a workshop on Maya hieroglyphic writing, modeled on the Nahuatl sessions.

Schele returned to Austin during the second half of the Fall 1977 semester and Troike assembled a body of interested students and members of the public to attend this first workshop, held on the upper floor of the library that houses UT-Austin's Nettie Lee Benson Latin American Collection.<sup>51</sup> From the very beginning, Troike had a strong hand in determining the workshops' format.<sup>52</sup> This first workshop was a watershed event that capitalized on the progress in Maya hieroglyphic decipherment incited and sustained by the Palenque *Mesa Redonda* series as well as closed gatherings among Schele and her fellow hieroglyph experts, particularly invite-only "mini-conferences" that occurred at the Dumbarton Oaks Research Library in Washington D.C. (mentioned in Coe 2000). This first lecture attracted diverse participants. In a letter to Schele after the first workshop, Troike wrote, "Have done up the basic statistics on who attended & can send to you if you want. Some people were from wild depts.: Elec. Engineering, Microbiology, Law School, Graduate School of Business, etc."<sup>53</sup>

The attendance of such wide-ranging participants inspired Troike and Schele to hold another workshop during the Spring 1978 semester. Propelled by Troike's administrative skills and Schele's compelling performances (described further below), the workshops acquired status as a recurring event at UT. Troike's work allying William Glade, the director of the Institute of Latin American Studies, was of crucial importance to the establishment of the workshops' recurring status.<sup>54</sup> There is evidence of Troike's

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<sup>51</sup> Troike, N. Personal Interview. 11 July 2007.

<sup>52</sup> Troike, N. Personal Interview. 11 July 2007. Troike to Schele, 16-17 March 1978.

<sup>53</sup> Troike to Schele, 15 November 1977.

<sup>54</sup> Troike to Schele, 15 November 1977, 5/7/9/17 April 1978.

success in producing Glade as a proponent and supporter of the event in a letter that he sent to Schele after the Spring 1978 workshop, in which he states that, “Reaction to these events has been mixed: ranging from wholehearted approval to enthusiastic accolades! Under the circumstances, we can only conclude that the whole effort was a resounding success, a success attributable to your outstanding capability in this fascinating area of concern.”<sup>55</sup>

At this early stage, the workshop had already become a powerful site through which the world of Mesoamerican studies was transforming. Schele came to articulate connections and establish working relations with a range of scholars outside the relatively narrow world of the Palenque *Mesa Redonda* group. The workshops inspired inquiries from Mesoamericanists such as art historian Cecelia Klein, who – at the urging of Troike – invited Schele “to give one of [her] now famous workshops on Maya hieroglyphs out here at U.C.L.A. next year.”<sup>56</sup> Likewise, in early April 1978 Mexican archaeologist Jaime Litvak King contacted Schele to request that she hold a workshop – or, as Schele put it, “*the workshop*” (emphasis added) – at the Mexican Universidad Nacional Autónoma de México (UNAM) in Mexico City.<sup>57</sup> In addition to these requests to conduct workshops at institutions other than UT-Austin, Schele was also articulating and nourishing alliances with scholars who could offer different kinds of expertise to hieroglyph studies.

For example, in the letter to Mexican iconographer Marta Foncerrada de Molina concerning the planning of the UNAM workshop, Schele emphasized the importance of

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<sup>55</sup> Glade to Schele, 18 April 1978.

<sup>56</sup> Klein to Schele, 3 February 1977.

<sup>57</sup> Schele to Nick Hopkins and Kathryn Josserand, 10 April 1978. Schele to Marta Foncerrada de Molina, 11 April 1978.

Mesoamericanist linguists Nicholas Hopkins' and Kathryn Josserand's attendance. Hopkins and Josserand were established Mayan language experts, and their presence and support thus helped to confer legitimacy on a relatively novel set of knowledge-practices. Schele was also establishing a division of labor in the presentation of the material, and requested that Hopkins and Josserand offer preparatory lectures at the UNAM on Maya culture and the calendar. In this sense, the workshops served not simply to stage public engagement, as I describe further below, but also to facilitate encounters between scholars with disparate forms of expertise. In the same letter where Schele described her expectations for their workshop roles to Hopkins and Josserand, she also requested specific examples of Chol terms and phrases that might aid in hieroglyphic decipherment.<sup>58</sup> From the beginning, Schele's workshops had the dual-purpose of public engagement and scholarly collaboration. In a letter discussing arrangements for the UNAM workshop, Hopkins and Josserand note that the Spring 1978 workshop in Austin "was a truly great experience for us, as neither of us had realized things were quite as far along as they are."<sup>59</sup> These workshops were thus not just sites of public engagement, but also locales for the production of a collective of assenting scholarly peers.

In the act of extending the workshops' alliances, Schele was also articulating cuts, defining which kinds of actors could contribute to the constitution of hieroglyphic interpretations, even as an ostensibly passive audience. Schele's correspondence indicates that she considered the proposed spin-off workshop in Mexico a risk that needed to be carefully managed, as members of the Mexican archaeological

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<sup>58</sup> Schele to Hopkins and Josserand, 10 April 1978.

<sup>59</sup> Hopkins and Josserand to Schele, 26 March 1978.

establishment, particularly Alberto Ruz Lhuillier, had opposed some of the premises and results of decipherment. Ruz Lhuillier and his allies voiced their opinions strongly at early Palenque *Mesas Redondas*, in scholarly publications, and in the public press (see Coe 1999[1992]:207-10; e.g. Ruz Lhuillier 1975, 1977). In the letter to Foncerrada de Molina concerning the organization of the UNAM workshop, Schele explicitly voiced her opinion that the oppositional positions of some Mexican scholars crossed boundaries of scholarly propriety:

I wish to repeat that I do not want to get involved in a war with Ruz and his friends. For this reason I do not want to be in any newspapers or filmed interviews. I do not want to have to face hostile people in the audience, especially anthropology students who hate "gringos." I would not limit attendance [sic] as long as everyone comes to listen. My presentation does not have to be accepted as the truth, but I would at least like to present it to an unbiased and non-hostile audience. I know I am probably overemphasizing my position on this issue, but we have gotten very hostile and generally unfair press in Mexico over the last few years. I cannot ethically or emotionally become involved in a fight in the public press. We are perfectly willing to answer criticism, but only in legitimate scholarly places. I understand that you are in the middle by having friends on both sides and I do not want to put pressure on you. It might be best if the workshop remain a private thing in UNAM and with the Hopkins group.<sup>60</sup>

This particular tension was sparked by contradictions between epigraphic and archaeological data (an issue discussed further in Chapter 8) and exacerbated by a contradiction between a Mexican sense of national proprietorship over historical materials and what might conservatively be described as a North American sense of universal patrimony. The complexities of this tension thus exceed the purpose of this section, and merit more comprehensive treatment than I can offer within my defined scope. But for the present purposes, Schele's reticence to open a workshop in Mexico to the general public suggests how the workshops were deeply political sites of

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<sup>60</sup> Schele to Foncerrada de Molina, 11 April 1978.

postcolonial conflict over how and by whom the past could be known. In other words, from the outset, the workshops functioned as sites where a conceivably assenting public could gather around Maya hieroglyphs and entertain North American scholars' transformations of these objects into texts. With the aid of Troike and allies such as Hopkins and Josserand, Schele had begun to constitute and regulate an audience predisposed to accepting her propositions.

The act of producing a regulated public of assenting witnessing was most manageable in the UT-Austin setting, under the hand of Troike, who continued to administer the workshops through 1990.<sup>61</sup> Troike crafted the workshop's material foundation. She arranged to use university carousel slide projectors, compiled lists of potential attendees, contacted these attendees, submitted advertisements to relevant newsletters, arranged for handouts to be copied, requested that publishing houses provide copies of their volumes for book displays, and supervised graduate student volunteers.<sup>62</sup> This entailed a large amount of administrative labor. For example, in the days leading up to the Spring 1978 workshop, the administration turned into a fulltime job for Troike.<sup>63</sup> Such an investment of Troike's time in organizing a workshop outside standard academic channels paid off in terms of the quantity of early attendees, which may have numbered over one hundred.<sup>64</sup>

Both Troike's labor and the success of the first year of workshops were integral to Schele's career change. On April 19, 1978, Kenneth W. Prescott, the chair of the UT-

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<sup>61</sup> Troike, N. Personal Interview. 11 July 2007.

<sup>62</sup> Troike, N. Personal Interview. 11 July 2007. See also multiple letters from Troike to Schele, e.g. 3 October 1977, 20 December 1977, 30 January 1978, 6 February 1978, 9 February 1978, 21 February 1978, 16-17 March 1978, 5/7/9/17 April 1978, 20 April 1978a, 20 April 1978b, 22 February 1979.

<sup>63</sup> Troike to Schele, 21 February 1978.

<sup>64</sup> Schele to Lounsbury and Mathews, 19 March 1979.

Austin Department of Art, provided Schele with a job offer, a lectureship for the Fall 1978 semester to be jointly sponsored by the Department of Art and the Institute of Latin American Studies. Schele responded with an acceptance two days later.<sup>65</sup> According to Schele, Troike “arranged” for this visiting lectureship.<sup>66</sup> Schele’s teaching responsibilities included a graduate level course on hieroglyphs in the Institute of Latin American Studies and an undergraduate level on Maya art history in the art department.<sup>67</sup> Schele considered her first epigraphy course an opportunity to demonstrate the centrality of hieroglyphs to reforming contemporary knowledge of the ancient Maya. In her letter accepting this position, Schele emphasized this point to Prescott: “Recent findings in the study of hieroglyphic writing have provided a framework of dynastic history which is allowing the study of architecture, city planning, stylistic development and iconography to advance at a very rapid pace.” She goes on to emphasize how such approaches to decipherment had effectively rendered art historical approaches epistemologically null.

The course fit with Schele’s developing ethic of openness, providing the conditions for non-experts to make legitimate contributions to decipherment. Some of the students, such as Barbara MacLeod, who was a linguistics graduate student at the time (on the way to becoming an epigrapher), made original hieroglyphic readings within the context of the class.<sup>68</sup> Schele’s presence on campus also affected the attendance at the third workshop, which took place in Spring 1979. In a letter to Floyd Lounsbury and Peter Mathews, Schele stated of this workshop that “96 people attended or registered;

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<sup>65</sup> Prescott to Schele, 19 April 1978. Schele to Prescott, 21, April 1978.

<sup>66</sup> Schele to George Stuart and Troike, 22 April 1978.

<sup>67</sup> Schele to G. Stuart and Troike, 22 April 1978. Schele to Peter Mathews, 9 September 1978.

<sup>68</sup> Schele to Prescott, 21 April 1978. Schele to Floyd Lounsbury, Peter Mathews, and Dave [Kelley?], 14 October 1978.

this is a drop from last year but the quality of the audience was generally much higher since at least 20 people were refugees from the Fall glyph class.”<sup>69</sup> Many of these participants began to return annually, becoming fixtures of the workshop scene. In a letter concerning plans for the third workshop (Spring 1979), Troike noted that most of the early registrants had already attended at least one of the two preceding workshops.<sup>70</sup>

As I detail in the following section, this public did not show up every year merely to learn an ancient script. The Maya hieroglyphs differed fundamentally from Latin or Egyptian. This difference did not reside solely in their linguistic or aesthetic form. It resided in the history of the script’s decipherment. The public showed up to learn how to read Maya hieroglyphs from Linda Schele, whose legendary intrusion into the disciplinary culture of Maya studies at the 1973 Palenque *Mesa Redonda* helped dramatically increase the rate of events coming to be understood as the decipherment of Maya hieroglyphs. Interviewees broadly cited the personal charisma, inclusiveness, and strong stage presence of Schele, a gruff and un-self-censored artist, as factors that held the audience in place. So, the power of the workshops to attract a public resided not just in the hieroglyphs as aesthetically appealing objects, or in Schele as an entertainer, but in the conjunction between these objects and this person, creating totally new affective and imaginative possibilities, to which I turn in the following section.

### **The Workshop as a Site of Scientific Witnessing**

This section turns to the structure of public engagement crafted in Schele’s weekend workshops. While I have emphasized some aspects of the material

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<sup>69</sup> Schele to Lounsbury and Mathews, 19 March 1979.

<sup>70</sup> Troike to Schele, 2 March 1979.

constitution of Schele's status as a spokesperson representing hieroglyphic voices, this argument further requires attention to how the logic of hieroglyphic fact production hinged centrally on the constitution of a public of "witnesses" (Haraway 1997; Shapin and Schaffer 1985; see Chapter 3). Schele's lectures constituted and maintained a public of witnesses who affirmed the power of epigraphy to transparently represent the ancient Maya by virtue of how they repeatedly assembled around Schele and the hieroglyphs' performative articulation.

The early workshop sessions lasted for a weekend, and were comprised mainly of lectures by Schele. Troike heavily influenced the structure and format of the presentation.<sup>71</sup> During the Saturday lectures Schele presented the basic information required to draw the attendees into epigraphy's disciplinary assemblage. She would review knowledge of ancient Maya culture, discuss Landa's "alphabet," introduce Knorosov's phonetic approach, review the calendrical system, mention the increasing importance of linguistic methods, and describe the syntax and the structure of hieroglyphic clauses (e.g. Schele 1980, 1982b), summarizing the interpretive innovations spawned by the *Mesa Redonda* collaborations. The Sunday lecture built on this foundation, reviewing a specific set of hieroglyphic texts. For the first decade of the weekend workshop, Schele focused exclusively on monumental inscriptions from Palenque. This emphasis stemmed from how the 1970s advances in hieroglyphic decipherment had occurred through the analysis of Palenque's hieroglyphs. As one long-time workshop attendee and volunteer put it, "Palenque was a known thing" that had "basic histories."<sup>72</sup> The Palenque material provided a stable, matter-of-fact

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<sup>71</sup> Troike to Schele, 16 March 1978, 22/29 March 1978.

<sup>72</sup> Manning-Schwartz, L. Personal Interview. 18 March 2008.

foundation for summarizing the power of epigraphy to transform knowledge of the ancient Maya.

The basic form of Schele's public engagement thus emerged through her weekend seminars. Schele had a powerful capacity to maintain the attention of her audience through two full days of lectures. Her performances were animated and captivating. She dynamically traversed the stage, intra-acting with transparencies of hieroglyphs, and deftly drawing the signs on overhead transparencies. Though the early workshops all concentrated on relatively well-known texts, this did not detract from the audience's collective sense that they were witnessing breakthroughs in decipherment. Indeed, Schele and Troike crafted the workshop as a site in which the attendees could feel that they were becoming privy to cutting-edge work that had transformed an entire field of knowledge. Troike clearly understood this sense as a primary factor that motivated the attendees' participation in the workshop. For example, Troike made a statement to this effect in a letter to Schele discussing possible improvements to the workshop: "When you said, at the beginning of the 2<sup>nd</sup> session...that there was nothing new in what you had said that morning, it might have been a bit disheartening to someone sitting on the return portion of a \$250 plane ticket. And actually it isn't true. I think maybe you should move the defense of the new presentation to an up-front position early in the first session."<sup>73</sup> By "the new presentation," Troike was referring to the *Mesa Redonda* group's insights into dynastic lineages and the fully syntactic quality of the script. These developing innovations clearly drew in the attendees, constituting them as privileged witnesses to epigraphic breakthroughs.

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<sup>73</sup> Troike to Schele, 16 March 1978.

Such an ethos further cultivated the sense that further breakthroughs might happen at any moment. While Schele produced workbooks to supplement her lectures, she frequently presented new readings in the workshop. As an Austin resident with graduate training in linguistics who regularly attended the workshops described it, “The ink was barely dry on the workbooks and things changed. [She stood] on stage, slapping transparencies down and presenting contradictory readings.”<sup>74</sup> Though such disconnects between the information in the workbooks and that presented on stage consternated much of the audience, it also cultivated their sense of being privileged as the first to witness innovations in interpretations of Maya hieroglyphs. The messiness of correspondence between the workbooks and the seminar also rendered the contingent and transforming nature of hieroglyphic interpretations apparent. It left open the possibility that attendees might make valid contributions to the field, without diminishing the certainty ascribed to the readings Schele presented on stage.

Thus, attendees did not feel that they were merely witnessing interpretations or speculations about the past. They were witnessing original discoveries. Attendee Sandra Noble, who later became the director of a Mesoamerican Research Foundation (the Foundation for Advancement of Mesoamerican Studies, Inc.), termed the transparencies “proofs.”<sup>75</sup> By this she meant that the readings presented on stage conferred tangible authorization to the veracity of decipherments printed in the workbooks and elsewhere. Through presenting these “proofs,” Schele was developing her public style, which conveyed a sense of epistemic certainty grounded in revelatory discovery. This sense of wonder and innovation would come to mark her rhetorical

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<sup>74</sup> Wakefield, W. Personal Interview. 17 July 2007.

<sup>75</sup> Noble, S. Personal Interview. 18 April 2007.

style in later popular publications (Freidel et al. 1993; Schele and Freidel 1990), which included narratives that could inspire a vividly imagined ancient Maya world. While the weekend seminars focused much more thoroughly on the structure of the hieroglyphs than the historical narratives that the transformation of inscriptions into texts effected, it seems clear that she was developing rhetorical techniques for constructing and maintaining a captivated audience. As MacLeod put it, Schele would “take people on a great adventure” into “a well-informed fantasy world.”<sup>76</sup>

This was not just a *rhetorical* adventure. It was a *demonstrative* journey into the matters of fact that Schele represented through her direct material intervention into the signs on stage. In these weekend workshops hieroglyphs simulated with the aid of overhead- and slide-projectors became the scientific instruments with which she could demonstrate the self-evidence of historical facts and thus produce public assent. The simulations of Maya hieroglyphs became Schele's equivalent of Boyle's air-pump, a material technology that stood in for nature and produced matters of fact in a manner that “crucially depended upon its physical integrity, or, more precisely, upon collective agreement that it was air-tight for all practical purposes” (Shapin and Schaffer 1985:29). The workshops were the “public offering” of a newly scientific set of methods for understanding the Maya past, experimental spaces in which the audience could become vested in knowing epigraphically. Thompson's age of interpretive persuasion, the Mayanists' equivalent of Hobbes' philosophical approach to truth (Shapin and Schaffer 1985), had given way to a culture of demonstration.

Constituting the audience as a set of witnesses who could attest to the air-tight status of decipherments required a high quality of projected images, the epigraphic

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<sup>76</sup> MacLeod. B. Personal Interview. 23 July 2007.

equivalent to solidly produced scientific technologies. Troike's accounts of her administrative labor in letters to Schele attest to the careful attention paid to constructing a performative, or perhaps experimental, arrangement that would enable Schele to neutrally represent the hieroglyphs themselves. In a letter describing preparations for the Spring 1978 workshop, for example, Troike detailed how she had rented two eight foot square screens and an electric pointer and was planning to acquire a microphone and to test the auditorium where the workshop would be held.<sup>77</sup> Troike was a key ally in the production of a controlled experimental space, as she understood the distinctive imaging requirements for effective pedagogical techniques in art and art history. In a passage from a letter describing the requirements for a class that she taught on her own specialty, Precolumbian Mixtec painted screenfold books, Troike noted that she was actively using four slide projectors and anticipated possibly using six in some classes.<sup>78</sup> The ability to project and compare multiple images was an important material technology for demonstrating epigraphic matters of fact.

While she never matched the proliferation of projectors employed by Troike, Schele did sometimes simultaneously employ two carousel slide projectors in her weekend workshop.<sup>79</sup> Thus the existing technical and discursive conditions of the slide lecture functioned as a sociomaterial basis for producing public assent around epigraphic innovations. In this respect, the weekend workshops bridged a demonstrative ethos of scientific culture with pedagogical techniques typical of art historians. The use of multiple slide projectors was a technique likely common among art historians by the beginning of the twentieth century (Nelson 2000:429). In his

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<sup>77</sup> Troike to Schele, 21 February 1978.

<sup>78</sup> Troike to Schele, 30 January 1978.

<sup>79</sup> Keeler, P. Personal Interview. 9 July 2007. Schele, D. Personal Interview. 12 July 2007.

analysis of the slide lecture in art history, Robert S. Nelson (2000) employs philosopher Michel Serres' (1987) and Latour's (1993:88, 89) concepts to characterize the slide lecture's "performative triangle" of speaker-image-audience: "If slides are tangible enough objects, and speakers and audiences readily recognizable subjects, their mutual interaction yields what Bruno Latour calls 'quasi-objects' and 'quasi-subjects.' Together they create narratives and social bonds and transform shadows into art, monument, symbolic capital, or disciplinary data" (Nelson 2000:415). Schele's slides intra-acted with her to produce audience members as witnesses to the constitution of a new "ancient Maya culture."

These blackboxed technologies had to be carefully regulated through Troike's arrangements in order to establish the faultless security of Schele's performances, as sites of scientific fact production that required the mutual operation of imaging technologies and the corollary assent of the audience. Schele indicated the importance of these technical specifications in the letter detailing arrangements for the UNAM workshop: "I need two carousel projectors. I will have my own trays, but you will have to provide the projectors. The image must be quite large so that people can see the detail. The light level of the room should be bright enough so that people can see to take notes."<sup>80</sup> The early staging of Maya hieroglyphic studies' public engagement clearly entailed these specific technical requirements. Schele was not a lone actor on the stage. Engaging the audience required careful intra-action with projected slides of hieroglyphs and imagery writ large on the flat screen. The act of demonstration entailed transforming these intricate, mysterious, often three-dimensional, otherworldly objects into stably known, two-dimensional signs.

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<sup>80</sup> Schele to Foncerranda de Molina, 11 April 1978.

As I will further detail in the section on the “long workshops” below, as well as in Chapters 7 and 8, the simulation of Maya hieroglyphs actively transforms these objects, rendering them differently intelligible. In the context of Schele’s performances on stage, the hieroglyphs were translated into signs mediated by the screen. Schele controlled the projection of images, channeling the audience members’ sensoria, fixing their gaze upon the multiply manipulated images projected behind her body. The audience did not see hieroglyphs in their original contexts, on monumental walls, ceramic vessels, or fold-out manuscripts. They saw signs projected onto a screen that Schele controlled, fixed images mediated by the slide projector and manipulable images mediated by the overhead projector.

The kinds of productive interventions into the simulated hieroglyphs that Schele could perform are evidenced in the transcripts of the workshops that were produced beginning in 1989. Many of the figures printed in the transcripts evidence Schele’s interventions, consisting most prominently of sign elements identified and annotated with their interpreted phonetic or logographic meanings. This act of adding a hieroglyphic translation to the actual simulation of the pertinent sign established Schele as a mediator, a spokesperson who could tease Maya voices out of an opaque aesthetic alterity.

It is important that Schele rendered this alterity intelligible to the audience of witnesses via mediation by the screen, a technology so familiarly commonplace that its betrayals to the simulated matter can be easily ignored. In an analysis of how “the human” can no longer be understood as separable from an “external” mechanistic cosmos, human geographer Nigel Thrift (2005:234) notes that “Screens are one of the

constants of everyday life, communicating, informing, entertaining, affecting life, simply being there providing ground. Their grip is constant and unremitting.” The projection of hieroglyphic signs onto a screen elides their complexities and material specificities. The screen was clearly a more familiar and sensible object to these witnesses than the carved wall-panels that the images simulated. The screen reinstated the hieroglyphs’ collective status as a set of laboratory objects purified from their material contexts through the technologies of decipherment originally crafted by Knorosov (see Chapter 4). Hieroglyphs cut from their original contexts and simulated on the screen functioned as a template for experts such as Schele to transform the unknown into the known, the hieroglyph into its phonetic or orthographic translation. This technology of projection further opened up the possibility that an indefinite number of subjects could collectively witness the articulation of matter-of-fact semiotic equivalences between ancient Maya stone inscriptions and late modern text.

In his history of the slide lecture, Nelson (2000) further attends to the coordination between images and speech in the construction of art historical arguments. Nelson begins with an analysis of a passage from *The Heidi Chronicles*, a play by Wendy Wasserstein (1991), depicting an art history professor lecturing about slide images. The linguistic features of the art historian’s monologue identified by Nelson provide a useful set of tools for analyzing Schele’s weekend lectures. First, Nelson calls attention to the character’s frequent use of deictics (or context-dependent words such as *here*, *I*, or *today*) that establish the speaker’s and audience’s shared space and time. With this common foundation articulated through deixis, the lecturer then employs a mix of the first-person singular (establishing her personal interaction with the audience) and the

first-person plural (unifying herself with the audience and becoming their representative). In addition to the use of deictics and alternation between singular and plural subjects, the art historian also frequently shifts linguistic codes, oscillating between describing images and directly addressing the audience. Finally, and perhaps most importantly, Nelson (2000:417) points out the following: “Throughout, the monologues assume that painting is the subject of discussion. However, the stage directions clearly state that Heidi is lecturing not before paintings but slides. ...the slide is taken not as shadow, projected photograph, or copy of an original, but as the object itself.”

The workshop transcripts provide evidence of how Schele employed the (admittedly rather generic) rhetorical strategies identified by Nelson in order to simultaneously construct arguments about hieroglyphic materials and the audience as a public affirming these arguments. In particular, Schele’s abundant use of deictics facilitates the illusion that simulated objects detached from their material contexts and transformed into manipulable, resizable images are the original Maya hieroglyphs themselves. Here is a typical passage from the 1990 workshop, in which Schele dissects a series of hieroglyphs from different archaeological sites, pastiched onto the same transparency (with deictics italicized):

Now another set that shows up is *here* in Fig. 25. *Here’s* an **ahau** [“lord” title] **(A)**. *Here’s* the **wa** to tell you to read this **ahau**.

Another place where **wa** showed up was identified back in the late 1970’s when I recognized that *this* combination of **wa** plus *this* sign appeared on positional verbs. *This* particular verb right *here* is the Late Classic or the Classic version. Now remember the torso sitting down? Well, *this* is *that* glyph. *Here’s* the leg right *there* and the torso squished down into a square cartouche. *This* is what *it* became. **(F)** I was *here* in 1978 and said, “Look, *this* thing has got to be a **wa** and a **ni** put together to be the inflection for *this* verb which means to sit down.” Barbara MacLeod stood up and said,

“Hey, in Cholan languages the way you say the past tense of to sit down is **wan**.” So *this* became *then* one of the principle ways of inflecting the verb and *it* is a **wa** plus a **ni** (Schele 1990:27, boldface for Maya words in original, italics added).

In this 12-sentence, 172-word passage, Schele employs the adverbial deictics “here,” “there,” and “then” eight times, the impersonal pronominal deictics “this,” “that,” and “it” 10 times, and the personal pronominal deictic “I” twice. In each of the three sentences that mark Schele’s introduction of the transparency, she employs the word “here.” This rhetorical strategy serves to orient the audience to the new transparency within the flow of Schele’s narrative. She is shifting from teaching the audience how to recognize hieroglyphic variations (“substitutions”) of the term *ahau* (alternatively spelled *ajaw*; see Figure 4-2), meaning “lord,” to the identification of the term *wan*, meaning “to sit down.” She transitions by constructing the phonemic sign *wa* as a mediator, a syllabogram used in inscriptions of both *ahau* and *wan*. At the same time, the adverbial deictics situate Schele as the mediator demonstrating how to read and understand images on the screen. “Here” and “there” hail audience members, pointing them towards specific components of the images, and compelling them to dissect and apprehend the images as does Schele.

The adverbial deictics further enable Schele to elide the multiple forms of artifice necessary to simulate the hieroglyphs. She does not say “Here’s an image rendered by shining light onto a screen through a transparency produced by photocopying a line drawing that I made to replicate a hieroglyphic “*ahau*” sign imaged in a photograph of a Copan stela.” These (and other) stages of technological and bodily mediation all disappear, reduced to a direct four-word statement printed with a supplementary reference to the figure in the transcript: “Here’s an **ahau (A)**” (Schele 1990:27, boldface

in original). Such statements conveniently shortcut the complex labor of translation required to simulate a stone carving in Honduras as a projected line drawing in Texas.

Thus, a multitude of actors swarm hidden inside the blackboxes of the “*ahau*” and the “*wan*” to which Schele points in her lecture. Indeed, the “*ahau*” and “*wan*” blackboxes contain earlier instantiations of Schele herself. She lives not just as the body on stage, but in the images on the screen. Thus, the simple sentence “Here’s an *ahau*” also means “here’s a sign that I helped identify as an *ahau*.” Schele is not cryptic about her own participation in the contemporary regime of hieroglyphic interpretation. In their intra-actions on stage, the images draw out some of these earlier instantiations of Schele’s self: “I was here in 1978 and said, ‘look this thing has got to be a **wa** and a **ni** put together to be the inflection for this verb which means to sit down’” (Schele 1990:27). In this sentence, the first deictic, “here,” performs two interconnected forms of semantic work. First, it situates Schele as one of the experts who innovated the interpretations expounded in her lectures, reaffirming her authority. Second, it emplaces this innovation at the Maya workshops, a space that Schele shared with her audience in that moment. Thus, she constructs the audience members as public witnesses affirming an interpretation that she advocates, and also indicates that they may witness and participate in similar interpretive advances.

In addition to instantiating more general rhetorical and imaging practices characteristic of the slide lecture, such as the abundant use of deictics, this passage also evidences the more particular point that the lectures were not purely one-directional pedagogical exercises. While there were certainly limits to the degree of interaction that could take place between Schele and audience members, the

established professional participants in hieroglyphic decipherment sometimes spoke up during Schele's weekend workshops. At the specific moment recorded in this passage, Schele recounts how MacLeod had intervened, mobilizing her linguistic expertise to facilitate a particular decipherment. As the workshops matured through the 1980s, a growing collective of experts could make such contributions. Indeed, at the outset of her 1990 weekend workshop, Schele (1990:2) made a specific point to emphasize that her lectures were supposed to entail interactions with other experts:

One other thing that I wanted to explain to everybody is that we have, mostly here sitting in this corner, a whole bunch of people who know this stuff as well as I do and several more who know more about this than I do. What I'm hoping for is that they will embarrass me only slightly, and most of all, they will talk to me from the audience. If any of you have questions or comments or observations, interact. This is supposed to be an interaction, so let's get started on Tikal.

Some such interventions have become powerful exemplars of public collaboration in the collective memory of Maya studies.

Perhaps the most apparent example of such a publicly staged discovery entailed Kathryn Josserand's and Nick Hopkins' identification of signs written by Diego de Landa's (1994[c. 1566]) informant Gaspar Antonio Xiu.<sup>81</sup> As the story goes, Schele was describing Landa's attempt to create a hieroglyphic alphabet during the weekend workshop. As she was reviewing the signs that Xiu provided, Josserand and Hopkins stood up and pointed out that Xiu's phrase "I do not wish" could have been a response to Landa's request that Xiu write a phrase of his own choosing in the Maya script. They took this as an act of personal, and perhaps anticolonial, resistance facilitated by Landa's inability to read the script. Such exemplary interventions by audience members (in this case, fellow Mayanist experts) undergirded the sense that the Schele's

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<sup>81</sup> Manning-Shwartz, L. Personal Interview. 18 March 2008.

workshops were open events where important discoveries could take place. In this case, the discovery positioned Josserand and Hopkins as spokespersons for more than particular hieroglyphic signs. They became spokespersons for a particular historical subject, Xiu, opening up a transparent window into the private thoughts of a colonial Maya subject.

In addition to creating the possibility that the public audience, comprised largely of Maya enthusiasts without professional stakes in decipherment, might witness a breakthrough offered by Schele or another epigrapher, the prefatory remarks (quoted above) to the weekend workshop also helped to affirm and secure the certainty of decipherments presented on stage. It hedged against any potential feelings of incredulity among the public audience by constructing Schele's demonstration as implicitly affirmed by a professional public of witnesses, a collective comparable to the modest, scientific gentlemen given privileged access to the experimental space of Boyle's laboratory (Haraway 1997; Shapin and Schaffer 1985).

The public at the (later) weekend workshops was not solely witnessing Schele's demonstration on stage. They were also witnessing a spatially segregated group of epigraphic experts implicitly affirming the matter-of-fact status of Schele's proofs when they chose not to intervene.<sup>82</sup> Schele's prefatory remarks indicate that in the absence of this intervention, she was not just speaking for herself, or for the hieroglyphs themselves, but also for the collective of epigraphers, including a few scholars to whose expertise she modestly deferred. Haraway (1997:25) points out that the "public space'

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<sup>82</sup> Schele's passage suggests that the audience itself was a political site instating a differentiated range of witnesses' positionalities and degrees or forms of authority through the spatial politics of association. One long-term workshop attendee noted that distinct social and professional factions occupied the same groups of auditorium seats through repeated workshops (Noble, S. Personal Interview. 18 April 2007).

for the experimental way of life had to be rigorously defined; not everyone could come in, and not everyone could testify credibly.” This is the paradox of the modernist laboratory; it is simultaneously the model of a public space and yet its access is highly restricted to a special community of experts. At the same time, this may also indicate a point of divergence between Boyle’s and Schele’s respective laboratories. It is clear that not everyone could testify credibly in the weekend workshops. Schele encouraged the interventions of other experts, but not the general public. Yet, despite her reticence to open up the UNAM audience to potentially “hostile” critics, Schele’s UT-Austin presentations did have relatively few limitations in terms of who could come in (and Schele and Troike purposefully made the event affordable). Indeed, in the age of “public science,” opening up the laboratory beyond the community of experts and constituting a different class of witnesses has become a crucial technique of scientific authorization (Jasanoff 2004; Nowotny et al. 2001; Wynne 2005). But given that science has always been in the business of constituting socially differentiated classes of subjects (Haraway 1997; Harding 2008), I am reluctant to adopt the rather deontological position that this openness is intrinsically good.

The public witnessed Schele’s demonstrations and they witnessed the professional witnesses witnessing Schele’s demonstrations. This performance of historical truth entailed laborious material arrangements assembling an array of actors that could articulate smoothly in the production of hieroglyphic readings as matters of fact. Such arrangements included the mundane details of advertisement and arranging a venue that Troike handled. It also entailed the more analytically compelling mobilization of visualization technologies, particularly projectors, that mediated Schele’s

compelling demonstration that hieroglyphs could be cut apart, analyzed structurally, and ultimately understood as equivalent to modernist writing. The significant labor that enabled this artifice paid off for Schele and other Mayanist experts by producing an audience of committed attendees, whose presence as witnesses affirmed the viability and importance of decipherment. But many of these annual attendees were more than interested Maya enthusiasts satisfied with watching Schele's demonstrations. My interviews and associated letters indicate that attendees were thoroughly convinced by Schele's insistence that hieroglyphs were clearly and primarily writing, and her corollary democratic claim that anyone with the basic tools could participate in decipherment. This ethic of openness and audience members' tedium in hearing her presentations repeated annually (given that many attendees returned every year) motivated the development of a "long workshop" in which attendees could try their hands at decipherment directly.

### **The Long Workshop as a Site of Staged Discovery**

The forms of public engagement operative at the Workshops on Maya Hieroglyphic Writing were not limited to Schele's weekend seminars. From the beginning the workshop facilitated the formation of social and professional connections outside the bounds of this rather delimited pedagogical space. For example, a Saturday night potluck at Schele's house was an important "unofficial" (i.e. not advertised) event for some workshop attendees. The potluck enabled the attendees to personally connect with and discuss epigraphic matters with some of the most important

professional contributors to the field, instating a controlled dissolution of the hierarchy of authority and affirming the workshops' ethic of openness.<sup>83</sup>

More importantly, in 1983 the organizers began to expand the workshop into a more elaborate series of events (Jones 1988b). Schele enlisted other Mayanists, particularly archaeologist Peter Mathews, to provide the Friday night lectures that offered background material on the ancient Maya. This preceded Schele's weekend workshop (which was open to the larger public for a separate fee), an event that continued to summarize the fundamental techniques utilized in hieroglyphic analysis. At this time the organizers began to further supplement the weekend workshops with an in-depth seminar during which participants could acquire more direct, "hands-on" training in hieroglyphic decipherment. The "long workshops" or "Advanced Seminars," which continue today, were originally a site for the seasoned seminar attendees to conduct more autonomous and advanced research into the glyphs. As participant Tom Jones (1988b:vii) described it, "In 1983 the annual Maya Hieroglyphic Workshop...was supplemented by a five-day Advanced Seminar designed to meet an apparent need for deeper and more personalized involvement of participants in the study of Maya writing than was possible through repeated exposure to the then six-year-old Workshop."

Building on letters, interviews, and my 2008 ethnographic participation in the event, this section presents the long workshop as a site where the status of hieroglyphs as a fully-functional writing system could be affirmed by constituting a public committed to this proposition through specific material practices. I present the material practices entailed in the structural analysis taught to workshop attendees as an example of what I call "staged discovery," or the systematic provision of conditions that enable a public to

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<sup>83</sup> Keeler, P. Personal Interview. 9 July 2007.

reproduce a scientific experiment, simultaneously initiating them into a scientific community, affirming the demonstrative power of the original experiment, and cutting away the conditioning agents. “Staged discovery” is an analytical term for rather generic types of pedagogical practices. Yet, here I concentrate on a very specific example, the material techniques of cutting apart and re-ordering hieroglyphic texts into a structural analysis. My claim is that this seemingly basic set of practices is actually a complex site in which the public subjects of epigraphy have naturalized the imposition of a modernist Euroamerican linguistic and cultural ideology and thereby affirmed the epistemological legitimacy of reducing hieroglyphs to texts.

While Jones (1988b) attributes the long workshops’ origin to the increased knowledge shared by annual attendees, it is important to note that the conception of this event had occurred well before they were put into place in 1983. In a 1979 letter to Schele, Troike discussed the possibility of conducting two advanced workshops, one for UT-Austin students and another for “outsiders.”<sup>84</sup> Her letter indicates that the idea for these proposed events was consistent with that later put into place in 1983, stating that the “purpose of advanced Workshop is small group & answering questions as well as demonstrating complexities of decipherment work.” She also mentions, as a drawback, the significantly increased financial burden of conducting such workshops. Schele’s transition to the UT-Austin campus may have eased the organizational and financial burden of conducting a more elaborate workshop. As Schele transitioned from a visiting instructor to a graduate student in the Institute of Latin American Studies and then a professor of art history, this more intensive event became a viable project to sustain.

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<sup>84</sup> Troike to Schele, 16 February 1979.

By 1983, the workshops had also helped Schele to consolidate a range of fellow Mayanist scholars who could aid in the instruction of the workshops as “group leaders,” as well as graduate student volunteers who helped to administer the workshops in exchange for a waived admission fee. On Sunday nights, after the second day of the weekend workshop, the volunteers and group leaders would gather at Schele’s house over leftovers from the Saturday night potluck, and create long workshop groups of attendees with similar epigraphy skill-levels but heterogeneous backgrounds in terms of their knowledge of the ancient Maya.<sup>85</sup> Schele and the organizers placed participants with doctorates in anthropology in groups with undergraduates and lay enthusiasts with little cultural knowledge of the Maya. Such heterogeneity in group members’ positionalities was supposed to democratize the terrain, without fully marginalizing the capacity of some Maya specialists to offer contributions based in their own expertise.<sup>86</sup>

Jones (1988a) compiled an unofficial “Roll Call of Seminar Participants” for the first five long workshops, based on memory and a circulated list of participants’ addresses. Including Troike, Schele, and other official participants in the count, 34 people attended this first Advanced Seminar. Of these 34 initial participants, 20 returned the next year, and 27 returned for at least one additional Advanced Seminar between 1984 and 1987. Schele, Troike, and eight more participants attended all of the first five Advanced Seminars. The number of participants increased annually, amounting to 80 in 1987.

Divided into small groups, these participants gathered over large photocopies of line-drawn hieroglyphic texts for five consecutive days. As a matter of initiation, new Advanced Seminar participants began by analyzing texts from Palenque, so that

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<sup>85</sup> Keeler, P. Personal Interview. 9 July 2007.

<sup>86</sup> Keeler, P. Personal Interview. 9 July 2007.

advanced participants in future years would have a common foundation. Further, the Advanced Seminar groups tended to work on different Palenque texts. Thus, when one group arrived at a component of the text particularly resistant to interpretation, they could consult their peers, searching for similar hieroglyphs in different contexts.<sup>87</sup>

Here I wish to focus on the material practices that defined the long workshop participants' collective engagements with these hieroglyphic texts. As I proceed to describe, participants considered their practices a means of teasing out underlying meanings. Yet, these material practices were also techniques for the public legitimation of decipherments' epistemological premises. They were techniques that prioritized and foregrounded Maya hieroglyphs' status as writing and marginalized alternative conceptualizations of these objects (as art, artifacts, or actors unintelligible to Euroamerican scientific logics).

So what powerful techniques and technologies did the advanced workshop public ally in the serious work of solidifying this epistemological fabric?

Perhaps Jones' (1988b:vii) account answers this question best: "Crowded around the long tables of the two narrow rooms of the third floor of the ILAS building armed with scissors, paste and tape, the groups spread their copies of the Palenque texts before them in hopeful expectation of a Truth about to be revealed." Scissors, paste, tape, and colored pencils or highlighters prominently figured in my interviewees' recollections as well. These must be some of the most mundane technologies ever encountered in cultural studies of science and technology. But they are critically powerful actors nonetheless. These technologies opened up a pathway to Truth, giving public actors a chance to participate in the key work of decipherment, which Schele and David Freidel

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<sup>87</sup> Keeler, P. Personal Interview. 9 July 2007.

(1990:401) elsewhere figure in a thoroughly sexualized and quasi-colonial rhetoric as a process of *removing the veil*. There is scientific conquest underway here.

At the same time, this conquest hinges on elementary technologies that seem to temper the seriousness of interpretation, of revealing Truths. There is something romantic, a sort of nostalgia, built into adults' use of scissors and tape to solve a construction-paper-problem. This is a reminiscent romanticism within the boundaries of the knowable, counterbalancing and making light of the difficult matter of interpretation: "While attacking the texts with scissors was simple enough, the business of structuring the horizontal lines [translating vertical hieroglyphic columns into horizontal structural analyses] soon proved frustrating. How did one recognize a sentence? How did one know where one phrase left off and another began? What did a verb look like? How could one identify a preposition or a noun?" (Jones 1988b:vii).

Even if the militaristic imagery of becoming "armed" with scissors and "attacking" texts seems unfitting of hieroglyphic interpretation, it aptly reflects the underlying sense of employing these tools in a regimented, habitual way to achieve a collective end. It also fits with the imagery of hieroglyphic interpretation as a process of "breaking" or "cracking" a "code" (e.g. Coe 1999[1992]). What is consistent in these rhetorical glosses on the labor of decipherment is the sense of rendering an alterity knowable, populated with classic tropes of masculine, colonial science (Haraway 1997).

At the same time, the material labor of decipherment was a "scale-making practice" (Tsing 2005) that obscured the potentially colonial associations of coaxing underlying primary qualities, in this case meanings, out of the semiotically-opaque surfaces of indigenous artifacts. The translation of hieroglyphs into a paper-and-ink

form had facilitated their reduction to writing before long workshop participants could even begin to apply scissors and tape. Such a transformation, established through Schele's weekend lectures, depoliticized the hieroglyphs by rendering them intelligible as grammatical puzzles.

Schele and the group leaders cultivated participants' collective sense that the hieroglyphs' ancient meanings could not be accessed through the direct visual experience of seeing their images. They assumed that hieroglyphs were *a priori* unintelligible. For example, Schele used the metaphor of "a plate of spaghetti" to describe participants' initial visual experience of the hieroglyphs.<sup>88</sup> The task of the long workshops was to turn this messy enigma into something meaningful. Peter Keeler, who took over Troike's position as the workshop administrator in 1990, described Schele's conception of participants' progress in the workshop settings in these terms: "Linda would say 'Don't panic. This is going to be difficult. You're going to get confused and fight with your neighbors.' About Wednesday or Thursday there would be a shift and suddenly they could see stuff. As Linda would say, 'You're looking at a bowl of spaghetti and your mind would be forced to a point where it would just reanalyze things and see it.'"<sup>89</sup> Structural analysis was the key organizing analytical technique for solving these puzzles. Through structural analysis they made a meal out of that bowl of spaghetti (to borrow and transform Schele's metaphor).

Structural analysis with scissors and tape continues to play a critical role in the beginners' workshops today. By turning to my ethnographic participation in the 2008 workshop, I can provide a more textured account of the experience of staged discovery.

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<sup>88</sup> Schele, D. Personal Interview 12 July 2007.

<sup>89</sup> Keeler, P. Personal Interview. 9 July 2007.

In the stead of weekend lectures (which were moved to the end, and took on a different role), the 2008 Maya Meetings began with a long workshop in which attendees were rapidly introduced to hieroglyphic texts as puzzles. Harri Kettunen, an epigrapher from the University of Helsinki, and UT-Austin graduate student Nick Carter were the group leaders who facilitated the workshop in which I participated. Kettunen performed the basic equivalent of Schele's introductory lectures, providing participants with the requisite background to understand the cultural context and structure of hieroglyphic texts. He recounted how he studied the Mam language spoken in and around Todos Santos, Guatemala, and has since applied his linguistic skills to the development of a database of hieroglyphic signs, with special attention to their changes across space and time. Kettunen's crash course on Mesoamerica then proceeded through an overview of Mesoamerican flora and fauna of Mesoamerica before concluding with an introduction of the structure of hieroglyphic writing.

The group leaders then supplemented this introduction by distributing a workshop handbook by Kettunen and Christopher Helmke (2008). Drawing on our lecture notes and the handbook, the participants, self-organized around tables into small groups, proceeded to "attack" hieroglyphic texts (as Jones put it in the quote above). We progressed from analyses of four small line-drawings of hieroglyphic texts inscribed onto shell plaques from a Classic Period royal tomb at the Maya site of Piedras Negras to the cut-and-paste project, a large line-drawing of the back of Piedras Negras Stela 3 (Figure 5-1). Charged with completing a structural analysis of Stela 3's grammar, we began by dividing the paper into columns corresponding to the linguistic and referential

structure of Maya hieroglyphic texts (Figure 5-2). The task was to transform the “spaghetti” of Stela 3 into an orderly, translatable, grammatical text.

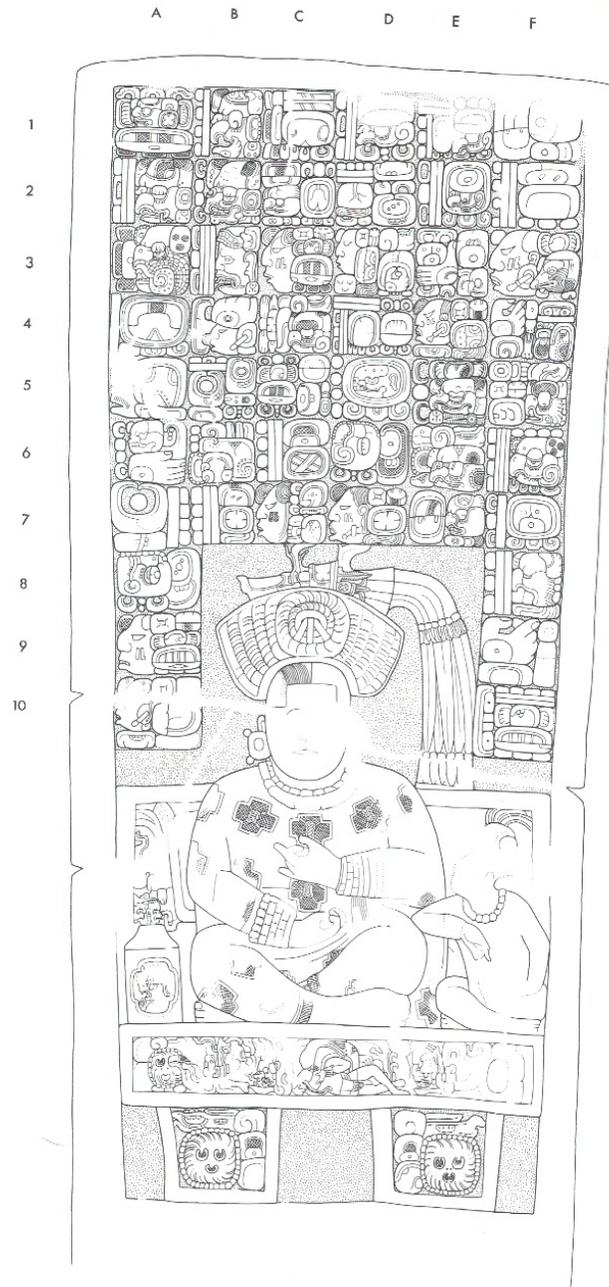


Figure 5-1. The line drawing that served as the primary object of structural analysis in the 2008 Maya Meetings beginners’ workshop. Piedras Negras, Stela 3 Drawing by David Stuart (2003:26), reproduced courtesy of the President and Fellows of Harvard College.

<u>Calendar</u>			<u>Verb</u>	<u>Object</u>	<u>Subject</u>
DN	ADI/PDI	Tzolk'in & Haab'			

Figure 5-2. Template for structural analysis of Maya hieroglyphic texts. “DN” designates the Distance Number and “ADI/PDI” designates the Anterior Date Indicator/Posterior Date Indicator. “Tzolk’in” and “Haab’” refer to the two day naming systems that comprise the Calendar Round.

In his lecture, Kettunen had supplied us with the structural categories that we would need to complete the analysis: calendrical glyphs, verbs, objects, and subjects. To further complicate matters, there are several different types of calendrical or dating hieroglyphs (see Chapter 4). There is the Long Count date, which opens complex monumental inscriptions and provides the absolute temporal “anchor” for the events recorded. Then there are “Distance Numbers” that mark the passage of time between the anchor date and other events recorded, complemented by “Anterior Date Indicators” or “Posterior Date Indicators” that reveal whether the secondary event associated with the Distance Number occurred before or after the anchor date. Finally, there is the Calendar Round, a pair of cycles – the tzolk’in and the haab’ – which intersect to produce unique dates for periods of 52 years (at which point they repeat from the beginning).

Kettunen had most clearly expressed the necessity of locating paired glyphs signifying the tzolk'in and the haab'. We scanned the text for these paired signs, coloring them blue and yellow respectively, and effectively dividing the text into structurally parallel "sentences." We moved on to coding the opening Long Count date and the Distance Numbers that helped us calibrate this fixed historical marker with the cyclical intersections of the tzolk'in and the haab'. Then began the difficult task of distinguishing verbs from objects from subjects, and hazarding meanings. With even this basic text, chosen as a pedagogical exemplar, we stumbled, hesitated, stuttered, and halted. We stared at the syllabary, or list of defined syllabic signs, in the workbook. We compared it to others from published texts, and we groused at their differences. We mistook and miscolored, and slowly, uneasily discerned fuzzy structural similarities between signs defined unequivocally in our reference material and the enigmas on the table. We cringed at realizing through conversation with Kettunen that two of the glyph collocations had to be cut down the middle, each strung across the solid line separating verbs from objects.

After abundant intra- and inter-group conferral as well as consultation with Kettunen and Carter, we eventually arrived at a completed structural analysis of the text (Figure 5-3). Through scissors, tape, and colored pencils, Piedras Negras Stela 3's Truth was revealed. We pieced together a product that Latour would certainly recognize as the culmination of a process of translating the complex, indeterminate material world into a sensible, reproducible inscription. In his ethnographic study of Amazonian pedological research, Latour (1999d:53) puts it this way: "I have never followed a science, rich or poor, hard or soft, hot or cold, whose moment of truth was

not found on a one- or two-meter-square flat surface that a researcher with a pen in hand could carefully inspect.”

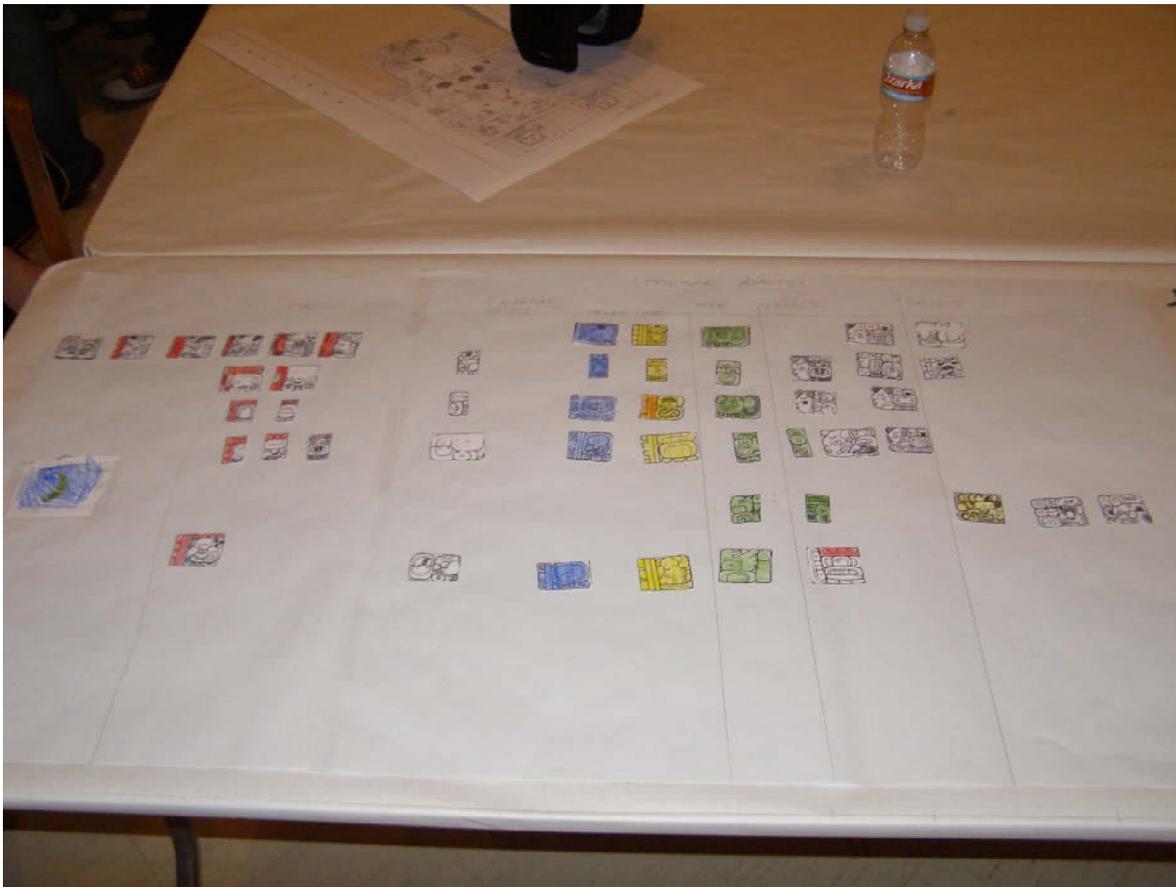


Figure 5-3. The structural analysis of Piedras Negras Stela 3 completed by my group at the 2008 Maya Meetings. Photograph by the author.

Scissors, tape, and structural grids became the technologies that collectively corrected and disciplined the participants' senses, like the air-pump of Boyle's early modern laboratory (Shapin and Schaffer 1985:37). These technologies conditioned the equivalent of an experimental space in which the past could be known with certainty at a spatial remove from the actual sites in which these objects performed their initial roles. The technologies of decipherment approximated the sociomaterial functions of Boyle's air-pump, publicly demonstrating how scientific discoveries were, indeed, matters of

fact. The demonstration that we produced, however, was not an original discovery. We were a public gathering around and modifying these signs in ways that were predetermined for us. We were demonstrating that we could follow along in the logic of decipherment, not merely as an assenting audience, but as a collective that could *reproduce* these discoveries in a laboratory setting.

In our case this affirmation of the epistemological coherency of decipherment found its ground in a few basic tools: scissors, tape, colored pencils, and a syllabary. It was also preconditioned by a pedagogical orientation that taught us that it was a legitimate, justifiable practice to cut the hieroglyphs from their material and aesthetic contexts and to recombine them in new ways (though I should state that Kettunen did note that structural analysis relied on Western principles that did not necessarily apply to the Classic Maya). The monumental inscription had already been extricated from its material context and transformed into a flat surface that could be easily apprehended as ontologically equivalent to contemporary texts. In turn, we, the workshop participants, cut the hieroglyphs from the image that accompanies them. A comparison of Figures 5-1 and 5-3 reveals that the structural analysis entailed cutting the hieroglyphs away from this scene and effectively discarding it as irrelevant to the meaning of the monument. We reduplicated this practice and gave writing a kind of semiotic primacy that issues from its status in the contemporary world. We assumed along with the epigraphers and instructors that the figures inscribed on this monument were of secondary importance to the text, a point to which I return in a critique of epigraphic imaging practices in Chapter 7.

While we employed a mere few tools in our staged discovery, past long workshop participants have brought different kinds of texts and technologies into the fray. As Jones (1988b:vii) puts it, “Among the tools to which many of the seminar participants were introduced in their struggle to find meaning in the glyphs were a number of Mayan dictionaries...and Bryant Tuckerman’s tables of *Planetary, Lunar and Solar Positions*.” These rather conventional tools for the linguistic and astronomical analysis built into hieroglyphic decipherment were complemented with some peculiar technologies:

I also had the good fortune to work in the same study group as Randa Marhenke, who came armed with a Hewlett-Packard HP-41C pocket calculator (augmented with two modules) that she had received the previous Christmas from her husband, Karl, and that boasted a Maya calendar program that he had written, and upon which we leaned heavily to work through calendric problems of the Cross Group [monumental inscriptions from Palenque] (Jones 1988b:vii).

Traditionally, these texts and technologies proved especially significant to the practice of decipherment in the Advanced Seminars because Schele and the other advisors worked – or were supposed to work – within a purely Socratic regime of answering questions with questions and refusing to reveal what the glyphs were.<sup>90</sup> This denial of direct help forced the participants to achieve interpretations themselves, aided by calculators, computers, and a copy machine. Such technologies enabled the attendees to achieve and circulate hieroglyphic readings that stabilized their status as public participants in epigraphy. The technologies helped displace Schele and the other workshop leaders, blackboxing their systemic necessity to the interpretive practices understood as “decipherment.”

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<sup>90</sup> Keeler, P. Personal Interview. 9 July 2007. Noble, S. Personal Interview. 18 April 2007. See also Jones (1988b).

In the process, participants reenacted the experiences of Schele and her peers as the individuals who achieved a systematic interpretation of the structure and syntax of hieroglyphic writing. Because hieroglyphs can be “cracked” only once, the arrival at an interpretation accorded veracity by the shifting interpretive framework accumulates high emotive charge. Advanced Seminars staged this discovery. They provided the conditions for every participant to crack the code, to remove the veil (Schele and Freidel 1990:401), to discover the other’s true essence.

In the dominant disciplinary ideology, *consensus* was – and is – key. Schele did later acknowledge in print (Schele and Grube 1996) and in a documentary (Schele et al. 1999) that contemporary Maya have the right to develop and act on their own hieroglyphic interpretations. Yet, the long workshops have conventionally had much more to do with the pedagogical sedimentation of concepts that regulated bodily and material practices enabling a public to know the glyphs in the same way that Schele and other epigraphers did.

Schele and Troike actively worked to condition the emergence of this public. Schele certainly embraced an ethic of openness and public engagement, and the Maya Meetings were the central site in which this ethic could have consequences. Schele described the importance of the Maya Meetings in these terms:

In actuality, if you look at the field, there are more housewives that know how to read glyphs than there are professional archaeologists, and that’s all because of Austin. So Austin has become the place you come to hear the latest decipherments, to interact – where people from graduate students to housewives can interact with professionals. And I think, [sic] they’ve been crucial in spreading the information and the methodology to a much wider audience than it would have had if we’d kept it in the hands of pure academics (Coe and Lebrun 2005:21).

The carefully controlled workshops created a public of witnesses affirming the legitimacy of decipherment. The workshops have never entailed an apolitical or democratic distribution of knowledge. They have also inevitably entailed the production of a committed, assenting public of witnesses constituted as participants in decipherment through the material practices of staged discovery.

In attending the workshops as vacations from their normal, workaday lives, the Advanced Seminar participants simultaneously sought to witness and maybe contribute to cutting-edge research. They collectively experienced the unique affective thrill of being among the first people to give voice to texts “mute” for 1200 or 1300 years. Most of them cared little about the existing structures of academic authorization and publishing. They instituted their own, rather inclusive, gray literature. In their journal, *U Mut Maya*, they edited, published, and circulated interpretations crafted in workshops. Many participants even stocked their workshop interpretations in the local Kinko’s copy shop for anyone who desired copies.

At the same time, the workshops provided an epistemological affirmation of decipherment’s legitimacy, by constituting a public that reaffirmed their interpretations, sometimes using rather grand terms:

Not only did the hieroglyphic inscriptions make syntactical sense, but their content was demonstrably historical. But most importantly, the realization of these truths had been brought about not by the two dozen odd members of the seminar paying the \$100 and patiently awaiting and receiving their cherished revelation from the lips of the Maya authority from whom they had all come to learn; it had come from the glyphs themselves. Linda had supplied the raw materials and provided the tools, the methods, and the inspiration for probing their meaning. For five long days she moved about the ILAS rooms, her tireless Socratic querying prodding the assortment of anthropologists, linguists, historians, students, housewives, accountants, bankers, lawyers, aerospace and petroleum engineers, physicians, artists, and travel agents that comprised the seminar into prying open the

hieroglyphic texts and discovering for themselves the treasures that lay within. The form and content of the inscriptions, it turned out, far from being the private property of an esoteric circle of believers initiated in the mysteries of the glyphs, were accessible to anyone possessed of a curious mind and willing to invest the requisite time and effort (Jones 1988b:viii).

Jones' account hits at the core of what I mean by *staged discovery*. The reaffirmation of this original experiment, or interpretation, takes shape in Jones' account as a shifting of the agency responsible for "realizing truth." The shy glyphs unveil and give themselves over to the prodding, penetrating interpreters. The Advanced Workshops blackboxed the instructor, the reliable witness, the spokesperson, Schele. They transformed her from a mediator (an active agent requiring acknowledgement in any account) to an intermediary (or actor necessary for producing knowledge but not requiring acknowledgement) (Latour 1999d). The affirmation of Schele's interpretations arrived precisely at the moment that she was no longer needed, when her practices of representation were deemed no longer critical to the matter of fact. The Maya did not have to be represented; they spoke for themselves, revealed as a latent presence beneath, beyond, or within the surface of the reproduced "texts."<sup>91</sup>

Unlike the virtue of repetition in spatially disparate laboratory settings enabled by the virtual witnessing of Boyle's experimental life (Shapin and Schaffer 1985), the

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<sup>91</sup> Jones does not fully explicate the assertion that the content of the hieroglyphs "was demonstrably historical." Yet, Schele and her peers, as well contemporary epigraphers, have clearly associated the possession of history with the possession of state-based politics. On the level of pedagogy, Schele successfully convinced the public of Advanced Seminar attendees that decipherment had shown that – contra the insistence of earlier scholars, especially J. Eric S. Thompson (e.g. 1959; 1971[1950]) – the Maya had partially phonetic writing that recorded History, meaning writing accounts of major political events. This would have resonated as "history" through association with material taught as "history" in schools and sold as "history" in popular books: "In the 'world history' courses that punctuate our childhood education, we learn to place a special value on written history and the civilizations that possess it. In antiquity, history was a very special and rare kind of consciousness and it is a momentous event in our own time when we rediscover a lost reality encapsulated in written words" (Schele and Freidel 1990:403). Such a notion of "history" contrasted with the types of materials-based narratives of quotidian lives and explanations of long-term environmental and economic processes presented by archaeologists of that era (see Hammond 1977).

staged discovery producing this scientific public involved carefully coordinated pedagogical techniques of recursion. The advanced workshops required that participants present their interpretations and structural analyses vocally and visually to an audience, including Schele. At the end of the workshop, they took the stage, roles inverted, and, occasionally, they taught Schele something. These were charged ceremonies that opened the portal between the present imagination of a shared public collective and the imagination of cohabitating an ancient landscape with the Maya leaders and scribes themselves. As examined further in the next subsection, this ritual consumed them and culminated in this collective imagination of another world:

The [1985] presentations reached a dramatic and emotionally charged climax when, after some ten centuries of silence, a classic Maya hieroglyphic inscription was brought back to life as the entire complement of the seminar squeezed into the report room and strained in hushed awe to hear Nick Hopkins give a glyph-by-glyph reading of [Palenque's] Tablet of the 96 Glyphs in Chol [Maya] (Jones 1988b:x-xi).

A great deal of the workshops' power is compressed into this sentence. Historical linguist Nick Hopkins put his ability to speak Chol, a contemporary language structured similarly to the language of the Classic inscriptions, into the service of transforming an inscription into a vocalization that approximated the text's original speech equivalent. Found in the Palace at Palenque, the Tablet of the 96 Glyphs is a grammatically complex text concerning a series of kings' accessions and the celebration of an accession's anniversary (Berlin 1968; Josserand 1995; Schele 1992a). As in participants' descriptions of Schele's lectures, Jones theatricalizes the reading, figuring Hopkins as an actor staging "a dramatic and emotionally charged climax." He emphasizes the critical importance of this reading to workshop participants by informing readers that "the entire complement of the seminar" attended the reading. These

captivated listeners, or witnesses in the visual analytic that I employ here, emerge as requisite participants in the interpretive act. The public is necessary to the science.

To those outside the Mayanist community, Hopkins' reading might seem a rather bizarre act. Here stands a U.S. scholar, a historical linguist, in front of a roomful of amateur Mayanists, with a few linguists, archaeologists, and art historians scattered throughout. Perhaps he stands in front of a projected image of a line drawing of the hieroglyphic tablet from Palenque. Glyph by glyph, he translates this image into a series of sounds that very few of the audience members understand. Perhaps a few other audience members, such as his wife Kathryn Josserand, could understand his vocalization of this inscribed dynastic list. The vast majority of listeners in this room could not. Yet, they "strained in hushed awe." What did they hear? What did they learn? Why is this the *climax* of the presentations at the end of the Advanced Seminar?

Hopkins' reading was no simple academic exercise. It was a ritual. To some audience members this ritual may have seemed to stage the original dedication of the tablet, which likely involved an oral delivery of the text to a public audience (Houston 1994). Thus, there is a sense in which the workshop leaders appropriate or inherit some of the authority or power attributed to the scribes who made the monuments or the nobles who commissioned them, a theme that I describe in Chapter 6. The workshop leaders repeated or replicated the actions of the ancient Maya scribes and nobles, and, in turn, the workshop participants learned to replicate the actions of the workshop leaders.

While earlier years' workshops ended with participant's presentations, perhaps intermingled with the performances of some workshop leaders, such as Hopkins, the

2008 Maya Meetings culminated in David Stuart's revelation of his new reading of the Hieroglyphic Stairway at Copan, which he crowned the "biggest puzzle in the world." The Hieroglyphic Stairway is the longest existing Maya monumental inscription, and its interpreters have been long troubled by the stairway's reconstruction, as the glyph blocks inscribed into the stairs were not positioned in the original order. Stuart proceeded to recount the methods and techniques used in the reconstruction and interpretation of this stairway: "In all my years working on the Hieroglyphic Stairway, [Adobe] Photoshop™ has changed everything.... We don't have to cut and paste." While the beginners' workshop still relied on scissors, tape and colored pencils in 2008, workshop participants have also embraced Photoshop™ and other forms of computer-assisted visualization that displace these mundane, cumbersome tools. Such comparatively expensive visual technologies further distance hieroglyphic interpretation from the hieroglyphs themselves, as they translate hieroglyphic texts into screen-mediated pixels, facilitating epigraphers' interventions into the simulated signs by reconstituting hieroglyphs into a flexible, digital form. Despite the basic availability of the material technologies required for hieroglyphic interpretation, the reordering of workshop social technologies seems to suggest the re-imagination of the public as a less-than-integral participant in epigraphic science.

At the same time the composition of the public is shifting. While the 2008 workshops marked a significant restructuring of public engagement, staged discovery continued, and, in a series of presentations, representatives of a Spanish-language beginners' workshop dissected Palenque's Tablet of the 96 Glyphs materially and digitally, voicing a reading in Spanish and Yucatec Maya (Figure 5-4). As the

organizers of the meetings have become more detached from the scientific public in the years since Schele's death in 1998, the stakes for hieroglyphic decipherment have become all the more critical for contemporary Mayan-language-speakers, who increasingly attend the Maya Meetings. Their presence testifies to the power of epigraphy as a set of epistemic practices that has reverberating political consequences for contemporary Maya who have increasingly articulated historical linkages to the ancient. Understanding the political and epistemic importance of these articulations requires a more complete background into the forms of public engagement staged by epigraphers (and linguists) in Guatemala, discussed in the next chapter.



Figure 5-4. Spokespeople for the Spanish Language Beginning Maya Hieroglyphs workshop present their structural analysis of Palenque's Tablet of the 96 Glyphs. Their digital coding can be seen in the top right-hand corner. Photograph by the author.

## Conclusions

Here I have examined the constitution of a public vested in the interpretive work of Maya hieroglyphic decipherment between the late 1970s and the present. I show that the workshops became kinds of laboratories where Schele and other experts could transform an audience into a set of witnesses who were committed to the epistemic legitimacy of claims to hieroglyphic decipherment. The extrication of hieroglyphs from their material contexts was thus naturalized in these public settings through aesthetic techniques that enabled hieroglyphs to be known at a distance from their actual contexts in Maya sites or on artifacts. I emphasize how Troike's behind-the-scenes labor and Schele's persuasive lectures constituted a public of witnesses who desired to replicate the initial acts of discovery, and occasionally described this process in terms congruent with the rhetoric of colonial science. In producing the public as a set of witnesses, these lectures required very specific techniques for abstracting the hieroglyphs from their material contexts, to which I turn in more detail in Chapter 7. Ultimately, the emergence of epigraphy's status as a public science developed through the participants' carefully managed replication of the innovations achieved by the experts in epigraphy.

Though their structure has shifted, the Maya Meetings remain a critical site for the constitution of an epigraphic public. Their critical importance is further evidenced by the emergence of distinct regional workshops around the United States, many of which build on the model of public engagement exemplified by Schele. Further, beginning in the late 1980s workshops in Guatemala produced witnesses with different kinds of alliances with Maya hieroglyphs and the notion of a Maya culture. The Guatemalan workshops shaped both the epistemic terrain of Maya studies and the forms of activism

operative within the rising pan-Maya activist movement of the 1990s. Thus, I turn to these workshops in the following chapter.

CHAPTER 6  
ARTICULATING A MAYA SINGULARITY IN GUATEMALAN EPIGRAPHY  
WORKSHOPS

**Introduction**

This chapter builds on my examination of the Maya Meetings as a cultural site that constituted a public as active allies of and participants in epigraphic knowledge production. I shift to a second set of workshops on Maya epigraphy facilitated by Linda Schele and her colleagues. Specifically, I explore the series of Antigua Linguistics Workshops sponsored by the Guatemalan research and advocacy group Oxlajuuj Keej Maya' Ajtz'iib' (OKMA). I present these events as another example of Maya epigraphy workshops as a site of public science that was integral to constituting the concept of a coherent, trans-historical Maya culture. As in the Maya Meetings, the instructors of this epigraphy workshop series understood the participants as vital contributors to generating interpretations of the ancient Maya.

Yet, the Antigua Linguistics Workshops differ markedly from the Maya Meetings in the composition and interests of their participants. While U.S. enthusiasts have comprised the bulk of Maya Meetings participants, the OKMA workshops gathered a public of Guatemalan Maya activists and linguists. Thus, it is worthwhile to include a second examination of workshops as sites of public science, due to the OKMA Workshops' distinctive social configuration. Through contrast with the previous chapter, this analysis suggests how scholars and lay participants rework Maya epigraphy in coordination with specific modes of engagement. The chapter further details how workshops stage encounters across cultural difference (Tsing 2005) in which Euro-American notions of history and writing have been mobilized to render the Maya sensible within a modernist episteme.

Linda Schele founded the Antigua workshops and served as their primary instructor from 1987 through 1994. As a result, this set of workshops incorporated materials and pedagogies developed in the Maya Meetings, and translated for spin-off workshops in Mexico and Honduras (Schele and Grube 1996). Yet, this set of workshops cannot be understood as entailing precisely the same types of scientific witnessing and staged discovery that marked the Austin sessions of the 1980s and early 1990s. This divergence issues from the distinct interests brought to the workshop setting by participants who self-identified as Maya and sought to deepen their knowledge of Maya history in order to strengthen their efforts to consolidate a large public of self-identifying Maya. Thus, these participants expressed culturally and politically distinctive concerns with the aesthetics of Maya history. Activist-researchers working with OKMA have entered this quasi-academic arena to gain access to the powerful discourses of Maya history enunciated principally by U.S. and European scholars. In the 1990s such activists layered these discourses into existing critiques of the ideology of mestizaje, or racial-cultural mixing, in Guatemala. I broach this topic by questioning how scholars and workshop participants have articulated concepts of the Maya as a public defined and delimited, in part, through historical aesthetics.

Thus I examine the Antigua Linguistics Workshops as a process that contributed simultaneously to the effort of linguists and activists to consolidate a widespread Maya cultural identity in Guatemala and to the process by which scholars such as Schele came to imagine a unified Maya identity. In other words, both the activists and the scholars had interests in constructing a notion of the Maya as a coherent, essential culture. I argue in this chapter that a workshop-based alliance between U.S. and

European epigraphy scholars, on the one hand, and Maya linguists and activists, on the other, developed into a mutualism providing beneficial political resources to the Maya activists-linguists and beneficial epistemological resources to the epigraphers. The political and epistemological resources unfold out of a mutually crafted and shared representation of the Maya. Since self-identifying Maya comprised the OKMA workshop participants, analyzing the product of the workshop is also a matter of analyzing how the scholars conceived participants as a public and how the participants conceived themselves as a public. Thus, this work extends the shift in the literature on Public Understanding of Science (PUS) and Public Engagement with Science (PES) to examine scientific publics' representations of themselves in relation to institutional knowledge production, to complement analyses of scientists' representations of the public (Michael and Brown 2005). This argument begins to establish a basis for examining the contemporary political and intellectual utility of the concept of a singular, coherent Maya culture, which became a major theme in popular, quasi-scholarly books such as *Maya Cosmos: Three-Thousand Years on the Shaman's Path* (Freidel et al. 1993).

The chapter is organized into three sections: a discussion of the inception of the Antigua workshops, an analysis of how the participants conceptualize the importance of the workshops, and a critique of the workshop instructors' ethico-moral justification of their public engagement. My commentary on these workshops was generated primarily through January 2008 research in Antigua, which involved visiting OKMA and interviewing several active workshop participants. I focus on how participants have envisioned the significance and utility of the Antigua Linguistic Workshops in relation to

their expressions of self-identity and political and intellectual agendas. Finally, I turn to an analysis of published reflections on the Antigua workshops by Linda Schele and Nikolai Grube, questioning how these commentaries represent participants as simultaneously research subjects and collaborators. Through these analyses, I show how the participants and instructors co-produced a specific concept of essential Maya culture through staged encounters across cultural and epistemological difference. “The Maya” emerges as a kind of actor-network emergent out of complex encounters between hieroglyphs (or their simulations) and the distinct enunciatory communities that participated (namely the instructors and Maya activist linguists).

### **The Inception of the Antigua Workshops**

During the 1970s and 1980s a range of anthropologists became intimately involved in empowering Maya intellectuals within the Guatemalan public sphere (Warren 1998). Of particular significance is the work of U.S. linguists Nora England, Terrence Kaufman, and Will Norman during at the Proyecto Lingüístico Francisco Marroquín (PLFM). Nora England played a major role in teaching linguistics to Maya-language speaking intellectuals through the PLFM, and she later founded OKMA (England 1995). Beyond institutional support, this collaborative linguistics research helped revalue Maya indigeneity in Guatemala and cultivated the intellectual-political atmosphere that motivated Maya-language speakers to appeal to Linda Schele for help in crafting their emergent historical discourse.

This offer took place in the summer of 1987 while Schele was working as a project epigrapher for the ongoing excavation of the Honduran archaeological site of Copan. A group of Maya and U.S. linguists had taken the opportunity to travel to Copan as the culmination of a Maya Linguistics Workshop sponsored by the Centro de

Investigaciones Regionales de Mesoamérica (CIRMA) in Antigua (Schele and Freidel 1990:401-3; Schele and Grube 1996). At the request of linguist Judith Maxwell, Schele volunteered to conduct a personal tour through Copan for the CIRMA workshop participants. In attendance during this tour was the director of the PLFM, Martín Chacach Cutzal, who took the opportunity to invite Schele to travel to Antigua and conduct a workshop on Maya epigraphy. Schele complied with the request and enlisted linguists Nicholas Hopkins and Kathryn Josserand to aid with a workshop session that summer (Schele and Grube 1996).

In the final pages of their popular book *A Forest of Kings: The Untold Story of the Ancient Maya*, Schele and Freidel (1990:402) describe the agreement to conduct the Antigua workshop opportunity in these terms:

They finished the final tour and ate a late lunch together before piling back into their buses to begin the long trip home. While they ate, the leader of the Maya, a Cakchiquel named Martín Chacach Cutzal, asked Linda if she would come to Antigua, Guatemala, [sic] that summer and give a workshop on the ancient writing system to a group of modern Maya. She thought about it (for about five minutes) and realized that a lifetime's dream was about to come true. The modern Maya had asked to learn about the writing and the history of their forebears. Linda traveled to Antigua and, amid the earthquake-shattered ruins of a Spanish church, went on a marvelous four-day journey of discovery into the ancient past with forty Maya men and women.

It is significant to note that Schele and Freidel's representation of this encounter and the resulting workshop is positioned as the climax of the narrative of historical discovery recounted in *A Forest of Kings*. That Mayan-language speakers such as Chacach Cutzal were interested in hieroglyphs emerged as a kind of legitimation of the continued importance of epigraphic knowledge production. The function of this and subsequent passages is to constitute contemporary Mayan-language speakers as a public that has directly benefited by the labor of decipherment. In this formulation, Schele emerges as

the central actor who could mediate the renaissance of a relation between “the modern Maya” and “their forbears.”

My central point in foregrounding this sense of a renewed historical consciousness is to emphasize that there were specific material-semiotic conditions that shaped what kind of past should be valorized. Indeed, such encounters functioned centrally to valorize hieroglyphs primarily as textual, historical records, a concept that cuts away alternative modes of historical consciousness and narrative that do not conform with Euroamerican institutional knowledge practices (as well as some knowledge practices that do fit within these institutional strictures).

As I have emphasized in my chapter on the Maya Meetings, a specific body of media are necessary to the constitution of publics vested in these knowledge practices. Having access to the appropriate media was one of Schele’s early concerns in the preparation for the Antigua workshops, a point emphasized in Schele and Grube’s reflection on the Guatemala workshops. Particularly, Schele was concerned with transforming the existing pedagogical media into resources that would be of value in a workshop conducted in Castilian (Schele and Grube 1996). This was not Schele’s first experience conducting a workshop in Spanish, but a full set of language-appropriate materials did not yet exist. Some Spanish-language materials had been developed for the 1978 and 1979 UNAM workshops, and a set of Spanish-language transparencies and half of the Maya Meetings workbook had been translated for workshops that Schele held at the Universidad Nacional de Honduras in 1986 and 1987. Nora England and Lola Spillari de López completed the translation of the workbook, making it possible for Schele to conduct a Maya Meetings-style workshop for a multilingual audience for

whom Spanish served as a *lingua franca* (Schele and Freidel 1990:507; Schele and Grube 1996:132).

In addition to the transparencies and workbooks that served vital roles in constituting epigraphy's public, the workshops in Austin also clearly relied on UT's institutional infrastructure, which Troike constantly accessed to render the workshops viable and compelling sites of public engagement. In Antigua, the established institutes for linguistics, cultural activism, and community development performed these necessary functions. The PLFM, CIRMA, and OKMA were already established institutional bridges between the linguists from the United States and leaders of Maya communities. Among these linguists was England (the founder of OKMA) whom Schele had met in Palenque in 1979.<sup>92</sup> England and other linguists played integral roles in establishing and maintaining the series of linguistics workshops that had already gathered a public of Mayan-language speakers who maintained tangible interests in Maya culture, history, and languages. While the epigraphy workshops did not have any real continuity with the preceding linguistics workshops in terms of content,<sup>93</sup> it is clear that Schele's epigraphy workshops piggybacked on the institutional infrastructure already established by the linguistics community. Most importantly, the initial workshops were held at the PLFM before they shifted institutional sponsorship to OKMA in 1991.<sup>94</sup>

At the time that Schele entered this space, the major violence of the Guatemalan civil war had diminished and the linguistics nongovernmental organizations were emerging as important sites through which activists and community leaders could craft a

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<sup>92</sup> England, N. Personal Interview. 24 March 2008.

<sup>93</sup> England, N. Personal Interview. 24 March 2008.

<sup>94</sup> García Matzar, L. P. Personal Interview. 28 January 2008.

sense of Maya cultural unity. This shift in cultural politics must be understood as a set of factors that was integral to the viability of a U.S. scholar coming to influence Mayan-language speakers' historical imaginaries, something starkly distinct from the resistance that she had faced by public intellectuals such as Alberto Ruz Lhullier in Mexico. The "Maya Movement" had begun to consolidate around issues of "culture," which, at the time, political and military leaders would have equated with seemingly apolitical "folklore" (England 2003:734). As England notes, the Movement increasingly made explicit demands for education, public services in appropriate local languages, and political autonomy. In part because it publicly contradicted Cold War-era stereotypes of the provincial, community-bound Maya peasant, the rise of a substantial Guatemalan indigenous movement became a principal matter of intellectual concern for Mayanist ethnographers of the 1990s (e.g. Fischer 2001; Fischer and Brown 1996; Nash 2001; Nelson 1999; Warren 1998; Watanabe 1995).

The Maya Movement (along with contemporaneous movements such as the Zapatistas in Chiapas) unsettled the stereotype of the provincial "indian," which had begun to erode within the discipline of anthropology by the early 1970s – partially as a consequence of the Vietnam war and the changing political perspectives of both Maya communities and Mayanist scholars (see Rus 2004). Yet, the Movement simultaneously invoked a related set of stereotypes pertaining to an essential indigeneity. At precisely the moment that "constructivist" accounts of culture emphasizing hybridities, diasporas, the inherently political character of research, and writerly reflexivity began to challenge "essentialism," the new generation of Maya

activist-researchers began to assert their essential, timeless Mayaness (one instance of a more general phenomenon occurring during the 1980s and 1990s; see Sahlins 2000).

For example, in her description of returning to Guatemala and sharing research with activists in 1989, Kay B. Warren provides an illustrative moment of encounter between her emerging constructivism and the activists' emerging essentialism. Warren (1998:74) explains how Demetrio Cojtí Cuxil, "the dean of Maya public intellectuals" responded to her new formulation of (Maya) identity by asserting that "the appropriate role for North American anthropologists should be one of helping to identify continuities in Maya culture, the timeless characteristics that make Mayas Maya." Cultural anthropologists grappled with this apparent contradiction in a number of ways. The dominant, if often reluctant, conclusion has been to frame this set of cultural enunciations as "strategic essentialism" or "self-essentialization" serving the purpose of political solidarity (Warren 1998). My subsequent analysis reflects on this conclusion by showing how "essentialism" also fed into a mode of Maya self-imagining that strengthened alliances with epigraphers who had significant power over the public construction of Mayaness primarily in academe (and workshops) and, secondarily, in educational and political contexts.

Schele and her German colleague Nikolai Grube, a fellow epigrapher and workshop-instructor, seem to have considered the epigraphy workshops in Antigua a kind of public engagement that bore the weight of the shifts in senses of scholarly responsibility that attended the decline of the Cold War and the rise of a more integrated global political economy: "We believe the world has changed in the last decade and that in the future academics of the developed world must consider the needs and goals of

the people whom they study” (Schele and Grube 1996:139). Clearly by the 1990s, Schele (and Grube) had come to recognize that Maya hieroglyphic studies had political relevance for Mayan-language speakers themselves, and that producing knowledge of the ancient Maya could no longer take place in a vacuum that disregarded contemporary Maya-language speakers.

Yet as I move to an analysis of the forms of public participation described in the next section, I continue to question what kinds of varied consequences these workshops enacted. Namely, I tease out my position that the workshops were not simply sites in which epigraphers could fulfill a “lifetime’s dream” of “giving back” to contemporary Maya communities. Rather, they were sites in which a historical narrative of continuity could be constructed for the purposes of consolidating a coherent sense of contemporary political solidarity for the activists and a sense of trans-historical continuity that enabled scholars such as Schele to extend the boundaries of their analytical field into the present. The “essentialism” that marked the sites of encounter between epigraphers and activists was thus not simply a cultural “strategy.” It was also a way for epigraphers to further affirm their centrality to the production of Maya knowledge.

### **The Constitution of an Antigua Workshop Public**

While science studies scholars have devoted considerable research to the question of how publics form around and understand specific sciences, less analytical work has been devoted to the problem of how members of such publics represent their own rationales for participating in such collectives. Thus, before I turn to an analysis of how the epigraphers have benefited by and justified the Antigua workshops, this section concentrates on how some of the participants have conceptualized the importance of

epigraphic knowledge pedagogically translated in the workshop sessions. In other words, I am concerned here with what scholars of public science have described as the “public’s representation of the public” (Michael and Brown 2005:42).

I acknowledge that my limited set of empirical materials, namely interviews with a few participants and a small number of pertinent letters, renders any pretense to a “representative” sample of participants’ views unviable. Thus, for reasons that exceed any stated epistemological commitment, my account here is quite partial. My representation of the “public’s representation of the public” is shaped by both Antigua workshops’ small scale and the specific motivations that compelled interviewees to discuss these matters with me. Thus, bear in mind that I am not portending to access the workshops’ publics directly, but to produce a series of translations constituted through my research encounters that open up the possibility of knowing and using the Antigua epigraphy workshop in a new way, namely as a complex site of encounter where differentially-interested publics have generated a kind of “friction” (Tsing 2005) that shapes pervasively-circulated and hieroglyphically-mediated concepts of “the Maya.”

As I mentioned above, the Antigua workshops operated through a structure comparable to the Maya Meetings in terms of the constitution of participants as a public vested in epigraphic knowledge practices. This cultivation of an assembly attached to simulations of Maya hieroglyphs took place through the same kinds of pedagogical practices of scientific witnessing and staged discovery that marked the Maya Meetings. Schele and Grube were the primary workshop instructors, though they also incorporated the aid of Hopkins and Josserand as well as Guatemalan archaeologist Federico

Fahsen. The workshop was principally a smaller and more intimate equivalent of Schele's weekend lectures at the Maya Meetings, here carried out in a single room at OKMA rather than in a large auditorium. Schele lectured with the aid of transparencies and the participants color-coded the hieroglyphs in their workbooks with crayons, and cut up signs with scissors.<sup>95</sup>

The bulk of the workshops' allotment of time was devoted to Schele's instruction, with a more limited version of the "long workshop" representing the culmination of the experience. An employee of OKMA described this independent decipherment as a "test" of what they had learned through Schele's instruction.<sup>96</sup> Schele and Grube typically determined the particular text that would serve as this test.<sup>97</sup> Yet, they also received input from other professionals involved in the workshop. For example, in 1992 Fahsen wrote to Schele to recommend that they use hieroglyphic materials from the Dresden Codex, a Late Postclassic Yucatec Maya document, as their annual test.<sup>98</sup>

I mention Fahsen's interest in using the Dresden Codex because his rationale for this recommendation speaks to the epistemic field of Mayanist epigraphy in the early 1990s as well as the cultural politics of selecting analytical objects for the Antigua workshop participants. Fahsen's letter notes that he was reading a book by archaeoastronomer Anthony Aveni (who was one of Schele's collaborators at the time, see Freidel et al. 1993) and that the Dresden Codex had relevance to research into both

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<sup>95</sup> Garcia Matzar, L. M. Personal Interview. 28 January 2008.

<sup>96</sup> Pérez, J. Personal Interview. 17 January 2008.

<sup>97</sup> Garcia Matzar, L. M. Personal Interview. 28 January 2008.

<sup>98</sup> Fahsen to Schele, 24 August 1992.

hieroglyphs and calendrics.<sup>99</sup> Schele and Grube apparently agreed, as they describe the 1993 workshop in these terms:

In 1993 Schele and Grube returned to Antigua for the fifth workshop sponsored by OKMA and CIRMA. The theme was the Dresden Codex (a pre-Columbian Maya book with astronomical and divinational texts) and how its structure relates to the highland [Guatemalan] tradition of divination still extant. In this workshop, participants included four *aj q'ija'* [shamans or "day keepers" who utilize the 260-day almanac for divination] (Manuel Pacheco and a friend from Joyabaj, as well as a day-keeper from Rabinal and one from the Mam area); five bilingual (Spanish/Mayan) teachers from Santa Catarina Ixtahuacán; and a Mopán speaker from San Luis Petén, who was considerable help in translating since Mopán is a lowland language closely related to the Yucatek of the Dresden Codex.

Participants received copies of the Villacorta version of the Codex (Villacorta 1934), which they glued together into the accordion format of the original book. The presentations and analysis concentrated on understanding the interplay of calendar, text, imagery, and prognostications. Most of the almanacs were analyzed, and the astronomical data were studied. Special emphasis was given to how these almanacs might have been used and how they related to historical and modern activities of highland Maya day-keepers (Schele and Grube 1996:135).

This passage should make clear that the constitution of the Antigua workshops' public differed substantially from the parallel process at the Maya Meetings. Namely, Schele and Grube place a strong emphasis here on the cultural and linguistic linkages between the contemporary Highland Guatemalan Maya participants and the practices represented in the Dresden Codex. While it was almost certainly produced during the Late Postclassic period (1200-1519), there is no consensus concerning the precise date of production for Dresden Codex (Vail 2006:503) and Grube (2001:337) has more recently proposed that the screenfold book is a copy of an earlier Classic Period document. This articulation of a high degree of cultural continuity between the ancient

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<sup>99</sup> Fahsen to Schele, 24 August 1992.

and contemporary Maya is a crucial point for understanding the scientific politics of the Antigua workshops, and a point to which I return throughout this section.

I should note, though, that utilizing a codex with calendrical significance and possible connections to the present as an analytical exemplar was not distinctive to the Antigua workshops. In 1992 Schele was deeply involved in research on Maya astronomy and cosmology, which was published the next year in *Maya Cosmos: Three Thousand Years on the Shaman's Path* (Freidel et al. 1993). The 1992 UT-Austin workshop focused on codical materials, as Schele and Grube presented workshop lectures on Maya cosmological engagements with the “Venus cycle” evidenced in the Paris and Dresden Codices (Schele 1992b). The codices, calendars, and hieroglyphs had become the entangled epistemic objects of Mayanists such as Freidel, Schele, and Grube as they proposed a continuity of Maya beliefs that had endured from the Middle Preclassic (1000-400 BCE) occupation of the Olmec site La Venta to the present (Freidel et al. 1993; Schele 1992b:237). Evidence for these arguments of continuity in cultural belief depended in part on contemporary ethnographic research into Maya communities in Guatemala and Chiapas, as well as Schele’s own quasi-ethnographic encounters with members of these communities (Freidel et al. 1993). The Antigua workshop was a site in which Schele and her colleagues could stage such encounters with contemporary Maya-language speakers.

These participants held their own distinctive cultural and political interests in calendrical matters. The ancient 260-day Maya calendar (what J. Eric S. Thompson termed the tzolk’in) remains a significant tool for the organization of Maya spirituality, and was a key object in the renaissance of Maya cultural identity in early 1990s

Guatemala. In discussing this issue, Maya scholar Victor Montejo (2005:151-2) positions the Maya calendar as a central actor in the affirmation of continuity between the ancient and contemporary Maya. He goes on to cite the concern of elders that the Maya calendar might disappear as well as the cultural importance of Maya priests' being able to hold public ceremonies in 1992, the year of the Columbian quincentennial, without fear of persecution. This was a crucial moment in publicly articulating distinctive forms of Maya spirituality as legitimate cultural practices that consolidated a trans-historical Maya voice in the wake of the violent civil war, the most recent event in a five-century history of colonialism. As Montejo (2005:152) puts it, "In this way the silence imposed on us has been ruptured, and we have begun to perform our ceremonies without fear of being called *brujos*, or witches, but as Maya priests and experts in the ancient Maya calendar and sacred knowledge."

For Schele and other twentieth century Mayanist epigraphers and art historians, the calendar was an object that they could research without having to consider the political and epistemic violence that suppressed its public use in contemporary Guatemala. The early Antigua epigraphy workshops were sites where actors invested in this renaissance of Maya spirituality could gain access to knowledge of the Maya calendar, as well as the hieroglyphs. A public came into being within this carefully managed zone of epistemic and cultural encounters through Schele and her colleagues' provision of access to their intimate knowledge of the cosmological functions of the calendar. Such sites exemplify the thoroughly political character of scientific – in this case epigraphic – research.

That non-Guatemalan scholars could study such cultural objects without fear of persecution is a postcolonial inequity that evidences how scientific assumptions concerning the apolitical status of research actually turn on the contradictory actor-networks that enact disparate relational associations in disparate nation-states. “The Dresden Codex” had divergent relational properties for actors in the United States and in Guatemala – particularly Mayan-language communities in Guatemala – during the 1980s. The Antigua workshops thus emerged as a political site where human actors with unequal access to the means of academic knowledge production came into forms of relationality through the mediation of actors, such as the “Dresden Codex,” its calendars, and its hieroglyphs. We can thus understand calendars and hieroglyphs in Donna Haraway’s (1997:11) terms as particular kinds of “objects into which lives and worlds are built” or “imploded atoms or dense nodes that explode into entire worlds of practice,” Maya studies’ historical additions to Haraway’s litany of figures through which our technoscientific worlds are rendered: the “chip, gene, fetus, database, bomb, race, brain, ecosystem.”

At this particular site, the simulated Dresden Codex instated a world of practice that articulated the ancient with the contemporary and positioned Schele as an actor with the historical-scientific tools to mediate a relation between Maya political and religious leaders, on the one hand, and the proposed underlying, trans-historical kernels of their cultural identities, on the other. Lolmay Pedro García Matzar, a former OKMA-employee, linguist, and long-time workshop participant, attributed Schele’s authority to her deep understanding of the calendar.<sup>100</sup> García Matzar claimed that Schele had knowledge of the calendar that had been lost by even the elder K’iche’-speakers who

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<sup>100</sup> García Matzar, L. P. Personal Interview. 28 January 2008.

continue to actively utilize the calendar for ritual purposes. Schele, Grube, and other Mayanists clearly offered knowledge that directly fed the political-spiritual objectives of the protagonists of cultural unification among speakers of approximately 30 contemporary Maya languages. Their workshop efforts helped to bridge a geopolitical inequity in the distribution of Maya spiritual knowledges that was deeply shaped by the silencing effects of the Guatemalan civil war.

The calendar was clearly a central matter of concern in the constitution of a public through the material, pedagogical practices of epigraphic workshops. Schele's status as a figure of authority issued from her ability to counteract the silencing and fragmenting cultural effects of Guatemala's colonial history. In letters to Schele, participants emphasized the workshops' importance for resuscitating a spiritual consciousness and set of religious practices through access to knowledge of ancient Maya cosmology. Letters sent to Schele by García Matzar and other participants in the emerging Maya Movement also evidence how political-religious leaders played active roles in maintaining public interest in the Antigua workshops during the mid-1990s. These letters request permission for members of their community to attend the workshop, request permission to film a workshop, and arrange for a workshop in the city of Chimaltenango, requests which speak to the rationale for a public to assemble at the workshops. Without the mediation of Schele's calendrical knowledge with contemporary Maya cultural politics, it is hard to imagine that her workshops would have been important to many of these leaders.

At the same time, in the examination of ancient Maya texts, calendrical glyph blocks are integrally linked with non-calendrical glyph blocks. While García Matzar is

probably an exception among the Maya linguists and activists in terms of his high level of hieroglyphic knowledge, other participants saw value in forms of distinctively hieroglyphic knowledge as well. Unlike the Maya Meetings, Schele and Grube's OKMA workshops of the 1990s culminated in the participants collectively traveling to the Maya site that provided the material for the "test." Such experiences aided workshop participants to connect the rather abstract work of decipherment with the actual material contexts of the hieroglyphs.<sup>101</sup> As the civil war came to an official end in the mid-1990s, the archaeological sites were becoming important actors in the consolidation of a contemporary sense of shared Mayaness in Guatemala.

Even before this political transition, participants took the epigraphic workshops as inspiration to imagine their personal, historical connections to the occupants of major Maya sites. For example, in a poetic account written on July 29, 1990 and sent to Schele later that year, a workshop participant described an imaginative journey in which he followed Schele from the present reality to Maya temples of the past and explained his desire to apprehend his ancestors' experiences.<sup>102</sup> He proceeded to suggest, creatively, that ancient kings had shared a small piece of their knowledge with Schele, England, and Grube through the temples, and that these scholars, in turn, shared these fragments with the workshop participants.

The workshop participants gathered around Schele's simulations of the hieroglyphs and articulated their experiences of these modifiable, manipulable signs with the hard stone carvings on stelae and temple walls at sites such as Tikal. In the process they crafted a collective materially- and aesthetically-grounded sense of cultural

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<sup>101</sup> Pérez, J. Personal Interview. 17 January 2008.

<sup>102</sup> Tuch Pum to Schele, September 1990.

coherence and historical continuity. Simulations of hieroglyphs were of critical importance to this sense of unification grounded in underlying continuities of belief and ritual practice.

In addition to the critical significance of simulated hieroglyphs on transparencies and in the workbooks, the workshops helped give rise to a dispersion of the hieroglyphic signs abstracted from their original cultural contexts and inserted into circulation for the contemporary purpose of cultural revitalization. Hieroglyphs thus mattered as actors both allied with and cut from their ancient cultural associations. Pakal B'alam, a linguist and workshop attendee who worked closely with England, recounted in a 1991 letter to Schele how OKMA had begun to actively replicate hieroglyphic Long Count calendar dates and other hieroglyphs in OKMA books and diplomas.<sup>103</sup> He also described the active utilization of the knowledge acquired in workshops to teach Kaqchikel-speaking children to read glyph blocks for dates and colors. B'alam thus emphasized how the workshop attendees had actively applied the knowledge that they achieved to the efforts of cultural revitalization. Furthermore, reproducing simulations of hieroglyphs, and the attendant invention of novel graphemes to accommodate the phonemic conventions of highland Mayan-languages,<sup>104</sup> became an integrally important sign of a revitalized historical consciousness for activist Maya NGOs and publishing houses in the 1990s.

Anthropologist Circe Sturm (1996) has addressed this issue in a chapter that analyzes the modification and production of such hieroglyphs as a contemporary practice in the constitution of a Maya ethnonational community. Sturm adroitly links this issue to how the workshops have established a relation of inequality that constitutes

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<sup>103</sup> B'alam to Schele, 3 November 1991.

<sup>104</sup> García Matzar, L. P. Personal Interview. 28 January 2008. See also Sturm (1996:119-120) and Schele and Grube (1996:133).

participants as consumers of outsiders' interpretations. In response, Sturm (1996:128) calls for an increased "epistemological modesty" among epigraphers. She asks that they enact an ethic of – to echo her use of Trinh T. Minh-Ha's (1982) phrase – "speaking near" the hieroglyphs rather than producing direct, objective interpretations. Her stance bears in mind that the public constituted in sites such as the Antigua workshops defer to the epistemological authority of those with positions in relatively powerful academic institutions, a point reiterated by my interviewees in Antigua. These are crucial steps in acknowledging how the analytical objects of Maya epigraphy are complex actors that can never be rendered fully sensible within Euroamerican structures of historical knowledge production, and that unreflexive attempts to do so reinstate inequalities. I reiterate Sturm's points today, a decade and a half later, while I simultaneously fear that they seem even less viable in the era after Schele's death, as the Antigua workshops have declined in public attendance outside OKMA-affiliates,<sup>105</sup> and in embracing linguistic "science" some epigraphers seem to have become more detached and ambivalent towards the contemporary consequences of their historical narratives (e.g. Houston 2000:141-2).

Nevertheless, the workshops have gained a kind of independent life and ability to travel, as García Matzar has sought to establish workshops with introductory and intermediary levels of instruction in dispersed Guatemalan communities. Resembling the work that Schele and Grube conducted in the Yucatecan community of Chichimila (see Mooney 1996), these regional workshops would permit the involvement of participants lacking the motivation or economic resources to travel to and stay in Antigua. This reflects García Matzar's commitment to the direct and localized political

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<sup>105</sup> García Matzar, L. P. Personal Interview. 10 January 2008.

application of epigraphic histories. In so doing, he seeks to produce a more widespread public involved in the kind of imaginative, cultural and historical unification staged at the Antigua workshops. In other words, he has worked to construct another layer of scientific engagement with the public. This example suggests that the increasing democratization of a particular science may be evidenced by the repetition of a particularly reflexive pedagogical form. Where the content of the workshops attended by García Matzar enabled him to become a representative of (and for) Maya hieroglyphs, the accessible form of the workshops has also enabled him to imagine their modular replication, facilitating the further dispersion and democratization of historical knowledge in Guatemala.<sup>106</sup>

While both the actualization and success of these subsequent workshops remains contingent on factors including funding and other resources, their very conceptualization indicates that the “public” that participates in reflexive social institutions can succeed in constructing its own subsequent public. While workshop participants and workshop-trained instructors such as García Matzar may not enter into scholarly debate with the small set of professional epigraphers (because the discipline has adopted rigid mechanisms of internal authorization), they can serve as Latourian (2004a) “spokespeople.” In other words, they translate the assemblage of historical inscriptions, professional interpretations of these inscriptions, and political implications of these interpretations for an emergent enunciatory public. The instructors, interpretations, politics, and publics fold into the hieroglyphic objects themselves. Understanding how this open system and mutating, replicating enunciation of publics has emerged requires

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<sup>106</sup> This “modular replication” depends on certain standardizing technologies crucial to workshop settings, particularly photocopiers and computers.

detailing the instructors' own concepts of public engagement, to which I turn in the next section.

### **The Ethics of a Maya Singularity**

Here I offer a reading of workshop instructors Linda Schele and Nikolai Grube's most detailed explication and moral defense of the Antigua workshops, which was published in an edited volume on Guatemalan Maya cultural activism (Fischer and Brown 1996). This volume is principally intended for readers with closely related academic interests. Therefore, to add breadth to the materials through which I question the instructors' concepts of the public, I read the defense of the workshops in conversation with an article reflecting on the history of Maya epigraphy published six years later by Grube (2002) for a University of Texas Office of Public Affairs newsletter, *Discovery: Research and Scholarship at the University of Texas at Austin*.

Schele and Grube offer their understanding of the goals and accomplishments of the initial 1987 Antigua gathering in terms that begin to illustrate how they conceived the potential contributions of epigraphy to a contemporary Maya public:

The first workshop was designed to teach the Maya participants some of the history of the writing system, how to spell using syllable signs and logographs, the syntax of the system, a set of common verbs and titles, and, on the last day, how to do a structural analysis of the Tablet of 96 Glyphs from Palenque. Though this last work was not finished, a team of Q'eqchi' speakers led by Eduardo Pacay read their translation of the first half of the tablet and thus became the first modern Maya to read an ancient text in their own language (Schele and Grube 1996:132).<sup>107</sup>

At work in this passage, and the chapter as a whole, is a concept of epigraphy as scientific labor that produces a public good. The form and utility of this good differs

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<sup>107</sup> Note that the hieroglyphic texts' Classic Ch'olan script differs substantially from modern Q'eqchi'. Therefore, the effort to read the hieroglyphs in Q'eqchi' would require a complete translation from the original. The glyphs were written in a language that has been reconstructed through a combination of research in epigraphy and historical linguistics (see Wichmann 2006).

somewhat from other sciences. Where mechanical engineering might design a more efficient car and biomedical science might innovate new AIDS treatments, the products of epigraphic research seem to lack tangible utility for a public of users. Yet, here, what Schele and Grube defend is precisely the *utility* of innovations produced by academic inquiry into the ancient Maya writing system. Schele and Grube do not code the public value of hieroglyphic research in the economic discourse of the mechanical engineer or the health discourse of the biomedical scientist. In asserting that the Q'eqchi' speakers became the first contemporary Maya to enunciate the words of their cultural ancestors, Schele and Grube imply that hieroglyphic interpretations have the latent capacity to consolidate and strengthen Maya historical consciousness.

Schele and Grube effectively balance a commitment to sharing their research with interested Maya and a utilitarian detachment from the consequential uses of hieroglyphic knowledge. Unlike other authors published in the edited volume, Schele and Grube do not position themselves as activists contributing explicitly and intentionally to the Maya Movement. Instead, they assume that the product innovated by hieroglyphic research since the early 1970s has intrinsic utility for participants in the Movement. Schele and Grube (1996:138) explicitly comment that their essay emerged, in part, as the response to an unnamed U.S. scholar's description of the relation between epigraphers and Maya informants as "colonial." Their moderate detachment enables the authors to position their research outside of an oversimplified opposition between activist and colonial research. Thus, it functions as a moral defense by asserting the intrinsic utility of the products of hieroglyphic research.

In other words, the authors express an ethical commitment to sharing their normative constructions of Maya history, materialized as hieroglyphic readings. Yet, they do not concern themselves much with any specific political consequences that might result from modern Maya developing knowledge of either their academic discipline or the history that its practitioners purport to represent. For example, Schele and Grube (1996:133) employ a carefully neutral prose to describe the modification of the lowland hieroglyphic writing system for the participants' highland Maya languages. The authors refuse to judge such transformations of the hieroglyphs. They do not overly romanticize this innovation, explore its potential political implications, or condemn it as a violation of the glyphs' historical purity. The maintenance of detachment in this arena differs somewhat from the generally politicized nature of anthropological research in Latin America, which I describe in Chapter 3, and Grube's later public description of the workshops, which I describe below.

Schele and Grube's defense of the workshops turns on the authors' conceptions of hieroglyphic research as a structure of action introducing a product to the public sphere. Yet, it remains important to question the experts' corresponding notions of the Maya workshop participants as contributors to this public sphere, beyond alluding to their characterization as consumers. Schele and Grube remain cautious in their descriptions of the participants and their motives. Perhaps addressing the interests of participants would risk a form of political representation, a form of "speaking for," that Schele and Grube situate as external to their abilities and responsibilities: "We do not feel that we have the right or the knowledge to speak for the Maya, either individually or collectively" (Schele and Grube 1996:137). Without judging whether they had "the right or the

knowledge to speak for the Maya,” I assert that the collaborative workshops assumed and worked to reinforce a nascent cultural concept of the participants with manifest political implications. In other words, the concept of the Maya public employed by Schele and Grube entailed a form of political representation.

That is, hieroglyphic experts conceived and portrayed “the Maya” as a singular cultural entity (at least in published descriptions). The workshops helped smooth the Maya – across space and time – into this undifferentiated culture by providing the conditions for contemporary self-identifying Maya to construct a cultural history. In the reflection provided by Schele and Grube the workshops sought to fulfill Cojtí Cuxil’s mandate for North American anthropologists: “to identify continuities in Maya culture, the timeless characteristics that make Mayas Maya” (Warren 1998:74). Without reducing Pan-Mayanism to a political game ruled by cost-benefit logic, the value of solidarity across difference in the imagining of a unified Maya nation far outweighed the material and epistemic violence perpetrated by the elision of linguistic, historical, national, and political differences.

For the Maya activists and intellectuals, emphasis on or assumption of cultural homogeneity worked to combat a general concept of cultural fragmentation caused primarily by colonial rule and secondarily by the civil war. In turn, the socially engaged hieroglyph experts’ awareness of the political valence of cultural homogeneity played an unequivocal role in their construction of the Maya workshop participants as a scientific public. They held that their research pertained to contemporary Maya because it illustrated part of the cultural essence of the living participants, despite the fact that the hieroglyphic texts dated principally to the Classic period. Since the scholars valued

hieroglyphic writing as a supreme cultural achievement, this re-historicization of the contemporary Maya had anticolonial value in its capacity to combat stereotypes of ignorant or unproductive “indians.” As cultural anthropologists struggled with an apparent gap between Maya intellectuals’ essentialist self-identities and the rising prominence of constructivist cultural explanations within the discipline, the hieroglyphic specialists began to assert quite loudly the essential, timeless nature of Mayaness.

Why would prominent hieroglyphic specialists diverge so strongly from their cross-disciplinary colleagues in anthropology and risk accusations of essentialism by advocating a timeless Mayaness in popular texts such as *The Forest of Kings: The Untold Story of the Ancient Maya* (Schele and Freidel 1990) and *Maya Cosmos: Three Thousand Years on the Shamans Path* (Freidel et al. 1993)? Clearly, the consequences of this construction of the Maya were not limited to the political sphere of Maya cultural revitalization. The conception of continuity between the workshop collaborators and the ancient Maya elite represented in hieroglyphic inscriptions had a dialectical value for Schele and colleagues’ scholarly and quasi-popular publications. For example, in the first chapter of *Maya Cosmos*, David Freidel, Linda Schele, and Joy Parker (1993:48) comment that, “Our experience and study have convinced us...that a unified view of Maya ritual and cosmology has endured for at least two millennia.” In practice, this implies that highland Guatemalan Q’eqchi’-speaking activists of the 1990s shared a core set of cultural values and traditions with the lowland scribes of the 600s CE, an articulation mediated through Schele’s calendrical knowledge. Such a remarkable degree of cultural conservatism enables the authors to draw heavily on their experiences with contemporary Maya cultural practices and symbols in interpreting

Classic period archaeological remains and epigraphic texts. Thus, regardless of its historical accuracy or the uncertainty that such invocations provoked among some peers in cultural anthropology, the mutual proposition of continuity offered beneficial political resources for the activists and beneficial epistemological and methodological resources for the scholars.

Where Schele and Grube largely evade explicit commentary on the political implications of cultural continuity in the 1996 publication, Grube's (2002) later reflection on his involvement in the Antigua workshops much more directly confronts the entanglement of politics and science in 1990s post-war Guatemala. This shift seems to reflect differences in both the historical-political context and the genre of expression. On the one hand, by the turn of the millennium, Guatemalan political conflicts had calmed considerably. On the other, this publication served the explicit end of university "public relations." Thus, *Discovery* presents a means for academics such as Grube to distribute representations of their research in the service of demonstrating the university's public accountability, and likely securing financial support for research.

The particular issue of the newsletter that included Grube's article highlighted research produced through the university's multifaceted Latin American Initiative. The editor's words that preface the issue indicate that the University of Texas, as an institution, was committed to the overt construction of a Latin American public:

For those of us who live in Texas the border with our neighbors to the south is merely a thin ribbon of river. Eighty years ago farsighted leaders at the University of Texas recognized that teaching and research on Latin America would greatly benefit this state because of its Hispanic heritage. They took the words of Ashbel Smith, first president of the UT Board of Regents, to heart: "It belongs to Texas by right of our geographic position to furnish all this instruction to our neighbors on the other side of the Rio Grande" (Hatfield 2002:1).

Inevitably, such attempts to offer “instruction” to “neighbors” raise difficult ethico-moral questions surrounding the potentially imperial character of a pedagogy that fails to consider its own historical conditions. Exactly how knowledges traverse cultural disjunctures raises further empirical and conceptual problems.

These potentially controversial dimensions of research are not lost on the epigraphers organizing, participating in, and observing workshops. In this piece, Grube lacks qualms with representing the political implications of their collaborations. For example, he represents their involvement in helping Maya political elites consolidate a widespread cultural consciousness:

Linda and I, together with the greatest Guatemalan expert on Maya hieroglyphic writing, Federico Fahsen, spent many days and nights discussing with Maya intellectuals the impact of hieroglyphic decipherment, the rediscovery of Maya history and the historical connections between the highland Maya and the lowland Maya who built the great cities of Tikal, Palenque and Copan. Demetrio Cojti [Cuxil], a Kaqchikel who participated in the Yaxchilan workshop [a workshop on texts from the site of Yaxchilan] and now is Guatemala’s minister of education, was convinced that the Maya needed to learn their history if they wanted to embrace the future. For him and many other Maya, the workshops were the only way by which they could learn about recent advances in hieroglyphic decipherment (Grube 2002:48).

Grube proceeds to detail the successes of the workshops in terms shared by the public described above, including the growing interest among Mayan-speakers in archaeology and hieroglyphic writing, along with the incorporation of hieroglyphic writing into school curricula, and the growing use of hieroglyphs for headlines and titles printed in newspapers and other Maya-related publications. Without devaluing the significant role of the workshops in dispersing interpretations and images of glyphs, it should be clear that the end of the civil war and severe violence of the 1980s provided the opening for collaborations among scholars and activists, as well as the increasing valuation of

history principally through the rise of cultural tourism. Thus, the cultural valence attributed to the hieroglyphs should not be seen solely as an outgrowth of the Antigua OKMA workshops. Instead, the workshops reflect a broader cultural revitalization with intersecting political, educational, and touristic components.

Within this context, the workshop instructors embraced a collaborative ethic imagining the participants as both the students and the subjects of study. This concept of the participants fueled both contemporary cultural revitalization and historical research. While critical anthropological sentiments might be disturbed by the hieroglyphic experts' explicit adoption of a conservative historical discourse verging on essentialism (if not by the activists' own strategic essentialism), the construction of the public on which this conservatism stood had such pervasive social and epistemic value that it was not just *defensible* for workshop instructors and participants, but *inevitable*.

Witness the concluding words of linguists J. Kathryn Josserand and Nicholas A. Hopkins' acknowledgements printed in a 2001 workbook used for the Antigua workshops. They write, "Finally, we thank you, the participants in this short course, because without students there is no class, and if it were not for your presence, we could not spend a few pleasant days investigating a unique literary tradition, the hieroglyphic literature of past Maya, and to do it in the company of the authors of the Maya literature of the future" (Josserand and Hopkins 2001, my translation). Maya activist-researchers have increasingly displaced foreign ethnographers and linguists as the principal representatives of their communities and culture. While, despite the efforts of Schele and Grube, there exists no analogous trend among epigraphers, it remains clear that new modes of narrativizing continuity – here revalued as literary tradition –

may continue into the future to endow epigraphy's Maya public with some power over the past. At the same time, one significant, otherwise unexamined consequence has been the attribution to hieroglyphic specialists of much greater interpretive authority over the nature of Maya identity.

### **Conclusions**

In responding to unsourced accusations of colonialism, Schele and Grube (1996) invoke the contributions of the Antigua workshops to the political causes of the Maya Movement. From my position, these were certainly laudable achievements. At the same time, there is a deeper kind of epistemic colonialism at work at the very heart of epigraphic research that seeks to render Maya history sensible within Euroamerican logics. The techniques of historical valorization mobilized by Schele, Freidel, Grube and their peers had a clear basis in conventions for legitimizing colonial rule and marginalizing subaltern subjects' conventions for rationalizing and ordering their past. Most centrally, they adopted a seemingly uncritical valorization of "writing" as the actor that consolidates historical consciousness and civilizational achievement:

In the "world history" courses that punctuate our childhood education, we learn to place a special value on written history and the civilizations that possess it. In antiquity, history was a very special and rare kind of consciousness and it is a momentous event in our own time when we rediscover a lost reality encapsulated in written words. The Maya inscriptions that have been unlocked by the decipherment offer us the first great history of the Americas (Schele and Freidel 1990:403).

This is certainly a more difficult kind of colonialism to address or counteract, since it operates at a much more basic level in the production of ancient Maya knowledge. Recognizing such complicity would certainly entail an active, caring response to Sturm's (1996) advocacy for a greater degree of epistemological modesty among epigraphers.

Yet the cultural and scientific politics of decipherment and ancient Maya studies are more complex. The very notion of a coherent, singular Maya culture rests on colonialist ideologies of history and is, at the same time, a necessary premise of the Maya Movement, a form of resistance postcolonialism. Maya essentialism is a shared conceptual resource that simultaneously strengthens the interested political arguments of cultural representatives of the Maya Movement, such as Demetrio Cojti Cuxil and Lolmay Pedro García Matzar, and hieroglyph experts, such as Linda Schele and Nikolai Grube. In both cases, the potential for the identification of a coherent Mayanness worked to cultivate a broader collective based on shared senses of history and self-identity. For the activists, this has involved organizing indigenous communities across linguistics barriers, unifying them under the cultural meta-sign “Maya.” As the epigraphers shared a notion of Maya culture as based on a unified cultural history, the activists shared their own cultural knowledge and a sort of authorization to use contemporary Maya practices as a way of elucidating Classic Maya thought and action.

My objective here is not to cast participants in this mutualism as strategic and self-interested rationalists using “collaboration” or “public engagement” as a cover for aggrandizing their own political and scholarly capital. Instead, I have sought to think through the problem as a form of encounter across cultural and epistemological differences, where a singularized “Maya culture” has become a site and resource for intersecting enunciatory publics. Like the calendar, “the Maya” is a network that constantly transforms as it comes into relations of contiguity with differentially positioned actors. For example, García Matzar and other members of the public constituted in

workshops have mobilized workshop pedagogies and objects to constitute new publics in very productive ways.

At the same time, the very notion of “the Maya” should continuously call epigraphers and other Mayanists to adopt positions of care and attachment in their contributions to a discipline that has significant power over the governance of Guatemalan identities. In this public science, the public became the scientific object, a problem that demands careful and consequentialist ethical commitments. Responding to accusations of colonialism by justifying the utility of workshops’ products does not address the more fundamental colonial valorization of “text” as the primary site for recording history, and it leaves open the possibility that scholars could consider readings fixed and context-independent. Addressing these problems entails closer analyses of how specific material-semiotic fields and the aesthetic practices that they sustain give meaning and consequentiality to hieroglyphs, as powerful organizing nodes in the Americas today. I turn to this problem in the following two chapters.

## CHAPTER 7 INSCRIBING AND CIRCULATING THE ANCIENT MAYA

### **Introduction**

Workshops for Maya enthusiasts, lay epigraphers, and activists clearly helped to consolidate the notion that a culturally and spiritually coherent Maya culture has existed for several millennia. The manipulation of simulated hieroglyphs and imagery stands out as singularly important among the various material practices giving visual form to the Maya in workshop settings. Yet these practices have not been confined to workshops and must be understood within a history of techniques of visualization through which scholarly interpretations and popular understandings of the ancient Maya have mutually formed. That Mayanists can produce knowledge of the past through the mediation of simulated images of Maya landscapes, imagery, and hieroglyphs is a basic, if often unacknowledged, technical premise of the field. Understanding the cultural politics of epigraphic knowledge practices thus necessitates attention to the technologies and aesthetic conventions that have shaped the artistic rendering of the Maya and their artifacts, the various media through which we can *witness* past worlds in the present. In order to understand the persuasiveness and consequentiality of historical discourses crafted inside and outside workshops, this chapter explores a sample of imaging and literary practices that have defined the community of hieroglyph scholars and their forms of public engagement.

I examine how a series of highly individualized, yet interconnected, innovations have facilitated decipherment and altered the contemporary imaginary of the ancient

Maya.<sup>108</sup> The innovations to which I attend include J. Eric S. Thompson's catalog of Maya inscriptions, Merle Greene Robertson's rubbings of imagery and hieroglyphs, and Linda Schele's drawings of hieroglyphs. I argue that these inscription practices have constrained how scholars and publics imagined the ancient Maya, and that the vividness of this developing imaginary served as an epistemological underpinning for scholars' claims that they can read hieroglyphs as literal texts that provide unmediated insight into ancient Maya ideas. Each example of aesthetic translation to which I attend in this chapter helps to tease out the practices that mediate the ancient Maya's becoming knowable and active within spheres of cultural practice at a spatiotemporal remove from their own worlds.

The images have spurned complexity and traveled smoothly across cultural and epistemological differences, fortifying scholars' claims to objectivity. A substantial body of literature has emphasized the critical importance of abstracted images to the formation of epistemic certainty (e.g. Baigrie 1996; Baldasso 2006; Daston and Galison 1992, 2007; Jones and Galison 1998; Latour 1990; Lefèvre 2003; McAllister 2002; Tucker 2005). Yet, the persuasive power of these imaging conventions and technologies also depends on the channels through which they circulate. Thus, in the following chapter, I present an argument integrally-connected with this brief history of aesthetic practices as I trace how one particular hieroglyph moved through different aesthetic media and emerged as a powerfully representational object, an object that materially instantiated the integral role of epigraphy to understanding the ancient Maya.

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<sup>108</sup> Because I focus here on publicly circulated images, this chapter builds only indirectly on my "fieldwork," or ethnographic and archival research. Yet it provides a crucial bridge between the more "fieldwork"-based workshops discussed in Chapters 5 and 6 and the interpretations discussed in Chapter 8.

I want to emphasize that the imaging techniques discussed in this chapter are also “scale-making projects” (Tsing 2005:57), or performances that selectively delineate and valorize bounded assemblages of actors in the ancient Maya world. They are conjurings, ways of bringing certain ancient Maya experiences into the present at the expense of others. These images propose, bound, define, and valorize different material objects and cultural forms. As such, they are micropolitical projects that control what is deemed significant and insignificant about the past, with powerful implications for how that past can be known and through which disciplinary and methodological tools. Scale-making practices are never epistemologically or politically neutral (Tsing 2005:57). Indeed, I think that each of these scales of imaging brings into being new means of imagining the ancient Maya with political consequences that ripple beyond the narrow collective of scholars professionally vested in Maya studies. Of course, the practices are also at the center of the field of Mayanist epigraphy, and, as I show in the final section, the cuts enacted by line drawings tangibly affect interpretations of the ancient Maya.

I have made a deliberate choice to concentrate on the aesthetic practices of Thompson, Robertson, and Schele in this chapter.<sup>109</sup> Imaging techniques crafted by

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<sup>109</sup> Here I consciously foreground how two prominent women became major contributors to Maya studies in part through artistic talents that they each acquired for reasons unrelated to the field. I do not frame my critique as a direct contribution to feminist science studies. Yet I am fully aware that the exclusion of women was integral to the development of early modern scientific knowledge practices, and that this exclusion enabled male scientists to claim privileged access to the representation of nature within public discourse (Haraway 1997). I further recognize that women artists historically found employment as scientists’ assistants, and that their male collaborators regularly effaced or appropriated their contributions to knowledge production (Daston and Galison 2007). It is without question that ancient Maya studies is a field historically dominated by men. Thus, though there are some examples of twentieth century women who became Mayanist scholars through more conventional interpretive competencies, it is remarkable that three of the most influential women within this field (including Tatiana Proskouriakoff, in addition to Robertson and Schele) achieved their disciplinary status through artistic skills.

other scholars have played comparable roles in the development of contemporary Maya studies. Additional practices of particular note include Ian Graham's hieroglyphic corpus project and Justin Kerr's development of "rollout photography," which has enabled the surfaces of painted cylindrical ceramic vessels to be transformed into flat, two-dimensional images. While these are markedly consequential imaging techniques, I devote space in the following chapter to rollout photography, and am reserving the hieroglyphic corpus project for a more thorough analysis in a different project.

### **J. Eric S. Thompson's Catalog**

The contemporary epistemic terrain of Maya hieroglyphic knowledge production has been heavily shaped by specific techniques of classification. This should not be surprising. After all, classification and standardization are pervasive structuring practices both inside and outside science. In their important study on the topic, Geoffrey C. Bowker and Susan Leigh Star (1999:10, emphasis in original) define classification in these terms: "*A classification is a spatial, temporal, or spatio-temporal segmentation of the world. A 'classification system' is a set of boxes (metaphorical or literal) into which things can be put to then do some kind of work – bureaucratic or knowledge production.*" In this section, I briefly analyze Thompson's (1962) book, *A Catalog of Maya Hieroglyphs*, as a scale-making classification system and, thus, as an actor that has mediated the work of hieroglyphic decipherment. While Thompson opposed Knorosov's (1958) argument for phoneticism (see Chapter 4), his catalog became an integral condition enabling hieroglyphic decipherment. As such, I maintain that the catalog operated through the practice of a strategic cut, a marginalization of the material texture and aesthetic complexity of specific empirically accessible Maya hieroglyphs that established ideal hieroglyphic types as standards.

Thompson was not the first scholar to produce a systematic classification of Maya hieroglyphs. Indeed, such efforts could be traced to Diego de Landa's (1994[c. 1566]) attempt to discern the logic of hieroglyphic writing, an inquiry flawed by his mistaken presupposition that the hieroglyphs functioned as an alphabetic system. However, the first attempts to produce catalogs that typologically structured empirical examples in the codices and monumental inscriptions did not emerge until the twentieth century. In 1931, William Gates published *An Outline Dictionary of Maya Hieroglyphs*, the first attempt to systematically catalog the hieroglyphs that appeared in the three then-known codices. Günter Zimmermann (1956) later improved on the Gates catalog in his *Die Hieroglyphen der Maya-Handschriften*.

However, Gates' and Zimmermann's catalogs both focused almost exclusively on materials from the codices, and neglected the monumental inscriptions. In 1962 Thompson rectified this limitation by publishing a catalog that categorized signs from both sources. The catalog identifies 862 signs divided into three categories: affixes, main signs, and portraits. Thompson provides individual listings that document the material contexts of many affixes and all of the 356 main signs. The individual listings cite as few as one example of the sign and as many 611 examples, as in the case of *Imix*, which designates the first day of the 260-day tzolk'in calendar.

Thompson (1962:5-9) provides sufficient explanation of how he compiled the catalog to enable its review and analysis here. The first step in the process was to acquire photographs or photostats of drawings of as many monumental hieroglyphs as possible. Thompson individuated the signs that appeared in the photographs and photostats, as well as those in drawings by Hermann Beyer, literally cutting them apart

and mounting them on large “gray cards” (23.5 x 18.5 cm) with one or more card designated for each main sign. This was a materially-intensive project of cutting and pasting, rendering the hieroglyphic system structurally sensible: “In the case of some of the commonest glyphs, such as Imix with 611 entries and Cauac with 427 entries, a dozen or more cards were needed” (Thompson 1962:6). Thompson notes that he did not include examples of glyphs from the codices or ceramic vessels on the gray cards. Rather, he added these directly to the listings as he transformed the gray cards into the listings.

The process of converting the gray cards into catalog entries posed further difficulties. This entailed the challenging problem of confirming that the ideal image that heads each listing faithfully represented the definitive qualities that distinguished the type. Such typological reduction, which subordinates real, material hieroglyphs to an abstract ideal, was further complicated by the damaged quality of the original hieroglyphs and the poor quality of some images. On this point, Thompson expresses his reservations about the catalog in very clear terms, worth quoting at length:

Identification of such damaged specimens from indifferent photographs can hardly be objective. Indeed, decision whether to accept, query, or reject an identification must on occasions have depended on passing mood or the flow of gastric juices. Users of the catalog should bear in mind the subjectivity of my approach, and be prepared to re-examine my very fallible identification before drawing any conclusions. I make no doubt that were I to reclassify the more badly damaged glyphs, I would reach quite a few decipherments different from those now listed; certainly there would be many changes in the employment of query marks, and more would be added than deleted.

Thompson is very clear on this point, emphasizing the burden on researchers using the catalog to check types against the original sources. He even goes so far as to note that his use of an indefinite article (a) – presumably rather than a definite article (the) –

before the term *Catalog* in the title indicates the degree of “subjectivity” characterizing his compilation (Thompson 1962:9). Despite this admitted “subjectivity,” Thompson’s drawings became accepted as the standard for hieroglyphic analysis. For example, in the introduction to a later catalog, Kornelia Kurbjuhn (1989:i) describes Thompson’s drawings as “a glyphic lingua franca” that each convey “the essence of a sign.”

The outcome of this chain of translations is a volume comprised of glyphs arranged in individual listings. Each listing is headed by a glyph number, followed by a clarification of the number of existing examples and reference to the Gray Card numbers as well as the glyph numbers for the individual listings in Gates’ and Zimmermann’s catalogs, if they were included therein. Beneath this basic information, the user of the catalog finds an image of the sign in question. Finally, each entry ends with a list of the original hieroglyphs used to create the abstract type, denoted by reference to their locations in monumental inscriptions and codices. These entries are further accompanied by specification of associated signs, designated by their respective T-numbers. Thompson clearly built on the material and semiotic labor of explorers and archaeologists who produced the documents to which he referred in compiling the list of hieroglyphs. But these terminological conventions typically point users “directly” to examples of each type. Users of the catalog must perform their own epistemic labor to follow the network of aesthetic production back to the original examples, mediated by his addendum on the “Sources of Texts” (Thompson 1962:404-11) and the associated bibliography.

Despite his explicitly expressed uncertainty, Thompson’s catalog has irrefutably shaped the reduction of Maya hieroglyphs to writing, consequentially shaping the way

that signs are designated within epigraphic discourse. Particularly, the specific numbers assigned to each listing, termed “Thompson-numbers” or “T-numbers,” function as semiotic stand-ins enabling specific signs to be designated and discussed in standard typesetting without the need to constantly reproduce the images under consideration. For example, in Chapter 8 I analyze how one particular glyph, designated T539 in Thompson’s system, has undergone transformations as an epistemic object over the course of the past century. While the interpreted semantic value of T539 has shifted dramatically over the past few decades, the sign’s T-number provides a constant means for designating the sign (though contemporary epigraphers seem to prefer the use of nicknames to T-numbers, which may be falling out of fashion, Van Stone 2005:7). In effect, the T-numbers enable hieroglyphs to be translated – in the Latourian sense – from their original aesthetic, material forms into a standardized referential sign such as T539.

Thompson’s catalog has become a crucial document for the standardization of Maya hieroglyphic analysis and discourse. The catalog’s value issues from its power to marginalize the heterogeneity and aesthetic complexity of actual inscriptions, rendering the hypothesized core aesthetic qualities of signs as line-drawings, and eliding the aesthetic variations or frills that may have been distinctive markers of individual scribes or scribal styles. In this sense, the catalog powerfully mediated the production of Maya hieroglyphs as a closed system populated from context-independent actors, cut from associated non-hieroglyph actors and purified into structured units of analysis. The catalog is, thus, a key site of the kind of extrications that enable hieroglyphs to become both stably known facts and exceedingly manipulable objects.

## **Merle Greene Robertson's Rubbings**

Robertson was not principally concerned with exploring the possible linguistic basis of Maya hieroglyphic writing. Yet, her personal connections and aesthetic labor beginning in the 1960s helped to establish the basis for transforming the Maya and their inscriptions as analytical objects. Robertson is an artist and art historian who grew up in Montana, where she learned artistic techniques from the Old Western artist Charles M. Russell and developed an interest in Native American culture through her father, who brought her along on visits to Blackfoot chiefs (Robertson 2006). Robertson traveled to Latin America around 1959 and originally came to pursue Maya imaging through collaboration with a team of University of Pennsylvania archaeologists working at the Guatemalan site of Tikal (Walker and Suárez 2008). Her initial work at Tikal entailed drawing sketches of the hieroglyphic and sculptural elements of the ruins.

While Robertson employed both photography and drawing to document Maya art, imagery, and hieroglyphs, I choose to focus instead on an aesthetic practice that has received much less attention in the history, philosophy, and sociology of science: ink and oil rubbings on paper and cloth. Robertson's rubbings became a new and critically important set of techniques for rendering the monumental inscriptions visible. Here I position these rubbings as a set of particularly consequential objects for rendering Maya hieroglyphs knowable as writing.

This may come across as an unconventional reading, since Robertson did not directly contribute to the turn towards interpreting hieroglyphs as "historical" records comprised of a linguistic script. But Robertson's rubbings brought these hieroglyphs and associated inscriptions into a new regime of vision, deeply shaping not only *how* the hieroglyphs could be known but also – due to the high quantity of rubbings completed

and the mobility of these objects as they circulated among a community of experts – *where* the hieroglyphs could be known.

The example of Robertson's rubbings provides a very tangible, traceable set of material practices through which inscriptions give rise to their simulations. The existing accounts of these practices (Mesoweb 2005; Robertson 2006) enable their summarization in detail. Thus, before I offer any analysis, permit me to review each step of the process. First, Robertson taped paper or cloth to the pertinent stone or stucco monument. While she generally used rice paper, Robertson also employed mulberry paper for fragile inscriptions. She then dampened the paper with a badger-hair brush, chosen on account of the filaments' firm adherence to the handle. Avoiding the hairs' transfer to the paper was crucial to evading artifacts in the simulation. Robertson then pounded the paper into the stone's variegated surfaces using "a wad of men's handkerchiefs or several American Airline washcloths (they generously gave them to us)" (Robertson 2006:50). Next, she would allow the paper to almost dry and proceed to apply either sumi ink or a mixture comprised of black- and burnt sienna-colored Weinsor Newton oil paint. Oil was required for inscriptions at sites in the humid El Peten department of Guatemala because moss that builds up on the monuments' surfaces transfers too much moisture for the water-soluble sumi ink. Due to the tendency of sumi ink to bleed from one adjoining sheet to the other, Robertson also preferred oil for large inscriptions that required the use of multiple overlapping sheets, such as the great carved sarcophagus lid in Palenque's Temple of the Inscriptions. She covered a pad in sumi ink using a palette knife and transferred the ink with a cotton ball wrapped in silk. Or, she covered a piece of tin with a layer of oil paint

and transferred it to the paper with her thumb: “I would gently press onto the tin with oil and then on to the rice paper or cloth, building the intensity of color up gradually until the desired effect was achieved. This took forever as thousands of thumb prints were necessary to complete just one monument” (Robertson 2006:50).



Figure 7-1. A rubbing and a photograph of the carved side of Yaxchilan Lintel 25. Rubbing by Merle Greene Robertson ©Pre-Columbian Art Research Institute, 1995-2007, used with permission. Photograph © Trustees of the British Museum, used with permission.

An example of the resulting images is reproduced and juxtaposed against a photograph of the same monument in Figure 7-1. Through this juxtaposition, readers can discern some significant effects of Robertson’s transformation of the monumental inscriptions into ink or oil rubbings. Since rubbings function on the basis of the water

adhering to raised features of the inscription and repelling the ink or oil as it is applied, the marking blackens the recessed, low-relief lines and spaces. In turn, it leaves the high-relief components of the inscription comparatively unmarked, revealing the white color of the paper or cloth, in some places shaded slightly into gray. The immediate result of this translation is an increased contrast between the inscriptions and the background.

As a particular technique of aesthetic translation, the rubbing elides depth in favor of contrast. The sensuous, material three-dimensionality of the monumental lintel (the differential depth of its relief) is elided or translated into a differential play of shadings in the production of a two-dimensional rubbing (see also Jones 2001). While the photograph is also two-dimensional, the uneven depth of the carving is more clearly indicated through the presence of shadows, a kind of technological trickery that evinces multiple planes of the monument's surface. Thus, the photograph performs a kind of worldliness – even if it is a simulated worldliness – lost in the rubbing. This is the central trade-off of the rubbing as an artistic-scientific mediator. The lines of the lintel become more vividly defined at the expense of cutting away its texture and depth (as well as its color in cases where the monuments were painted and the paint remains). The aesthetic of the rubbing presupposes and instantiates a certain kind of user, or intra-actant. I suggest that the rubbing constitutes a subject presumably content to view the monument's simulation at a distance and at a fixed viewpoint, as if it were a piece of art hanging on a museum wall. While, in terms of perspective, the photograph is also already stabilized, the conscious viewer can perceive the monument's sculptural

dimensionality and thus, perhaps, more easily imagine experiencing it from a different vantage point.

The trade-off in experiential qualities brought out by contrasting the rubbing and photograph exists despite the absence of any obvious blackboxes, such as the camera, in the sequence of steps required to translate stone into inked paper. Each step in Robertson's translation is clearly traceable and characterized by a high degree of contiguity between one mediator and the next. Robertson places the paper directly against the monument and dampens it with a brush. She touches the ink and directly transfers it to the paper. Through these steps, she remains a mediator required to transform the monument into something else, a piece of art and a scientific document. She does not (yet) require instrumental blackboxes that subsume their component actors, those mirrors and shutters and lights. She has not yet been cut from the scene, though the material contexts in which Maya actors intra-acted with the inscription have.

Once the ink dries and the simulation becomes a finalized, definitive record, it can be further transformed in myriad ways. The size of the initial rubbings replicates the monuments that they simulate, with the minor deformations that attend the reduction of three dimensions to two. The bulk of these originals, or secondary originals perhaps, have been archived in The Merle Greene Robertson Collection curated by Tulane University's Latin American Library.<sup>110</sup> But most users of Robertson's rubbings do not directly consult these secondary originals. Instead, they consult facsimiled copies of Robertson's photographs of the rubbings. It is *these* mediated images, or copies of photographs of rubbings of monuments, that circulate widely through the Mayanist community. Thus the rubbing in Figure 7-1 is thrice-removed from the monument, as

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<sup>110</sup> See the collection's online listing: <http://lal.tulane.edu/collections/rubbings/index.htm>.

opposed to the twice-removed “direct” photograph. In Latour’s (1999d, 2005) sense, a “realist” and methodologically robust account is one that slowly and deliberately follows each of these steps of transformation, each of these removes.

While it is initially deferred, the camera becomes requisite to transforming or deforming carved monuments and stucco walls into widely accessible actors. The rubbings serve not solely to preserve the likenesses of the monuments, many of which remain *in situ* and are thus vulnerable to damage caused by humans and nonhumans alike. Through an alliance mediated by the camera, the rubbings and Robertson intra-actively produce a tertiary form, a photograph that can be digitized and indefinitely reproduced as well as flexibly manipulated. Of course, the photographs introduce their own deformations of the rubbings. This is especially apparent in digital photographs, as their standardized pixilation transforms the rubbings’ into artifacts of a queerly technologized pointillism. Here we are pretty far from carved stone.

The savviness of Robertson’s sequence of techniques does not issue from its evasion of technological blackboxes such as the camera. Instead, it issues from the power of the rubbing technique to counteract a particularly loathed artifact in “direct” photographs, shadowing that obscures some of the lines and features (while it reveals others). That the rubbing reduces the monument to two dimensions permits Robertson to avoid photographs with any shadowing at all. While this may be a matter of maximizing the images’ verisimilitude, it also evades the possibility that shadows might remind viewers that the image is the product of a singular instant never to be reproduced, the photographic encounter. The photographed rubbings thus achieve the power to circulate as even more abstract and detached simulations than the “direct”

photographs. They become detached not only from the original monuments but also from the rubbings themselves, which are hidden away in 920 linear feet of boxes shelved at Tulane.

Digital copies of Robertson's rubbings are currently posted online at [www.mesoweb.com](http://www.mesoweb.com), the gateway to much work sponsored by the Pre-Columbian Art Research Institute in San Francisco, which Robertson founded. Thus, anyone with access to the Internet can intra-act with the rubbings, a multiply mediated linkage to the ancient Maya world. We can download, view, print, cut up, and even manipulate Maya hieroglyphs and imagery. We can also readily transform them into line drawings. This presenting of the Maya past thus opens up the possibility for countless acts of further translation or traducement, in the etymological sense of "leading across."

There is a scene relevant to this point at the end of a short documentary on Robertson's career (Mesoweb 2005). Viewers encounter an elderly Robertson, a woman in her 90s, sitting in front of a laptop in her home office. An image of one of her rubbings inhabits the computer's screen. She describes her continued use of computers both at home and in the field. Then she takes control of the laptop and erases a white streak from the simulated rubbing, informing viewers that it should not be there. The aesthetic and rhetorical force of this scene leans heavily on the apparent incongruity of an elderly woman seamlessly utilizing a computer. In watching the scene, I cannot help but to think of Diane M. Nelson's 1990s work on the deft use of computer and Internet technologies by Maya activists. Nelson termed these activists "Maya hackers," explaining that "[t]he incongruity of the term...tends to occasion chuckles, which I think highlights the continuing power of the primitive-modern divide. So I deploy

it as a caution against what I call ethnostalgia” (Nelson 1996:289). In Robertson’s case, it is as if, like the Maya activists and the Maya monuments themselves, she refuses to be relegated to the past.

### **Linda Schele’s Drawings**

This chapter’s third and final figure requires less of an introduction than the preceding two. Schele has already emerged as a primary actor in this study, a protagonist of decipherment if there has ever been one (see Chapter 4). I have already dealt heavily with how Schele’s mediation of the workshops in Austin and Antigua conditioned and intertwined an ethic of public engagement and an aesthetic of the ancient Maya. Thus, in this section, I focus a little more closely on how her specific artistic talents shaped this image of the Maya through skillful, flexible manipulation of the hieroglyphs themselves. Drawing the elements of hieroglyphic inscriptions is not a neutral, objective act performed by someone wholly outside the experiential field in which the hieroglyphs achieve significance in the present. The artist’s imagination is shaped by a preceding body of experiences that render intricate signs that may present a *mélange* of ambiguous forms visible as hieroglyphic units of meaning. In this sense, drawing hieroglyphs is already a theory-laden act of interpretation.

The hieroglyphs have become requisite to how the Maya can be known today, and Schele’s drawings have played major mediating roles in this production of the past. Such mediation of the ancient Maya world is not limited to the professional producers of Maya historical knowledge. Indeed, like Robertson’s rubbings, many of Schele’s drawings (almost 1000) of Maya hieroglyphs (as well as images and some architecture) are freely accessible on the internet, archived as the Schele Drawing Collection through website of the Foundation for the Advancement of Mesoamerican studies, Inc.

([www.famsi.org](http://www.famsi.org)). The original drawings have been bequeathed to The Nettie Lee Benson Latin American Collection at the University of Texas at Austin. Thus, actors involved in the production and simulation of Schele's drawings have rendered these objects, like the rubbings, actionable and consequential at a distance from both the original inscriptions and the original simulations.

Schele was adept at drawing and she developed her own techniques for rendering hieroglyphs. On the FAMSI website there are two introductions to her drawings which provide crucial information for apprehending the specific techniques that she employed as well as the techniques used to simulate the hieroglyphs in digital form. In his introduction, Schele's colleague in decipherment Peter Mathews (2003) praises Schele's drawings for both their accuracy and aesthetic qualities: "When combined with her intimate knowledge of imagery and hieroglyphs, her skills as an artist enabled her to create simultaneously beautiful and accurate records of the monuments and objects from the Mesoamerican world she loved." Thus, how did this "intimate knowledge" manifest a materially consequential imaging practice for visualizing hieroglyphs and positioning them as central objects for knowing the ancient Maya?

Though the steps required to produce drawings are less determinate than those entailed in producing rubbings, I can still locate the general practices and materials necessary to this aesthetic rendering of ancient Maya objects. Mathews (2003) informs potential users of the FAMSI collection that Schele rendered her drawings with Mylar drafting film. During the 1970s and 1980s she employed technical drafting pens and China Ink, and by the 1990s she had transitioned to felt-tipped pigment liner pens. Of note to an analysis of Schele's imaging practices, Mathews (2003) adds that she

typically utilized pens that could produce very fine lines, with thickness sizes ranging from 3x0 to 2 in the United States drafting standards (equivalent to 0.25 to 0.60 millimeters). Such pens intra-acted with Schele in the production of simulations with high degrees of calligraphic and iconic accuracy. In his introduction to the FAMSI Schele Drawing Collection, Matthew H. Robb (2003), an assistant curator of ancient American and Native American art at the Saint Louis Art Museum, offers some additional detail concerning the digitization of Schele's drawings for the FAMSI website. The drawings were photographed using 35-millimeter slide film to produce white-on-black negatives. FAMSI proceeded to scan the negatives and adjust and compress the digital images for reproduction as the Schele Drawing Collection.

Thus, we can follow the set of translations necessary to making hieroglyphs and images originally rendered with paint or stone into paper or pixels. As I have noted, Robertson's lithographic images are not direct simulations of the monumental objects that they document. They are, rather, images that serve the purpose of productively reducing the complexity, dimensionality, and sensual materiality of their objects. They emerge as forms partially connected to and partially cut from the original inscriptions. This strange mixture of connection and disconnection imbues them with scientific sense and legibility in a material-semiotic context distanced from the Maya monument. While commendable in their verisimilitude, Schele's drawings clearly enacted similar forms of reduction, particularly the translation of three-dimensional objects into two-dimensional images.

Yet these two series of translations also differ in substantial ways. In the initial production of the rubbings, Robertson's practices required a series of direct material

contiguities. As described above, rubbings entail practices of material contact that simulate carvings' or moldings' contours of differential depth as a pattern of (nearly) binary colors. In the stead of material practices bringing several nonhuman actors into direct contact, Schele's drawings entailed a spatial distancing of the artist and her tools from the object being simulated. In other words, as in any freehand drawing, the artistic subject's field of phenomenal perception and capacity for bodily self-regulation mattered somewhat more in Schele's freehand drawings than in Robertson's rubbings. The act of drafting and redrawing inscriptions from either the original object or images of that object requires a high degree of precision and accuracy. Indeed it is a kind of competence that draws on and re-instantiates multiple forms of knowledge. The quote from Mathew's (2003) introduction to the Schele Drawing Collection clearly makes reference to the conjunction of these different skills. Producing line drawings of Maya inscriptions heralded for their accuracy required preceding knowledge of the symbols employed and the hieroglyphs as well as artistic skill (see Coe and Lebrun 2005).

This may seem obvious to readers, but I defend it as a point worth noting, especially since a primary challenge of simulating Maya inscriptions by hand is discerning how the abundant and intricate intersecting lines and shapes are to be parsed. While the drawings are invoked as evidence for specific interpretations, they are heavily shaped by the producer's ideological and interpretive dispositions. Prerequisite knowledge includes the established conventions for drawing hieroglyphs that predecessors have employed. As I have touched on throughout this study, these conventionalized dispositions involve a capacity to extricate or cut the hieroglyphs away from various co-actors in their fields of association, including architecture, imagery, and

environmental actors. Perhaps even more important to the simulation of hieroglyphs as line drawings is the capacity to distinguish different elements internal to the hieroglyphic signs. Indeed “distinguish” is a very conservative verb for describing this act. I suspect that such parsing actually involves acts of performative constitution that render hieroglyphs accessible as objects of knowledge in the present.

Possibly the most infamous, and perhaps even humorous (in retrospect), case of how an interpretive disposition may shape the form of simulated Maya hieroglyphs can be found in drawings that “philosophical traveler” Jean-Frédéric Maximilien de Waldeck rendered in the 1830s, which were later published in the 1866 volume *Monuments anciens du Mexique* (coauthored by Charles-Étienne Brasseur de Bourbourg). Waldeck was French by birth but claimed multiple European citizenships. His philosophical travels took place at a time in which North Americans were coming to displace Europeans in the production of significant travel writing on Latin America (Evans 2004). This ideological backdrop seems to have shaped Waldeck’s images of both monuments and hieroglyphs. Waldeck’s images evidence an implicit defense of trans-Atlantic (or trans-Pacific) diffusionist theories of Mesoamerican cultural origins. He drew monuments with markedly Egyptian features and went so far as to discover elephants in the inscriptions as well as in the decorative architectural elements of Late Classic buildings in Yucatan. Did Waldeck “know” that geopolitically charged trans-Atlantic contact theory shaped how he simulated the inscriptions? In his refutation of various claims to the presence of elephant images in American art and architecture, Thompson (1927:393) claimed that Waldeck “may...have ‘read into’ the original figures elephants’ heads which actually never existed.” But Thompson, like other commentators, does not

venture to address whether Waldeck really saw the elephants, or whether he conjured them from thin air. Regardless of the implausibility of discerning intent, this case presents a hyperbolic example of how apparently neutral acts of simulation actually occur within regimes of vision that provide evidence for one side of a controversial claim. In the epistemic terrain of decipherment-era epigraphy, the semantic intricacy of hieroglyphic lines and shapes has been amplified greatly, such that acts of imaging as interpretation are bound to carry much more nuance than the Waldeck example.

Drawing hieroglyphs is not an act of “mechanical reproduction,” as in Robertson’s rubbings. Instead, like Thompson’s (1962), it conforms more closely to what Daston and Galison (2007) term “trained judgment” and identify as a principle virtue of twentieth-century science. Daston and Galison (2007:311, emphasis in original) identify the shift from mechanical reproduction to trained judgment in these terms: “Slowly at first and then more frequently twentieth-century scientists stressed the necessity of seeing scientifically through an interpretive eye; they were after an *interpreted image* that became, at the very least, a necessary addition to the perceived inadequacy of the mechanical one.” The shift to trained judgment attended the rising prominence of psychology and concern with the unconscious in the first half of the twentieth century. Scientific discoveries were no longer the privileged outcome of industrial diligence. Instead a rising trend in scientific communities was to perceive advances as the results of interplay between willfully patient empiricism and machinations of the unconscious, that source of “aha! moments” (which line the pages of some of Schele’s more popular publications [esp. Freidel et al. 1993] and the Maya Meetings Proceedings).<sup>111</sup> I should

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<sup>111</sup> Explicit use of the “aha! moment” trope that circulates in popular representations of science has its own history and function within the disciplinary politics of Maya hieroglyphic studies. In the opening

note that while it is convenient to present the transition from Robertson's mechanical reproduction to Schele's trained judgment as an epistemic shift that seems to follow (at least in sequence, if not in historical era) Daston and Galison's historical chronology, this is somewhat misleading, and not solely because Robertson produced her own line drawings. Indeed, travelers, antiquarians, and Mayanists have, of course, employed (variably "trained" degrees of) judgment in distinguishing the shapes and forms of hieroglyphic inscriptions since the late eighteenth century. It nevertheless remains significant that Schele's own popular narratives of discovery (esp. Freidel et al. 1993) revel in the activity of the unconscious to a degree unprecedented by earlier accounts.

Schele exercised a certain capacity for trained judgment in the simulation of Maya hieroglyphs by carefully building on and diverging from her predecessors in drawing inscriptions. This is the reason that Schele's use of very finely tipped pens is such a crucial technological locale for analyzing her contributions to decipherment. One of the primary points of aesthetic slippage or cutting in the production of hand drawn simulations concerns the width of lines that define boundaries of different elements in the inscription and require high degrees of calligraphic precision to simulate. This is particularly significant in the case of monumental limestone carvings that did not use lines of finite thickness to construct the boundaries of hieroglyphs and other elements. Carvings' contours frequently lack discernable or fixed boundaries and employ curves of differential relief, a topological play of depth that drawings inevitably traduce. Thus, when simulating a carving as a line drawing, Schele, like any other subject, was

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chapter in an important volume for the establishment of phoneticism as a viable research topic among epigraphers (Justeson and Campbell 1995[1984]), Lyle Campbell (1995[1984]) attributes the experience of "aha! moments" to the interactions between linguists and epigraphers, whose analytical repertoires served as necessary complements during the methodological transformation of epigraphy in the 1980s.

continuously required to make judgments regarding the relative boundaries of semantically significant lines. In this sense, the use of such finely tipped pens reflects not just a degree of precision in the imaging practices, but also a degree of discretionary confidence in how to faithfully marshal the qualities of ancient and often highly damaged objects, giving these qualities a scrupulous material and aesthetic manifestation on an entirely new template.

In addition to the acts of discretion that drew constantly on her predecessors' practices and her vivid imaginary of ancient scribes' intentions, some of Schele's line drawings also indulged a particularly significant set of cuts for this study: the dissection of inscriptions into units considered semantically autonomous. In Chapter 5 I have already provided an analysis of the cutting – in the most literal sense possible – requisite to forms of structural analysis in workshop pedagogies. Here I wish to push this analysis further by attending to how the production and simulation of line drawings mediated by Schele (and, I acknowledge, other less prolific and accurate producers of drawings) has further instantiated the onto-epistemological and semiotic presumption that the Maya hieroglyphic signs could be cut not solely from their indexical fields and material contexts but also from associated ostensibly non-hieroglyphic components of the inscriptions.

In the case of stone monumental inscriptions, the separation of clearly textual hieroglyphic components from the apparently non-textual images or representations is a modernist practice of purification. In my use of the term “modernist practice,” I am not merely drawing attention to the underlying ideological presupposition that Maya hieroglyphs have fixed meanings that can be known with certainty and finality in the

present, with the act of apprehending these meanings perceived as lacking recursive or performative effects. Clearly this is the case. But, furthermore, I wish to designate acts of interpretation that are shaped by material translations distinctive to “modern” technologies and media.

For example, the phenomenological experience of a large monumental panel may not readily permit the intra-actant to absolutely separate its imagery and hieroglyphs. However, once this panel is drawn, reduced in size, copied, and digitized, it becomes possible to experience it in entirely new ways. One of these new modes of experience is clearly the differentiation of text and image. This contemporary technological practice has implications for the interpretation of Maya hieroglyphs. Namely, it provides an always unstated aesthetic basis for the modernist epistemological premise that the primary, if not sole, function of Maya hieroglyphs was congruent with the function of today’s writing systems: to provide a standardized set of transposable markings that convey fixed morphemic values in the absence of direct intersubjective contact. I suggest that cutting hieroglyphic from non-hieroglyphic components of monumental inscriptions has tended to confer aesthetic and interpretive primacy to the hieroglyphs relative to other components of the images, a problem that Mayanist Karen Bassie-Sweet (1991) has also discussed in an example that I analyze below. Furthermore it has created an aesthetic ground for interpretive practices that required, and rarely explicitly acknowledged, a presumption that the ancient Maya considered the hieroglyphs to be a transposable writing system that could be divorced from its material contexts.

I do not wish to throw the baby of decipherment out with the bathwater of modernist writing technologies. But I do aim to draw attention to how epigraphic imaging practices have helped to render Maya culture a singularized totality and established an epistemological hierarchy that disvalues interpretations that do not lend interpretive primacy to hieroglyphs.

Here I find it illustrative to concentrate on a single example of the material and aesthetic practices that invite my intervention here. Figures 7-2 and 7-3 reproduce Schele's line drawings of the Palenque Palace Tablet (Schele Numbers 121 and 124). The images comprise, or foreground, different components of the tablet. As the description of the panel's image on FAMSI's webpage makes clear, Schele's simulation reproduced as Figure 7-2 represents the outline of the tablet in its entirety, yet it omits the internal lines of a scene depicted in the upper, central portion of the tablet. There is thus a stark aesthetic differentiation constructed between the figures outlined in the scene and the hieroglyphs that surround the scene on three of four sides. But Schele did not pass over drawing the scene altogether. Indeed, Figure 7-3 reproduces her line drawing of the components omitted in Figure 7-2, revealing the outlined shapes to be representations of three seated interacting figures, two of which hold objects. This second line drawing further reveals that in differentiating the text from the scene, Schele and FAMSI have also outlined an additional hieroglyphic component, the "secondary text" internal to the scene.

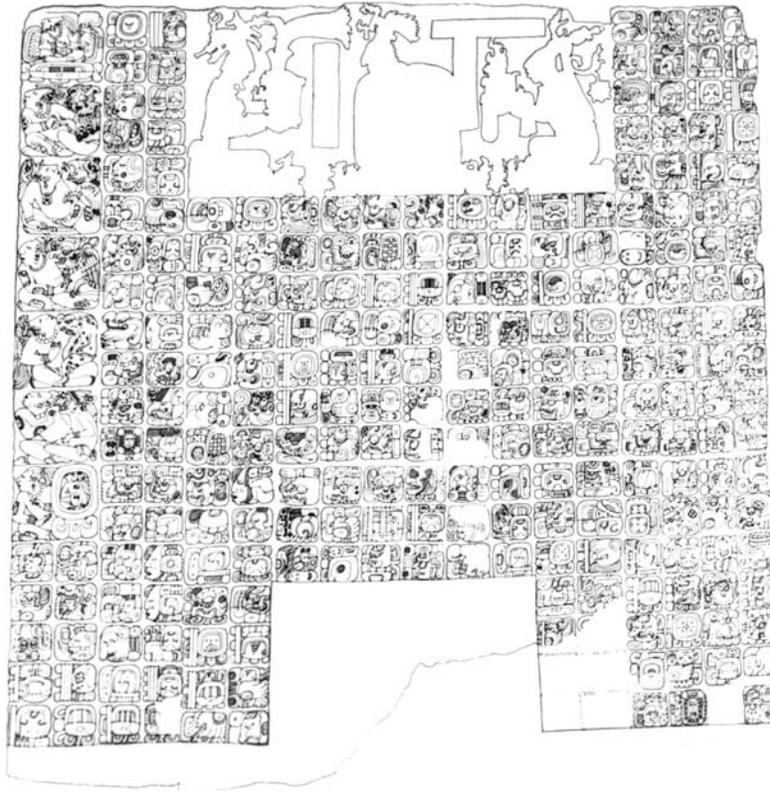


Figure 7-2. The Palenque Palace Tablet primary text. Drawing by Linda Schele, © David Schele, courtesy Foundation for the Advancement of Mesoamerican Studies, Inc. [www.famsi.org](http://www.famsi.org).

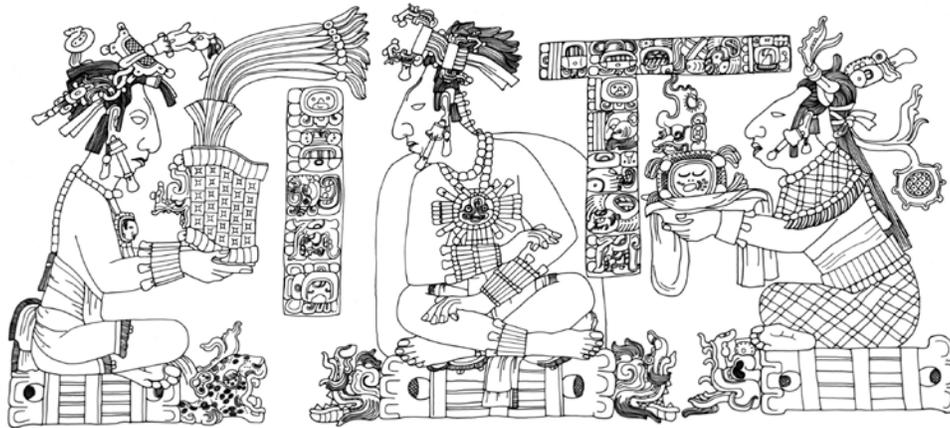


Figure 7-3. The figures, secondary text, and associated objects on the Palenque Palace Tablet. Drawing by Linda Schele, © David Schele, courtesy Foundation for the Advancement of Mesoamerican Studies, Inc. [www.famsi.org](http://www.famsi.org).

How does such differentiation shape the ways in which this particular tablet can be interpreted? Namely, the text and the image become construable as semiotically independent forms. I hold that this aesthetic practice performs an absolute distinction between hieroglyphic and iconographic components of inscriptions, and that this distinction necessarily elides the original experiences and uses of the tablet. Such elision is perhaps most provocative in cases where the imagery and secondary text are reproduced without indication of their association with the primary text. Most importantly, I submit that this performative act gives rise to the distinction between image and writing implicated in the interpretations (or, at least, it reveals the practice as a disposition that exists regardless of whether it manifests aesthetically in drawing practices). This requires a somewhat detailed overview of the interpreted content of the text and the represented scene.

I should note that there is not complete consensus among Mayanists concerning the relationship between image and text in the inscriptions.<sup>112</sup> Bassie-Sweet (1991) has offered the most detailed and explicit conceptualization of the text-image relation (see also Herring 2005 for a discussion of the influence of hieroglyphic calligraphy on imagery techniques). Bassie-Sweet notably diverges from Schele (and Schele's close colleagues) in attending not solely to the content of the text but also to the experience of reading the text. Greater attention to the relational fields of the signifying objects leads Bassie-Sweet to argue that the spatial placement of texts relative to their accompanying images entails a specific framing convention that identifies the protagonist of the text.

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<sup>112</sup> Additionally note that text and image are not the only aesthetic-semiotic categories employed in the analysis of Maya inscription practices. There is also rising attention to "pseudo-glyphs," or signs that appear to imitate hieroglyphs without conforming strictly to the morphological or syntactic conventions of hieroglyphic writing (see Calvin 2006 for the most extensive study of pseudo-glyphs to date).

To clarify, many monumental inscriptions may contain images beside, below, or above the text itself. Sometimes, as in the Palace Tablet, there are also components of the hieroglyphs internal to the image area, so-called “secondary texts” (see Figure 7-4). For these examples, Bassie-Sweet (1991:38) argues that the spatial progression of the syntax itself identifies the central actor and action of the text within the image. In other words, the interruption of the reader’s line of sight requisite to complete the reading of a particular unit of syntax is not incidental, but a necessary experiential dimension of monuments as text-artifacts.

In other words, Bassie-Sweet (1991) claims that this interpolation of a figure into the reader’s perceptual field is an aesthetic technique used to reveal the central actor and event in the more elaborate accompanying text, which often recounts multiple events enacted by multiple subjects. I push this phenomenological orientation one step further, refracting it with something closer to object-oriented ontology. Thus, I am appropriating Bassie-Sweet’s perspective, and probably failing to stay true to her intention, translating and traducing it for my own analytical ends. Namely, I argue that it is the actual *ontology* grounding how actors distinguish these semiotic objects, and not merely the *relation* installed between text and image, that has been construed too quickly as a foregone conclusion in Maya hieroglyphic interpretation. To simplify somewhat, this can be understood an issue of framing, an issue of what scale we must assemble in order to locate, stabilize, and render visible, however momentarily, the meaning of particular objects, monumental texts as actor-networks.

To illustrate my analysis, I am turning briefly to a particular monumental text for which Schele (1978) and Bassie-Sweet (1991) have proposed contradictory

interpretations. As it is currently understood, the text on the Palace Tablet (Figure 7-2) recounts an important series of events in the history of rulership at Palenque. Schele (1978) offered the first summation of these events in an important Palenque *Mesa Redonda* paper published in workshop's proceedings, and Robertson (1985) presented an updated version of this position in the third volume of her four-volume series *The Sculptures of Palenque*. According to these accounts, the events recounted in the text all pertain to the rulership of K'inich K'an Joy Chitam II (whose name was formerly read by epigraphers as Kan-Xul II and as Kan-Hok-Xul) from 702 to 711 CE. Schele determined that K'an Joy Chitam is the "protagonist of the text" based on the observation that he "is the subject of the majority of the events recorded" on the tablet (Schele 1978:53). Yet, as a point relevant to comparison with Bassie-Sweet's account below, she also does not absolutely discount the possibility that the central figure might instead be the ruler Xoc (Schele 1978:61). This account holds that the Initial Series date that opens the text marks the birth of K'an Joy Chitam. He is then identified as the child of the former ruler K'inich Janaab' Pakal and his presumed wife Ix Tz'akb'u Ajaw. The text proceeds to record the dates of a series of significant events. For example, K'an Joy Chitam's title (and perhaps status) changes, Pakal performs a significant ceremony and dies, Pakal's elder son K'inich Kan B'alam II (whose name was formerly read as Chan-Bahlum II) accedes to rulership and dies, and K'an Joy Chitam accedes to rulership.

Schele and Robertson contend that the scene (Figure 7-3) accompanying the text depicts this accession as a ceremony in which the parents of K'an Joy Chitam (seated in the center) present him with offerings that symbolize the rise to the office of king. On

the viewer's left, Pakal presents a so-called "drum-major" headdress. On the viewer's right, his mother Ix Tz'akb'u Ajaw presents a bowl with a hanging cloth, a bundle cover opened to reveal what is interpreted as the image of a slightly anthropomorphized (or deified) shield supporting above it the figural form of the flint-shield god shown in profile (Robertson 1985; Schele 1978). In effect, Schele and her followers presume that the primary text provides the essential storyline for the monument and that the scene, which is presumed to clearly depict an accession ceremony, represents the culmination of the sequence of events recounted. From this interpretive stance, the secondary text emerges as a caption to the imagery. This logic leads Schele to have to reconcile the imagery with data from hieroglyphic inscriptions that mark the death dates of K'inich Janaab' Pakal and Ix Tz'akb'u Ajaw, both preceding the date for the accession of their son, K'an Joy Chitam. In other words, if we accept the logic of Schele's readings, we must follow in her assumption that the flanking figures on the tablet are represented in postmortem states.<sup>113</sup>

In her conflicting interpretation, Bassie-Sweet (1991:214) extends to the Palace Tablet Kathryn Josserand's (1987) assertion that the secondary texts of tablets from the Palenque Cross Group should be read before the "main" text. Bassie-Sweet lends

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<sup>113</sup> The argument for these figures' postmortem states relates to epigraphers' claims to access Maya cosmology, which I discuss further in Chapter 8. Here, Schele invokes several pieces of evidence to justify the argument that the flanking figures on the tablet are represented in a postmortem states. Her most crucial claim is that the water lily pads that adorn the figures' headdresses denote their status as otherworldly figures. This iconographic interpretation is rooted in Thompson's (1971[1950]:72) claims that the water lily had integral associations with the three-layered Maya cosmos. Thompson interpreted the water lily as a symbol of the structure of the cosmos, with the water lily pad equated with the back of the alligator or earth monster, or *imix* (the first day name of the Tzolk'in calendar), which served as the stage for human life. Thompson identifies the other two layers of the cosmos with the sub-aquatic world and the sky, and notes that underworld denizens could travel between these two levels without passage through the middle world, the alligator's back or water lily pad. That these pads adorn the heads of the flanking figures indicates to Schele that they are occupying a cosmological plane below that of the earth: "The water-lily headdresses mark the parents as supernaturals" (Schele 1978:64).

interpretive primacy to the experience of reading the caption text. Particularly she notes that to read the caption text, the viewer's line of vision must pass across the central figure's body. The secondary text begins with the column of hieroglyphs to the left of K'an Joy Chitam and proceeds to the columns to the right side. That this pragmatic shift in the user's line of vision occurs in the middle of a sentence concerning the first lineage event of a ruler named Xoc indicates to Bassie-Sweet (1991:214) that he – and, by implication, not K'an Joy Chitam (Kan Xul II) – is the central actor of the narrative.<sup>114</sup> Rather, building on a 1987 personal communication from David Stuart, she notes that K'an Joy Chitam may play a primary role in the narrative because he was Xoc's ruler. In this reading, Xoc was a "subsidiary" lord. Bassie-Sweet does not provide much further detail concerning the implications of this competing interpretation for the meaning of the tablet, and particularly the flanking figures. Though, she does clarify how this shift in the identification of the tablet's protagonist should impact interpretations of the rooms of the Palace, which had been attributed to K'an Joy Chitam as a result of Schele's style of interpretation.

Bassie-Sweet (1991:215-6) attributes Schele's interpretation of the main figure as K'an Joy Chitam to the prominent role played by this ruler in the main narrative. The purpose of my intervention here is to indicate how this is an incomplete explanation, and that a more compelling explanation would have to consider how Schele's and Bassie-Sweet's interpretive practices hinged on contradictory logics of the relation between image and text, which implicated contradictory notions of the object's cultural function. That Schele's analyses of both hieroglyphs and images with embedded symbols

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<sup>114</sup> Note that epigraphers now consider the identification of "Xoc" a misreading, indicating that such a ruler did not actually exist.

involved dissecting the units of meaning into their own individualized component parts effaced the plausibility of interpretations that attended more closely to the phenomenal field within which the tablet originally functioned.<sup>115</sup>

In other words, these contrasting dispositions entail different scale-making practices. Schele constructed a scale of analysis in which hieroglyphs functioned as central mediators, and “epistemic things” (Rheinberger 1997) that provided the interpretive key to the aesthetically dissociated images and, by implication, the material sites in which the monumental inscriptions were situated. Bassie-Sweet, on the other hand, valorized a scale in which text, image, and viewer were treated as mutually imbricated within the tablet’s phenomenal field. Regardless of the epistemic statuses of these arguments today – statuses that are largely irrelevant to my intervention – I am intrigued by how these two authors differentially identify which actors mattered in the interpretation of the text. Schele’s epistemic practices were heavily determined by a reductive logic, an attempt to dissect the tablet into the most basic units of meaning. As I will discuss in the following chapter, such reductive work was, of course, integral to the advances in hieroglyphic knowledge production beginning in the 1960s. On the other hand, Bassie-Sweet’s orientation to how the interplay of the image and text shaped the message that it conveyed to the viewer fits comfortably within the late-1980s and early-1990s turn to greater attention to participatory frameworks within linguistic anthropology, including Mayanist linguistic anthropologist William Hanks’ (1990) examination of the interplay between Yucatec speakers’ bodies and the surrounding material landscape in the use of deictic expressions.

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<sup>115</sup> While I have chosen the Palace Tablet as a salient example to make this argument, one could also analyze the effect of differing interpretive dispositions on the identification of important figures on other monuments, such as the Palenque Cross Group tablets.

Ultimately, the aesthetic detachment of the hieroglyphs and associated images served the broader purpose of constructing hieroglyphs as autonomous windows into the ancient Maya world. In effect, the primacy attributed to the hieroglyphs became a kind of blinder, at least implicitly relegating dimensions of the ancient Maya world inaccessible through the glyphs (which is to say almost the entirety of social practices within ancient Maya society) to a subordinate status. This, at least, is apparent. What is less apparent but of equal value to the set of questions that I am crafting here is that the act of deferring the significance of acts and actors inaccessible through the hieroglyphs was complicit with a very narrow understanding of the operation of hieroglyphs as actual signifying objects no doubt performing multiple kinds of semiotic work within their ancient Maya contexts.

There is something of a contradiction here. Robertson and Schele were drawn in part to the images and hieroglyphs because of these objects' intricacy, complexity, and aesthetic beauty. This attraction is quite apparent in Robertson's passing comments on the glyphs. Regarding the Palace Tablet (Figures 7-2 and 7-3) alone, Robertson (1985:54) notes that "[t]he individual glyphs are not only beautiful pieces themselves but are in some instances whimsical and amusing," and that "[t]he row of Full-Figure Glyphs at the left of the tablet are rare and considered by many the most beautiful known of this type." Such aesthetic evaluations of Maya hieroglyphs have a long genealogy and attest, at least indirectly, to the autonomous power of the hieroglyphs to draw in and shape co-actors in their world. But the aesthetic practices that extricate these artifacts from their worlds of material association and make them knowable as writing are consequential reductions. The various alternatives to interpreting hieroglyphs as texts

have dissolved. That the figures on the Palace Tablet physically wear hieroglyphs – name tags on their headdresses – and that part of the bundle presented by the figure on the right is a hieroglyph no longer troubles the interpretation. That Robertson rightly notes that the full figure glyphs on the Palace Tablet are remarkable aesthetic forms does not give rise to questions concerning the epistemic violence entailed in reducing these anthropomorphic figures to modernist logics of text. Through practices of aesthetic transformation, these complex and heterogeneous objects became two-dimensional, in more ways than one.

### **Conclusions**

Thompson's, Robertson's, and Schele's images continue to play intertwined roles in the production of Maya historical knowledge. Epigraphers actively deploy Thompson's types and manipulate Schele's drawings to inspire new epigraphic readings. These readings provide the basis for interpretations of the cultural significance of the monumental tablets that Robertson imaged. The power of such historical imaging practices resides in how they can be cut from the subjects who produced them, subjects productively animated by their individual idiosyncrasies, biases, and desires. The simulated hieroglyphs and monuments come to substitute for a world that has, in some *and only some* ways, crumbled into fragments and dust.

Certain images float to the surface of the public and scholarly imaginary of the Maya; they become the primary technologies shrouding all other articulations of the Maya, with many other enabling intermediaries occluded beneath (as well as those errant actors that protest their subsumption). Thompson, Robertson, and Schele each reanimated and re-performed the ancient Maya. Through practices that more regularly effaced distinctions between art and science than invoked them, these figures made the

ancient Maya imaginable within contemporary social ideologies. As I discuss in the following chapter, this transformation of the ancient Maya even entailed specific material and interpretive practices that can be located by tracing the transformations of a single hieroglyphic sign.

## CHAPTER 8 TRANSLATIONS AND TRANSFORMATIONS OF THE “HALF-SPOTTED AHAU”

### **Introduction**

In the introduction to an edited volume of contributions to the decipherment of Maya hieroglyphs, epigraphers Stephen D. Houston, Oswaldo Chinchilla Mazariegos, and David Stuart (2001:3-7) include a brief section on philosophical approaches to their field. Here, these prominent and publicly-influential Mayanists (Houston and Stuart are MacArthur Fellows) outline their philosophical position, presenting the case for a “modified” realism holding that “science is both in and of society” (Houston et al. 2001:6, 7) and acknowledging that historical assertions have contemporary consequences. They effectively offer readers an epistemological compromise as they claim that decipherment takes place within a cultural context while they also maintain the stance that “Maya glyphs exist out there” (Houston et al. 2001:7) and have fixed meanings discernable through scientific methods. They further seem to draw a line between “close decipherments of sound and meaning” (Houston et al. 2001:7) accessible through linguistic methods and the more anthropological facet of ancient Maya studies, entailing the interpretation of cultural meanings and translation into contemporary languages.

In their brief description of the cultural context of decipherment, these authors attempt to explain convergent interpretations, or discoveries, proposed by independent researchers, without recourse to “either a mystical process of epistemological maturation (Kroeber 1917) or triumphal convergence on the path to truth” (Houston et al. 2001:6). While their alternative position is not entirely clear, the authors point out how divergent research rationales or personal dispositions may incline different

epigraphers to arrive independently at the same interpretation. The authors cite the decipherment of what they term, “the way glyph, a sign for ‘companion spirits’” (Houston et al. 2001:6), as an example of how this works. Describing this set of events, Houston and colleagues (2001) relate that Houston and Nikolai Grube arrived independently at a new interpretation of the same glyph at approximately the same time in 1989. They further claim that the inspirations for this common reading clearly differed. On the one hand, Grube came to the reading through his interest in imagery and Yucatec ritual practices, while Houston arrived at the same interpretation through epigraphic methods emphasizing patterned phonetic substitutions. While Houston and colleagues (2001) acknowledge that the cultural meaning of spirit companionship for the ancient Maya remains an open question, they hold that the core decipherment of this glyph as *way* is a firmly established fact, evidenced in part by Houston’s and Grube’s interpretive convergence.

In this chapter I offer a new analysis of how this glyph came to be read as a logograph for *way*. I should clarify up front that my concern is not solely with the epistemic status of a single decipherment. Rather, I am more broadly concerned with the fundamental issue of how specific actor-networks emerge through Maya hieroglyphic decipherment’s dominant epistemic practices. I assume that such actor-networks are shaped by and shape Houston and colleagues’ (2001:6) understanding of the field’s epistemological ground, which they describe through a highly simplified summarization of given alternatives in the philosophy of science: “a basic conflict exists between those espousing philosophical ‘realism’ – the notion that our scholarly accounts reflect real truths in the world – and ‘antirealism,’ which holds that ‘theories of

science are...useful fictions, convenient methods of representation' (Kourany 1987:338).” In presenting their modified realism, thus, Houston and colleagues (2001) establish a spectrum ranging from realism to antirealism as the only epistemological option. They do not take into account alternatives to this spectrum, including actor-network theorists' (Callon and Latour 1992; Latour 1993, 1999a) and feminists' (Haraway 1997; Harding 1986, 1998; Strathern 1991) claims that the realism-antirealism spectrum is fundamentally flawed.<sup>1</sup>

In what I consider to be his most compelling argument against the legitimacy of a realism-antirealism epistemological polarity or spectrum, Latour (1999a; see also Chapter 2) traces the material practices of translation entailed in soil scientists' research. Latour (1999a) follows scientists as they transform measurements taken in the Amazon into published articles concerning shifts in a boundary between zones of forest and savannah. The relevance of Latour's (1999a) argument for this chapter resides in his refusal to accept either a straightforward version of representational realism – that texts can adequately and accurately represent an external world – or a straightforward version of representational antirealism – that there exists an unbridgeable gap between texts and the world. He offers an empirical, ethnographic argument against these competing philosophical positions, the claim that representations are materially linked to their worldly referents through series of material mediations or translations. In the stead of an alternative between a representational correspondence between text and world, on the one hand, and a representational abyss, on the other, my Latourian stance holds that these translations result in “partial

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<sup>1</sup> Note that a version of Latour (1999a) was originally published four years earlier as an article (Latour 1995).

connections,” as such articulations have been described by Donna Haraway (1991[1985]:181) and Marilyn Strathern (1991).

To bring a logic of partial connectedness to the practice of producing epigraphic interpretations is to emphasize that arguments emerge out of embodied and material linkages among relational objects (see Chapter 2). Since this metaphysics holds that objects cannot reproduce or enter into new relational configurations without some deformation, a Latourian understanding of epigraphic knowledge practices necessarily maintains that contemporary readings are not equivalent to the original meanings of hieroglyphic objects. Rather, contemporary interpretations achieve functional connectedness to antecedent meanings as an effect of the assemblage – or network of partially connected, ontologically heterogeneous material associations – that their emergence renders.

Thus, from my position it is untenable to assume that contemporary hieroglyphic readings directly identify, represent, or reinstate the original semantic values of particular glyphs. They are no longer connected to their original semantic values, which, in turn, no longer exist in their original form.<sup>2</sup> However, partial readings of Maya signs remain a plausible and viable objective for those who adopt a Latourian metaphysics. Namely, it is possible to mediate the articulation of specific hieroglyphic

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<sup>2</sup> Though he occasionally addresses the issue explicitly, I maintain that Latour has insufficiently conceptualized the operation of time within his metaphysics. As in Gilles Deleuze and Félix Guattari’s (1987[1980]) use of organic objects – the rhizome, the tree – as models for competing processual logics, Latour’s emphasis on “networks” and “assemblages” seems to prioritize spatial, as opposed to temporal, dimensions of material transformation. I think that this prioritization of the spatial haunts attempts to mobilize Latour’s object-oriented philosophy for historical analyses. For the sake of argument, I should clarify that I embrace, with some hesitation, an understanding of time as an *a posteriori* rationalization – i.e. a cognized trace or effect (and one not distinctly limited to humans) – of shifting material relationalities. I additionally supplement this notion of time with a concept of temporality that emphasizes the nonlinearity of recursion in material processes, such that the differential consequentiality of past actions remains continuously contingent or under revision (Rheinberger 1997).

signs with morphemes situated in distinct relational configurations. One could argue that a specific reading positions a hieroglyphic sign (or its simulations, to be more accurate) in a relation of productive contiguity with a morpheme postulated through the material knowledge practices of historical linguistics. I maintain that the utility of this partial connection depends further on the articulation of the proposed sign-morpheme alliance with traces of the specified material and deictic context in which the morpheme hypothetically acquired its postulated semantic value. Furthermore, I maintain that such “readings” explicitly incorporate an acknowledgement of the material practices and alliances requisite to their articulation, thus making it abundantly clear that readings remain perpetually open-ended and under revision.

In this chapter I focus on the material practices and alliances requisite to the articulation of a specific sign with the morpheme *way*. I hold that the glyph in question came to position itself materially and aesthetically as a central organizing object in a variegated material assemblage simultaneously shaping Grube and Houston, despite their geographical distance. In terms of methodology, I maintain that explaining the convergence of a particular hieroglyph and the morpheme *way* requires close attention to the series of transformations that have attended the hieroglyph, as both an artifact and a sign, during its movements through distinct media, the material sites of Maya hieroglyphic decipherment. I trace how this sign passed through disparate media, becoming allied with and cut from a series of distinct morphemes. Moreover, I show how each of these alliances emerged out of materially- and historically-specific configurations that rendered the decipherment of *way* epistemologically sound and efficacious.

I am thus sketching how a particular actor has emerged and transformed into a “deciphered” hieroglyph. While the glyph previously occupied a rather unremarkable place in the field’s actor-network, it began to consolidate and influence a wide range of allies by the 1980s and the 1990s, positioning itself as one crucial node (among many) through which the ethical and epistemological stakes of hieroglyphic decipherment came to be articulated.

### **How the *Ahau* Got Its Spots**

This section traces how a range of signs became classified as variants of a single hieroglyphic type. The key identification of this glyph that remains consequential today was published in Thompson’s (1962) catalog, which I analyzed briefly in Chapter 7. In his catalog, Thompson (1962) nicknames this particular glyph the “half-spotted *ahau*” (Figure 8-1).<sup>3</sup> The nickname indicates that Thompson considered the main sign’s spots to be a graphic property distinguishing it from other hieroglyphs. It is important to remember that Thompson was working within an interpretive logic that treated hieroglyphs as an abstract system of icons and rebus-writing that did not amount to a fully-functional writing system. Within this logic, the half-spotted *ahau* appears in the catalog in a series of nine main signs that resemble and are potentially cognate with the well-documented main sign for *ahau* (in the Colonial Yucatec spelling; also spelled *ahaw* and *ajaw* in the scholarly literature; Figure 8-1). The *ahau* glyph is the twentieth day sign in the tzolk’in, and is also commonly read as “lord.” Since the *ahau* glyph is marked by a longstanding history of colonial and postcolonial interpretation, it is

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<sup>3</sup> Thompson (1962) assigns nicknames to many of the catalog’s affixes and main signs. Some of these nicknames are interpretations of signs’ iconic values, while others are readings of hieroglyphs’ meanings. In the case discussed here, the nickname describes the properties of the hieroglyph relative to another listing in the catalog. Thompson thus assigns the other listing, *ahau*, relational priority, likely based on the greater frequency of *ahau* signs in the hieroglyphic record.

important to begin this section by sketching background on the *ahau* before questioning how the spots became a property that marked the half-spotted *ahau* as a distinct graphic type.

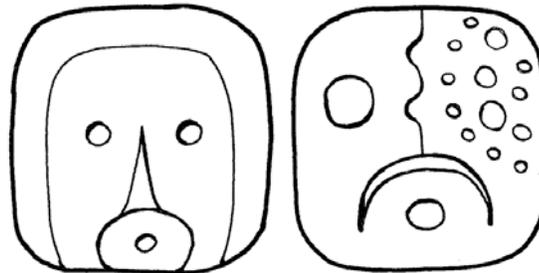


Figure 8-1. The *ahau* (T533), left, juxtaposed against the half-spotted *ahau* (T539), right. Drawings by Lucas Martindale Johnson, after Thompson (1962:145, 152), reproduced with permission.

Colonial examinations of the *ahau* sign date to Diego de Landa's sixteenth century attempts to apprehend the structure of Maya hieroglyphs and calendrics. Images of the *ahau* sign appear in two contexts in the manuscript attributed to Landa: the last of the twenty Maya day names that combine with 13 numbers to produce the 260-day tzolk'in calendar (Tozzer 1941:134), and as signs marking the 13 periods of a calendrical diagram termed the "katun wheel" (Tozzer 1941:167). The term "katun" (also spelled "k'atun") designates a period of 20 (360-day) years. The k'atun wheel was a diagram for measuring a series of 13 successive k'atuns, amounting to slightly greater than 256 solar years. Scholars have documented seven colonial Yucatec calendar wheels (Glass 1975), as well as a turtle sculpture bearing a circular inscription of 13 *ahau* signs from the Late Postclassic Yucatec site of Mayapan (Taube 1988).

More importantly, the *ahau* sign is pervasive in both Classic period monumental inscriptions and Postclassic screenfold manuscripts. Many such uses of this sign have a meaning that differs from its documented colonial sense as a term for the final unit of

the 20-day month. The sign is frequently read as “lord” or “ruler.” This reading is based on the polyvalence of the morpheme *ahau*, as the term and a number of its equivalents in distinct Mayan languages carry this meaning (Barrera Vásquez 1980:4; Gates 1931; Thompson 1971[1950]:87).

The sign’s pervasiveness has long been considered a marker of its significance. For example, in his first major work on Maya hieroglyphs and calendrics, Sylvanus Morley (1975[1915]:40-1) compares the *ahau* glyph to the 19 other day names, stating that it is “by far the most important, since it is found in both the codices and the inscriptions more frequently than all the others combined.” As a result of its prominence and documented historical meanings, the *ahau* glyph has continuously functioned as what could be called a Latourian “matter of fact” (in forms that are distinctly unmodified, as opposed to the derived or cognate forms included in Thompson [1962]), at least since its meaning was further discussed by antiquarians such as Brasseur de Bourbourg at the end of the nineteenth century (see Macri andLooper 2003:65).

Unlike what could be called the “unmarked” *ahau*, the range of signs that Thompson categorized as instantiations of the “half-spotted *ahau*” did not attract attention as a distinctive graphic form in attempts to interpret the hieroglyphs until the twentieth century (see Macri andLooper 2003:67). The first published speculation concerning the meaning of this modified *ahau* sign appears in Charles P. Bowditch’s (1906) discussion of calendrical signs on tablets in the Palenque Cross Group. Bowditch (1906:9) postulates that the division of the *ahau*’s face into two halves indicates that it is a symbol of the equinox, the biannual dates in which night and day take up (approximately) the same length of time: “It is at least possible that this glyph

may have an equinoctial meaning, for here the upper part of the Ahau face is divided by a line from top to bottom, and on the left side but one eye (the day sun or day?) is seen, while on the right a multitude of eyes (the moon and stars or night?) are shown.”

Bowditch’s proposed reading fit comfortably within the broader discursive parameters defined by the emerging Morley-Thompson consensus that the hieroglyphic system was primarily an astrological tool. In 1924 archaeologist Herbert Spinden (1924:152) reiterated and further clarified this reading, identifying this particular version of the *ahau* as one of two ways to designate the equinox. The other sign that Spinden interpreted to mean “equinox” is readily identifiable as what Thompson (1962:160) later described as a “half-cross-hatched *kin*.” *Kin* (now spelled “*k’in*”) is a word for “day” (Thompson 1971[1950]:142-3) represented frequently in the inscriptions (particularly as a component of Long Count dates). While *ahau* typically designates either the final day of the 20-day calendar or the term “lord,” it is important to note in this context that Mayanists such as Thompson (1971[1950]:87-8) have considered the root of these two meanings to reside in the term’s use for the sun god. Mayanists of this era interpreted the *ahau* and *k’in* as having associations with the sun. Therefore, the symbolic logic that animated Spinden’s argument for these two signs’ common designation of the equinox issues from their half-covered status. In the case of the *ahau* sign, Spinden resembles Bowditch in interpreting the spots as icons for stars (though he does not explicitly equate the left-hand spot, or “eye,” with the sun). Likewise he interprets the hatching on the examples of *k’in* that he cites as representing darkness.

Spinden (1924:151-2, 2001[1929]:136-7) invokes these examples of modified *ahau* and *k’in* glyphs in the context of an argument that the “Old Empire” Maya – in the

chronological terminology of the era – had a complex calendrical system enabling them to predict and record equinox dates. The specific signs that he reproduces appear in monumental texts from Palenque that bear the date of 9.12.18.5.16, 2 Cib 14 Mol. Spinden correlates this Maya date with the Gregorian equinox date of September 23, 430. However, Spinden's proposed framework for correlating the Maya and Gregorian calendars was later overturned and displaced by the model championed by Thompson (1935, 1971[1950]), which integrated the preceding arguments made by Joseph Goodman (1905) and Juan Martínez Hernández (1928). In the Goodman-Martínez-Thompson (or GMT) Correlation, the Palenque date corresponds to July 23, 690, which is not an equinox. More recent Mayanists have contended that July 23, 690 corresponds with an alignment of planets and the moon that was significant to the Palenque Maya of the time and was associated with a dedication event recorded in the inscriptions (Dütting et al. 1982; Lounsbury 1989). While it is beyond the scope of the current chapter to examine contradictions in assemblages emergent out of controversies over the proper correlation, it is apparent that the GMT Correlation effectively intercedes in the alliance between the modified *ahau* and the equinox.

The resolution of the correlation controversy resulted in a weakening of the alliance between the equinox and the modified *ahau*. In *Deciphering the Maya Script*, an important book written during the 1960s and published in 1976, David H. Kelley (1976:51) summarizes the bearing of the correlation question on the equinox interpretation of the *ahau* and *k'in* signs in the following terms:

Spinden's correlation put one of these points at the winter solstice. This was excellent supporting evidence for the correlation, and, if the correlation were accepted, the interpretation would seem plausible. However, Thompson (1937, p. 89) pointed out that there were other occurrences of

the half-darkened *kin* and that the intervals between them were such that it would be impossible for all or even most of them to be equinox intervals, and hence that the decipherment was wrong. Spinden has indirectly responded to Thompson's criticism by saying, in effect, that if a text made reference to the equinox in a glyph, it need not always make reference to it in a date. In English we might refer to one event as occurring at a certain date and to another, related event as occurring "at the last equinox." While Spinden's statements are nowhere as explicit as this, his interpretation of various uses of this glyph suggests that this is what he intended. Although this is certainly possible, it is equally clear that Thompson's criticism greatly weakens the case. It may also be pointed out that the half-darkened Ahau remarkably resembles the Hopi Kachina called *ahul*, the sun kachina, with stars painted only on half of his face.... This kachina was particularly associated not with the equinoxes, but with the winter solstice.

While Kelley accepts that the correlation problem destabilized Spinden's interpretation of the modified *k'in* and *ahau* signs, he does not fully reject the possibility that the sign might have a calendrical, astronomical meaning. He even mediates the alliance of the "half-darkened Ahau" with an example of an aesthetically similar figure with a different astronomical meaning in Hopi cultural contexts.

While Kelley was a strong advocate for phoneticism, as opposed to the Morley-Thompson consensus, he does not directly challenge this particular calendrical interpretation and affirms that the spots on the modified *ahau* could be reasonably interpreted as stars. In a subsequent discussion in this same book, Kelley (1976:278) even returns to affirm Spinden's alliance between the modified *k'in* and *ahau* signs:

In the TS [Palenque Temple of the Sun] and TFC [Palenque Temple of the Foliated Cross], rather lengthy parallel passages deal with a date 2 Cib 14 Mol, and in the TC [Palenque Temple of the Cross] a very similar passage is associated with the same implicit date, which, however, is not actually recorded there.... These parallels strongly suggest that Spinden is correct in thinking that the half-darkened *kin* and the half-darkened Ahau convey similar information. The fact that the latter has the prefix *chac* ('great, red') in the TC suggests that it is some unusual phenomenon.

So, in this view, the modified (or "half-darkened") *ahau* emerges as an "unusual phenomenon" that may have calendrical significance and some kind of semantic

overlap with a modified version of the sign for “sun” or “day.” But its specific association, or alliance, with the equinox has been effectively cut, as the calendrical correlation that functioned as a premise of this association has dissolved.

Based on this evidence, it is not surprising that Thompson (1962), presuming the accuracy of his correlation, made no reference in his catalog to Bowditch’s and Spinden’s interpretations of this modified *ahau* as an equinoctial sign. It is plausible, though, that this interpretation weighed on his consideration of the sign as a distinctive form. Neither of the dictionaries that Thompson utilized in the compilation of the catalog (Gates 1931; Zimmermann 1956) lists this particular modified *ahau* as a distinct sign.<sup>4</sup> Thus, the sense that the spots comprised a property that might confer a distinctive value on this *ahau* plausibly issued from its faltering reading as a symbol of the equinox marked by iconic depictions of stars.

If he considered Bowditch’s and Spinden’s interpretations at all, they did not weigh on Thompson’s (1962:152) nickname for this particular sign, the “half-spotted Ahau.” Here, the sign for the equinox or for some “unusual phenomenon,” in Kelley’s (1976:278) words, emerges as a rather generic sign, with very little distinctive information presented in the listing and no attempt to clarify its meaning. In contrast, Thompson (1962:160) rather definitively equates the modified *k’in* sign, which he terms the “half-cross-hatched Kin,” with “the sun or sun god in his underworld aspect.”

While Thompson’s catalog provided little information on the sign’s meaning, it did materially channel how the modified *ahau* could be known. Particularly, both the nickname “half-spotted *ahau*” and the term “T539” permit the sign to be referenced

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<sup>4</sup> It is possible that this modified *ahau* does not appear in the codices that comprised Gates’ and Zimmermann’s empirical materials.

without necessary recourse to simulating the actual glyph. “T539” is necessarily abstracted from its referents. Both the name “half-spotted *ahau*” and the term “T539” can take the form of text, terms in standard typesetting that are legible as writing. They are signs detached from their referents. This detachment elides empirical specificity in order to create a logically ordered system, a conceptual tool for the deductive, interpretive machinations of the epigraphic community. In other words, when discussing specific hieroglyphic signs through use of T-numbers, epigraphers are not making reference directly to empirical examples in context, but to a system that already imposes a logic of how the signs operate and should be experienced. Thompson’s catalog emerges as a master list of “immutable mobiles” (Latour 1990) circulating and constituting the community of epigraphers, mediating their interpretations of and therefore associations with ancient Maya lords and scribes.

The entry informs users that Thompson located 25 cases of T539 in archaeological materials from the Classic Period sites of Copan, Yaxchilan, Palenque, Piedras Negras, and El Cayo, and is holding two additional examples from a book by Miguel Covarrubias (1957:214, fig. 94c, A1 and A4) “*sub judice*,” Latin legal jargon for “under judgment.” He performed novel epistemic labor in drawing together 27 references to hieroglyphs found at distinct sites into one listing, thus mediating their singularization under the equivalent signs “T539” and “half-spotted *ahau*.” Through Thompson’s mediation, a series of distinct material forms with disparate material contexts became identifiable as instantiations of the same. The only other piece of information offered in Thompson’s listing is a cross-reference to another glyph in the catalog: T751.

Thompson provided relatively little information about T539 because it was not a centrally important sign in discussions of the ancient Maya world. In the years before and after the publication of Thompson's catalog, T539 did not play a critical role in conversations surrounding Classic Maya history, cosmology, and religion. It appeared in scattered commentaries as a matter of secondary importance to other considerations.

In a 1989 catalog of Maya hieroglyphs, Kornelia Kurbjuhn (1989) notes that Thomas S. Barthel and Dieter Dütting read the sign as "*balam-ahau*," meaning jaguar-lord (or hidden-lord). As the spots on some examples of T539 resemble the spots on jaguar pelts depicted on Maya vases, it is highly plausible that these authors ascribed a new value to the features that had been previously interpreted as icons of stars. T539 was undergoing a transformation, but Barthel and Dütting were not paying primary attention to this sign, constituting it as a central analytical object. This shift did not occur until 1980, when Schele allied with T539 and positioned it as a central matter of concern in decipherment's actor-network.

### **Linda Schele's *Balan-Ahau***

In 1978, during the third Palenque *Mesa Redonda*, Linda Schele identified T539 in passing as the main sign in a glyph collocation from Palenque's Dumbarton Oaks Tablet. In this paper, and in a letter from the same period,<sup>5</sup> Schele terms T539 the "jaguar-spotted *ahau*" rather than using Thompson's term, the "half-spotted *ahau*." There is no published discussion of this terminological substitution and specification. It comes across in both the *Mesa Redonda* paper and the letter as an incidental, and possibly accidental, shift unworthy of note, an indication that hieroglyph experts shared Barthel's and Dütting's considerations of T539's spots as iconic depictions of jaguar

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<sup>55</sup> Schele to Lounsbury, Mathews and unspecified others, January 10, 1978.

markings. The shift may be explained by a reference that Schele made in a subsequent paper (Schele 1985), discussed below. There, Schele attributes a new reading of T539 to David H. Kelley, submitting that in a 1976 personal communication he noted the near-homophony between terms for “jaguar” (*balam*) and “hidden behind something” (*balan*), thus suggesting that there might be a linguistic argument that iconic jaguar signs referenced “hidden lords,” or actors in the underworld.

By 1980, T539 had clearly begun to claim a greater share of the Maya hieroglyph experts’ attention. The *Mesa Redonda* group had progressed from identifying specific events in Palenque’s dynastic genealogy to completing structural analyses of the hieroglyphic syntax. Linda Schele completed her dissertation, *Maya Glyphs: The Verbs*, in 1980 and published it as a book in 1982, marking a watershed moment in this progression. For epigraphers, at least, the book represented a final, definitive statement resolving the controversy over hieroglyphs’ status as a completely syntactic writing system. Schele mapped out the hieroglyphs’ syntactic structure and catalogued the known examples of verbs in the inscriptions. Among these examples, Schele lists seven examples of T539 in verb phrases (six definite occurrences and one possible occurrence).

Additionally, in 1980, the same year that she completed her dissertation, Schele presented a paper at the fourth Palenque *Mesa Redonda* that further mediated the transformation of T539, and provides a clearer indication of how she thought about the hieroglyph than did its references in *Maya Glyphs: The Verbs* (Schele 1982a). Significantly, her paper was titled “Balan-Ahau: A Possible Reading of the Tikal Emblem Glyph and a Title at Palenque” (Schele 1985). Here Schele presents the most

extensively researched reading of the T539 glyph to date. The paper marshals a wide range of actors, mediating their coordinated effort to establish and stabilize a new – or newly publicized – articulation of the T539 sign and a “meaning,” or a set of morphemes that remain attached to an object or series of objects as they circulate. The particular articulation that her argument attempts to establish and stabilize is a link between the sign designated by Thompson’s T539 and the term *balan-ahau* or “hidden-lord” (or, alternatively, *balam-ahau*, or “jaguar-lord”).

In this case, *balan-ahau* is not simply a name. Rather, by documenting the repeated appearance of this glyph in association with arguably cosmological imagery, Schele makes the case that *balan-ahau* designates a mythological or supernatural figure, including historical lords who had died and transformed into “underworldly” (Schele 1985:60) figures. This interpretation is clearly shaped by Michael Coe’s (1978) claims that scenes on ceramic vessels primarily represented events of the underworld. Working within this interpretive framework, Schele identified T539 as a sign indicating that associated figures were “hidden” by death. To achieve this reading, Schele must line up a series of actors well beyond Kelley’s 1976 assertion of the linguistic basis for a hieroglyphic *balam-balan* substitution.

In light of my Chapter 7 argument concerning text-image relations, a particularly significant set of associations that Schele attempts to establish turns on the spatial proximity of examples of T539 to anthropomorphic and zoomorphic figures depicted in scenes on pottery. Schele opens her paper with a discussion of the types of secondary texts that appear on ceramic vessels. Here, Schele invokes three cylindrical pottery vessels (the Altar de Sacrificios vase, the “Fire-Jaguar” vase, and the “deer-dragon”

vase [sic]), which entail painted scenes of anthropomorphic and zoomorphic beings participating in processions. Following Coe (1978), Schele informs readers that these scenes depict nonhistorical supernatural creatures and deities, some of whom are involved in clearly non-naturalistic acts.

To the outsider, Schele's interpretation of the vase scenes as cosmological might seem benign and apolitical. But within the Mayanist world Schele's claim weighed in on a charged controversy over whether scenes on such vases represented cosmological or historical content. The vessel from the archaeological site at Altar de Sacrificios, or the "Altar Vase" (Figure 8-2), was a key site in this disciplinary dispute, because it had secondary texts associated with the figures represented. Unlike many Maya vessels, this artifact has an archaeological provenience; it was found in a burial associated with a significant tomb at Altar de Sacrificios. Archaeologist Richard E. W. Adams (1971, 1977) interpreted the imagery as an historical scene depicting the funerary rites surrounding the interred individual, rather than as a cosmological vision of the underworld. T539 became enrolled in this controversy because it appeared in the secondary texts, and could thus be seen as a sign that might contribute to a resolution of the controversy over whether the scene was historical or cosmological.

In effect, Schele's attribution of cosmological status to the figures depicted in this and similar vases marked a rupture between such representative archaeological claims and the epigraphers' positions, which heralded the interpretive value of hieroglyphs. The collective of epigraphers was establishing the primacy of the hieroglyphs to historical interpretation, and presuming that the signs provided a direct window into ancient Maya minds. At approximately this same time, Schele and her allies were

involved in a parallel controversy with archaeologist Alberto Ruz Lhuillier over discrepancies between ages of rulers that they deciphered in the Palenque hieroglyphs and physical anthropologists' determination of the age of the remains attributed to the Palenque ruler Pakal (Ruz Lhuillier 1977). Taken together, such controversies challenged the power of hieroglyphic readings to provide direct, literal information about the ancient Maya. The patterned appearance of T539 in name clauses inscribed as "secondary texts" on ceramic vessels became a crucial point of evidence helping to demonstrate that these scenes depicted cosmological rather than historical beings and, thus, legitimize epigraphic interpretive advances achieved in the 1970s and 1980s.

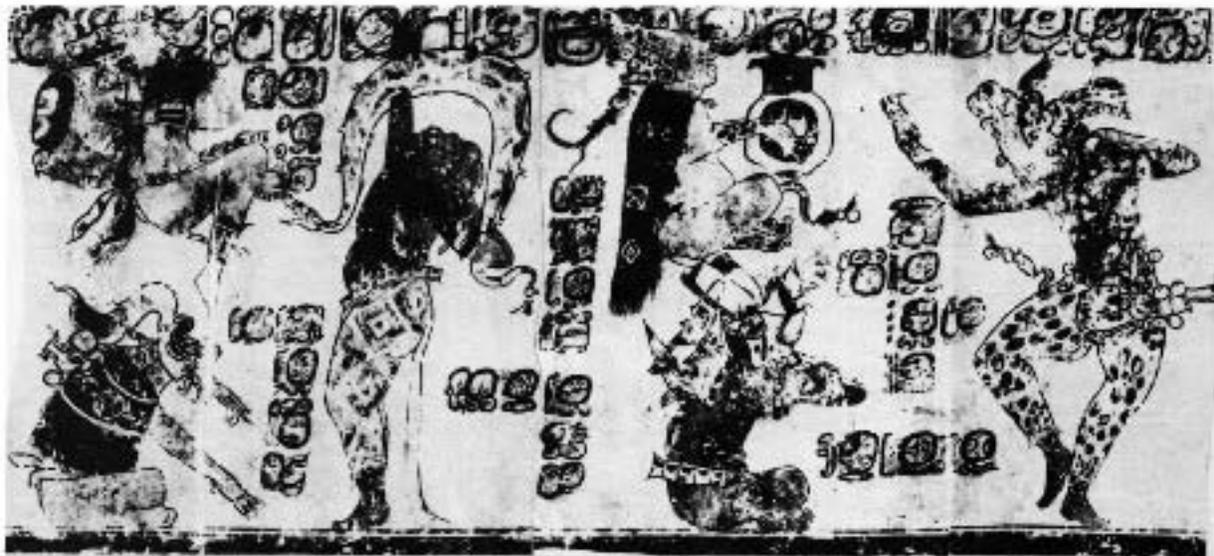


Figure 8-2. The black-and-white composite photograph of the Altar Vase reproduced (without attribution) and analyzed in Schele's (1985) proposal of the *balan-ahau* reading.

In Schele's paper T539 becomes an enrolled ally of epigraphers who had begun to position the hieroglyphic record as textual evidence of ancient Maya cosmology, and thus evidence for the critical importance of hieroglyphic interpretation as a means to access ancient Maya thoughts. Schele asserts that the pattern of association between

nonhuman figures and T539 “can be found in an entire category of pottery scenes” (Schele 1985:60). The power of epigraphy was to determine what these intricate ancient Maya scenes meant with the certainty of literalist conviction. Schele’s paper helped to position competence in hieroglyphic interpretation (a skill that not many archaeologists possessed) as a prerequisite to the effective interpretation of scenes on excavated pottery vessels. These vessels’ material contexts, to which the archaeologists could claim privileged access, came to matter much less in the determination of their meanings. They became otherworldly artifacts, and their meanings now had to be accessed through hieroglyphs such as T539. These hieroglyphs thus became “obligatory passage points” (Callon 1999[1986]) stabilizing an asymmetric distribution of epistemic competencies in ancient Maya studies.

This shift more generally entailed de-emphasizing the importance of documented provenience in interpreting ceramic vessels. Such a turn likely alarmed archaeologists, particularly those who had established professional stakes in an ethics of opposition to the use of unprovenienced vessels (e.g. Coggins 1969). Many such vessels were looted from archaeological sites and sold in the illicit antiquities market. The articulation of T539 and the Altar Vase thus became one site in a controversy that drew together ethical claims, economic stakes, interpretive propositions, and disciplinary authority. Schele’s argument cut many archaeologists and archaeological sites from the network that relationally constituted iconographic vessels, contributing to a controversy that resulted in the entrenchment of competing disciplinary loyalties. Particularly, Schele positioned her claim as a privileged epigraphic reading, one that would invalidate Adams’ (1977:415) suggestion that the glyph collocation containing T539 on the Altar

Vase might be read “Lord-Jaguar Lineage,” a term with a clearly historical rather than cosmological referent.

Mary Ellen Miller (1989:139-40), an art historian closely allied with hieroglyph experts such as Schele, described the controversy in these terms:

As art historians and some anthropologists began to rethink the implications of the scenes and texts on painted pots in general, including the unprovenanced corpus, many archaeologists dogmatically supported the Adams interpretation of the Altar Vase (Hammond 1982; Morley and Sharer 1983) [sic],<sup>6</sup> even going so far as to redraw and reposition the glyphs to make the argument more believable (cover of Henderson 1981). For many archaeologists, a line against looted pots had to be drawn, and by following the Adams interpretation, they positioned themselves on the side of the line with those who did not use such pots, regardless of the correctness or incorrectness of the reading of the glyphs.

My point is that Miller seems to mix together two issues that have no obvious bearing on each other. In Miller’s view, archaeologists’ ethical commitments to avoid complicity with the antiquities market overrode their epistemological commitment to telling the truth about the past. They were willing to disavow an obligation to support accurate readings in order to take an ethical stand.<sup>7</sup>

Perhaps the clearest assertion in Miller’s passage is a challenge to the integrity of archaeologists. This challenge takes their imaging practices as a key point of evidence. Miller does not explain precisely how archaeologists such as John S. Henderson (1981) manipulated the image of the Altar Vase to support their argument. The cover image on Henderson’s textbook reproduces one of the figures from the Altar Vase next to a glyph collocation with iconic signs for a bird and jaguar. Adams had interpreted this figure as

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<sup>6</sup> The second citation here refers to the fourth edition of *The Ancient Maya* (1983[1946]), by Sylvanus Morley, George Brainerd, and Robert Sharer.

<sup>7</sup> As a side-note, this is precisely why we need ANT-style analyses that seriously consider how actors such as Miller enact distinctive ontologies that do not have recourse to standardized academic distinctions (here, the distinction between ethics and epistemology).

a historical individual named Bird-Jaguar, reading the first glyph in the secondary clause associated with the figure as this name (Adams 1977:415). This glyph collocation does not appear on the Altar Vase and Miller, here, appears to be objecting to the choice to depict the Altar Vase figure with a glyph collocation extrinsic to the vessel's scene. I would not contest the point that cover art could amount to an actor bearing on the epistemic terrain of ancient Maya studies, but this assertion seems to hinge on an opposition to an aesthetic decision that does not amount to an explicit interpretive claim. Moreover, such assertions indicate a lack of awareness that all rollout photography and images of cylindrical ceramic vessels rely on betraying the original form of the image in order to make them commensurable with the two-dimensional printed page.

By tracing out the matrix of hidden actors implicated in Schele's mediation of T539 and the Altar Vase it becomes apparent that the significance of her argument did not depend on any transparent empirical or aesthetic criteria. Rather, the strength of the transformation of T539 into the "jaguar-spotted *ahau*" issued from how this reading helped to performatively solidify the integral roles of hieroglyphs and their intra-active producers in the interpretation of iconographic scenes. These intra-active producers are primarily epigraphers and art historians such as Schele. Thus, while I may seem to have detoured here from T539 into a controversy over provenience and interpretive authority, the actors enrolled in the controversy partially constituted and were partially constituted by T539. Following the transformations of T539 indicates how such hieroglyphs take on their own sets of relational competencies in the production of arguments about the ancient Maya. They are vital players in this sociomaterial world. And, through the course of Schele's (1985) *Mesa Redonda* paper, T539 became one

important hieroglyph (among many playing active roles in this epistemic terrain), positioned vitally between the contemporary world of Mayanist scholars and the ancient Maya themselves, and between competing factions of archaeologists and hieroglyph experts.

While I have now begun to indicate the importance of this particular sign's simulation, it remains critical to trace its movements more completely, as T539 took on new associations even within Schele's (1985) paper. The re-interpretation of figures depicted in vase scenes as cosmological actors, and the associated interpretation of jaguar-spotted *ahau* as a sign of this supernatural status was not the extent of the glyph's transformation. Rather, possibly implicitly building on the work of Barthel and Dütting, she proceeds to offer a logographic reading of T539, a designation that could displace and defer the nicknames "half-spotted *ahau*" and "jaguar-spotted *ahau*." She proposes that the sign was read as "*balan-ahau*" or "hidden lord."

In justifying this new reading Schele (1985:61-2) invokes four lines of evidence, based on the glyph's contexts of association and substitution on the three analyzed pottery vessels. First, a third-person-singular possessive pronoun (T1, *u*) is often affixed to the sign in its appearance in the name phrases of figures that she generally interprets to be supernatural. In other words, the sign's usage in name phrases frequently appears to denote the status of a particular lord, statements that epigraphers transliterate into "his hidden lord, [the proper name of the lord]" and translate into "the hidden lord of [the proper name of the lord]." This piece of evidence is autonomous and distinct from the following three, which meld together glyphic and linguistic evidence to make the case that T539 is a modification of the *ahau* sign with a logogram for the term

*balan*, “hidden.” Thus, Schele’s second point of evidence is that an anthropomorphic head sign accompanied by a jaguar tail or rope ostensibly functions as a synonymous substitute for T539. Third, the anthropomorphic “head variant” sometimes appears in (what was then interpreted as) Tikal’s emblem glyph, substituting for the conventional main sign, an icon of bundled strands. Finally, icons of twisted rope and jaguar tails often accompany head variants of jaguar-spotted *ahau*.

Schele’s argument then turns to the near-homophonous relation between the terms *balam*, “jaguar,” and *balan*, “hidden,” as she proceeds to compare two different sets of meanings of the root word *bal*. The comparison of these two roots establishes that *bal* alone is a linkage between Mayan-language terms for “hidden” and for “rolled up, coiled, wrapped up, and bundled” (Schele 1985:63). Both of these concepts now acquire a close association with the jaguar, whose pelt partially hides the *ahau*’s face. Through this linguistic claim, Schele is able to extend the effects of her argument into a much more pervasive relational network of actors. She maps how this new reading of T539 improves interpretations of objects beyond the hieroglyph and the series of ceramic vessels of which she has already analyzed a small sample. In effect, she has allied the unknown, T539, with a series of glyphs that have known semantic values related to jaguars.

Schele’s interpretation mediated the clear expansion of a network of previously unconnected actors, positioning T539 at the center of a system that acknowledged the interconnection of ceramic vessels with and without provenience, deity figures, jaguars, and monumental inscriptions at Tikal, Palenque, and Yaxchilan. These were the active mediators in Schele’s translation. But this constitution of the past deferred, cut, or failed

to acknowledge the contemporary actors and processes shaped by this translation. These included imaging practices, archaeological contexts, ethical propositions concerning the effects of interpreting unprovenienced vessels, and parallel controversies over contradictions between epigraphic and archaeological evidence.

Despite these deferred and unacknowledged actors, 1970s and 1980s epigraphers' claims to direct insight into ancient Maya belief gave rise to a new set of representations (with significant ethical consequences which I discuss in the study's concluding chapter). The consolidated access to past beliefs came to pervade historical arguments concerning ancient Maya political organization. As represented in the epigraphic and art historical literature, the ancient Maya political elite who produced cosmology-themed inscriptions had profound connections to other dimensions of reality. This is nowhere more apparent than on the pages of Schele and Miller's (1986) impressive exhibition catalog and book, *The Blood of Kings: Dynasty and Ritual in Maya Art*. Here, the authors draw together diverse expressions of ancient Maya art, rendering these heterogeneous objects intelligible within a haunting narrative of bloody regal rituals. Schele and Miller offer consumers of their exhibition and catalog a figure of the ancient Maya king as a sacred actor:

The king acted as a transformer through whom, in ritual acts, the unspeakable power of the supernatural passed into the lives of mortal men and their works. The person of the king was also sacred (Schele and Miller 1986:301).

The institution of Maya kingship was expressed in the metaphor of the cosmic vision. In its earliest manifestations, the king stood at the apex of pyramids with symbols that defined the movement of the sun and Venus across the backdrop of the sky. The king ensured that the heavens would rotate in perpetuity through the rituals of sacrifice and bloodletting. The gifts of blood served both to nourish and sustain the gods and to communicate with them. In this role, the king was a nourisher of the gods, of maize and of his people (Schele and Miller 1986:301).

The strength of the network partially performing and partially performed by the hieroglyph experts turned on the capacity of linguistic interpretations to make claims to accessing ancient Maya beliefs, and thus a multiplicity of (historicized) cosmological actors that were presumed inaccessible without texts comprehensible as so-called “emic” accounts. With *The Blood of Kings*, it became difficult to apprehend the ancient Maya world without first considering the gory rituals of bloodletting and sacrifice. Ancient Maya life became a matter of ritual obsession, and the court of deities “invented” by “Anglo anthropologists” (Montejo 1993) grew ever more populous.

T539 offers a powerful point of entry into understanding the emergence of this more elaborate assemblage. Though it is not a matter of direct importance to *The Blood of Kings*, the T539 reading is suggestive of how the decipherment actor-network oriented scholars to mediate relations between hieroglyphic signs and cosmological phenomena. The sign’s reading was one of many important actors within a strong network of associations contributing to the image of the ritual- and cosmology-obsessed, bloody ancient Maya lords. Within this broader set of interpretations, the *balan-ahau* reading became one among many relevant matters of fact for Mayanists. For example, in formulating a new reading of the *ahau* glyph (T533), epigrapher Barbara MacLeod (1987:454) exhibits certainty in the interpretations that inspired the *balan-ahau* reading:

Schele (1985) has firmly established [the *balan-ahau* glyph’s] frequent presence in name phrases of underworld supernaturals.

Schele (1985) has demonstrated with abundant evidence that the Balam Ahau complex involves the jaguar, and that it replaces a sign reasonably read bal-(VC?) which is the canonical form of the Tikal Emblem Glyph.<sup>8</sup>

Likewise, in a later chapter that returned to the question of the Altar Vase, Schele (1988) shows no signs of questioning the *balan-ahau* reading, though she does allow for the possibility that the figures represented might have historical aspects. Likewise, in a different late-1980s publication Schele (1989:146) describes the interpretation of T539 as a designator for the associated iconographic figure as an “Underworld denizen” as her “primary contribution” to the interpretation of pottery rooted in Coe’s (1978) argument that their images comprised cosmological scenes.

As a side note, MacLeod’s acceptance of Schele’s reading also provides important evidence of the unconventional structure of epistemic authorization in the field of Maya hieroglyphic studies. As the presenters at the *Mesa Redonda* were invited to participate by Merle Greene Robertson and her collaborators, and their publication was printed by Robertson’s privately funded research organization, the claims of participants were not subjected to a process of peer review. Yet, many of the papers printed in *Mesa Redonda* volumes have been treated as significant, original, authoritative contributions to the field. Thus, witness how MacLeod mobilizes claims made outside of the conventional structures of peer-reviewed scholarly production into a peer-reviewed publication, *Anthropological Linguistics*, treating these claims as practically unassailable. An even more remarkable Mayanist knowledge practice, which I will discuss in a different context below, involves the use of unpublished letters as definitive sources. Indeed, MacLeod proceeds to cite an unpublished argument made by David

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<sup>8</sup> The term “bal-(VC?)” designates the root *bal* suffixed with a vowel-consonant combination, as in *balam* and *balan*.

Stuart in a private letter as the cause of one reading of a different hieroglyph's "widely accepted" status (MacLeod 1987:455). That an argument in a letter no doubt circulated among a small group of epigraphers can be taken as "widely accepted" indicates how the security of historical propositions in Mayanist epigraphy weighed heavily on the opinions of a very small group of people, increasingly comprised of epigraphers trained in linguistics.

It is my contention that Schele's argument was not "firmly established" on the basis of empirical evidence or rhetorical persuasiveness. It was neither an empirical fact nor a social construction. It was not the outcome of a process that can be accurately reduced to the positions of either realist or antirealist epistemologies. Rather, T539 became *balan-ahau* through the high number of linkages, articulations, or alliances that this new reading conditioned, combined with the reading's capacity to cut away a contradictory network comprised of archaeologists and the sites for which they served as spokespersons. Its truth-value was not based on any correspondence to worlds past or present, or, for that matter, on a community of assenting scholars without access to worlds beyond their collective mentalities and social practices. In her constant material engagement with the world of Maya hieroglyphic inscriptions and their simulations, Schele had become a mediator and a hieroglyphic choreographer acting in the interests of T539 as it strengthened its coordinated connectivity, positioning itself as a thing, an assembly, or a Latourian *ding*, around which a multiplicity of actors circulated. She had become T539's spokesperson.

**Barbara MacLeod, Nikolai Grube, Stephen D. Houston, and David Stuart's Way**

Thus far, I have followed as a run-of-the-mill hieroglyph that Thompson (1962) nicknamed "half-spotted *ahau*" transformed into "jaguar-spotted *ahau*" and "*balan-*

*ahau*,” a designation for ancient Maya supernaturals and dead Maya rulers to which Mayanist epigraphers could claim privileged access. Now I proceed to trace how this matter of fact dissolved in the face of a new reading. In Latour’s (1987) terms, a new reading subjected the T539-*balan-ahau* alliance to a “trial of strength.” The new reading cut the alliance between these two actors, drawing T539 into a new material system. The remarkable feature of this particular trial of strength and systemic shift is that it undermined Schele’s seemingly incontestable, matter-of-fact reading without conceding Maya hieroglyphic studies’ achieved centrality to the field of ancient Maya studies. Indeed, as I show how the definition that appears in Macri and Looper’s (2003) catalogue, Nikolai Grube, Stephen D. Houston, and David Stuart’s new interpretation of T539 as *way* managed not simply to retain the expanded power that Schele’s earlier reading helped to consolidate, but it further strengthened the disciplinary influence of Maya epigraphy as the reading mediated a new set of linkages between ancient and contemporary Maya cultural practices.

The 1980s was an integral decade for the consolidation of epigraphers’ influence within the field of Mayanist historical knowledge production. As I have already outlined, during this decade the Maya Meetings emerged as a significant force enrolling an ever-growing public into the field’s institutional structures of knowledge production (Chapter 5). By the end of the decade, epigraphers had begun to ally with Mayan-language speaking activists, who further mediated the production of a public vested in Maya culture as a trans-historical singularity (Chapter 6). The imaging practices of Mayanists provided an aesthetic basis for this cultural singularization (Chapter 7). Interpretations of Maya hieroglyphs in the 1980s were conditioned by and bound into this singularizing

figure of the Maya, shaped heavily by the existing technologies and media of historical knowledge production.

Schele's reading of the T539 glyph as *balan-ahau* aided in performing an assemblage that made the Maya a knowable singularity and positioned epigraphy as a requisite mediator for understanding the Maya past. But "*balan-ahau*" was also an antiquarian triviality, a reading divorced from anthropological interpretations of twentieth-century Maya practices. It helped to open a presumably direct, unfiltered visage into ancient Maya beliefs. But it left the ancient Maya in a past unconnected to contemporary culture. The reading to which I attend in this section helped to overcome this discontinuity, establishing a clear alliance between ancient and modern Maya. In so doing it further expanded the epigraphers' power to shape how "the Maya," Mayan-language speakers in all times and places, could be known.

As epigraphers came to decipher a high number of glyphs during the 1980s, they expanded means of circulating their readings outside conventional academic media. This development of a significant "gray literature" had multiple causes, including the problem of publication delays, the structure of progress in decipherment – a process of incrementally deciphering individual sign elements – and the high proportion of non-academic participants in the field.<sup>9</sup> Indeed, Barbara MacLeod, an important linguist and epigrapher situated outside the institutions of academia, emphasized how there exist significant disincentives to publication among non-academics. Publishing readings effectively invites those with professional academic stakes to critique and overturn the proposed readings. This problem is exacerbated by the finite quantity of hieroglyphs.

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<sup>9</sup> MacLeod, B. Personal Interview. 23 July 2007.

Epigraphers compete to read individual signs and thus acquire a kind of intellectual proprietorship to the signs that they decipher.

A high degree of collaboration among Maya hieroglyph experts clearly dates to the early Palenque *Mesa Redonda* workshops. But in the 1980s these collaborations came to achieve new forms. In addition to communicating through letters, some of the newer hieroglyph experts distributed unpublished research reports by mail. The informality of this medium encouraged some authors' playfulness and experimentation. MacLeod titled the standard issues of her newsletter "North Austin Hieroglyphic Hunches," and the more ambitious issues, "North Austin Half-Baked Glyphic Possibilities." In 1985, George Stuart's Center for Maya Research began to publish an internally reviewed series called "Research Reports on Ancient Maya Writing." Soon thereafter, Linda Schele and colleagues initiated similar desktop published series called "The Copan Notes" and "The Texas Notes on Precolumbian Art, Writing, and Culture."

The editors positioned these series as the cutting-edge spaces for epigraphic interpretation. The "Note" became the paradigm of documentation in Maya hieroglyphic studies. Notes tended to consist of an individual article with three to ten pages of writing accompanied by hieroglyphic illustrations. While The Mesoamerica Center at the University of Texas has recently digitized many of these notes, they previously had very limited distributions. As the lone series of this era still in publication, the "Research Reports" are currently transitioning from print to digital distribution through the Mesoamerica Center webpage. On this page, the "Research Reports" editor and former National Geographic archaeologist George Stuart comments wistfully on the transition:

These [digitized reports] will, in effect, end an era of publication that began in the summer of 1985 with the publication of two papers by then 20-year-

old David Stuart. The design, format, and typography for these numbers and their successors, worked out by David and myself in our home library, has served well, as has the quality of their content. The number of subscribers gradually increased to a peak of several hundred individuals and institutions in nearly two dozen countries around the world.<sup>10</sup>

Thus, as a print publication, the “Research Reports” differed from letters and newsletters in terms of their wider distribution. They became an archive of significant decipherments. The reports also signaled the increasing formalization of the discipline as younger scholars moved from art historical approaches towards ostensibly scientific methods grounded in linguistics.

This formalization is reflected in a documental supplement on style and content distributed by the “Research Reports” editor in May 1988. The supplement essentially institutes the conventions for acceptable hieroglyphic interpretation. George Stuart frames the document as an aid to both potential contributors and readers attempting to understand the increasingly technical interpretive style. He lays out the proper jargon, the standards for designating hieroglyphs, the acceptable use of nicknames for glyphs, the standard practices for transliteration, etc. This supplement creates a distinction between the consumers and producers of Maya epigraphic knowledge. Many of the subscribers were likely enthusiasts who attended workshops such as the Maya Meetings and became attached to the ancient Maya charismatically brought to life by Linda Schele. Here, such subscribers are rendered passive witnesses. At the same time, the document constrains potential contributions of knowledge-producers, defining certain literary conventions as “obligatory passage points” (Callon 1999[1986]). For example, the section on definitions constructs the semiotic and referential structure of hieroglyphs as beyond question, justified through the authority of named scholars.

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<sup>10</sup> See <http://www.utmesoamerica.org/research.php>, accessed on November 16, 2008.

Thus, while the “Research Reports” achieved a relatively high level of circulation, they simultaneously depended on a somewhat authoritarian imagining of the public as unnecessary and external to the true labor of decipherment.

After a period of relatively blackboxed, stable status, T539 once again became a matter of concern within this proliferating body of media in late 1989. While T539 and the morpheme “way” remained autonomous objects until Grube and Houston came to mediate this relation “independently” in September 1989, the attempt to locate a sign for spirit companionship seemed to already inhabit a space within the collective disciplinary imaginary by March of that year. Thus, to trace the convergence of T539 and way it is necessary to detour momentarily into the initial constitution of way as a matter of concern for those who took the ancient Maya and their hieroglyphs as central analytical objects. While I maintain that the cosmological concerns indexed by the *balan-ahau* reading and manifested in popular publications such as *The Blood of Kings* (Schele and Miller 1986) served as an assemblage within which the status of way as a matter of concern makes sense, the two (sets of) actors initially inhabited and performed different epistemic sites.

Evidence for hieroglyph experts’ emergent interest in the way phenomenon comes in the form of circulated documents penned by MacLeod and dated March 23, 1989 and April 1989 (archived in Schele’s correspondence). These documents lack the letterheads that defined MacLeod’s more “official” series of circulated decipherments. Unaccompanied by prefatory letters, they resemble pages from a personal research notebook rather than interpretations initially and intentionally penned for circulation.

Thus, MacLeod may not have distributed these documents to a substantial audience beyond Schele.

In any case, the documents are remarkable artifacts in that they outline an association between the semantic complex associated with the morpheme “*way*” and a set of related glyphic elements: T130, T157, and T769 (Figure 8-3). Schele (1982a:287) had earlier identified T130 as a sign for the phonetic value *wa*, and the documents by MacLeod indicate that Schele had interpreted this sign (and probably the closely associated T769) as an iconic “jaw of the underworld dragon.” Integrating the iconic basis of the hieroglyph with the complex of the root *way*, MacLeod made a case that the T157 glyph holds the logographic value of *way*. Her understanding of *way* differed somewhat from that adopted later in the year by Grube and by Houston. Specifically, MacLeod displayed concern with the iconically geographical qualities of these signs. She terms T157/T769 the “cenote glyph,” and equated the opening into a cenote, or watery sinkhole common in Yucatan, with the jaws of a dragon opening into the underworld.

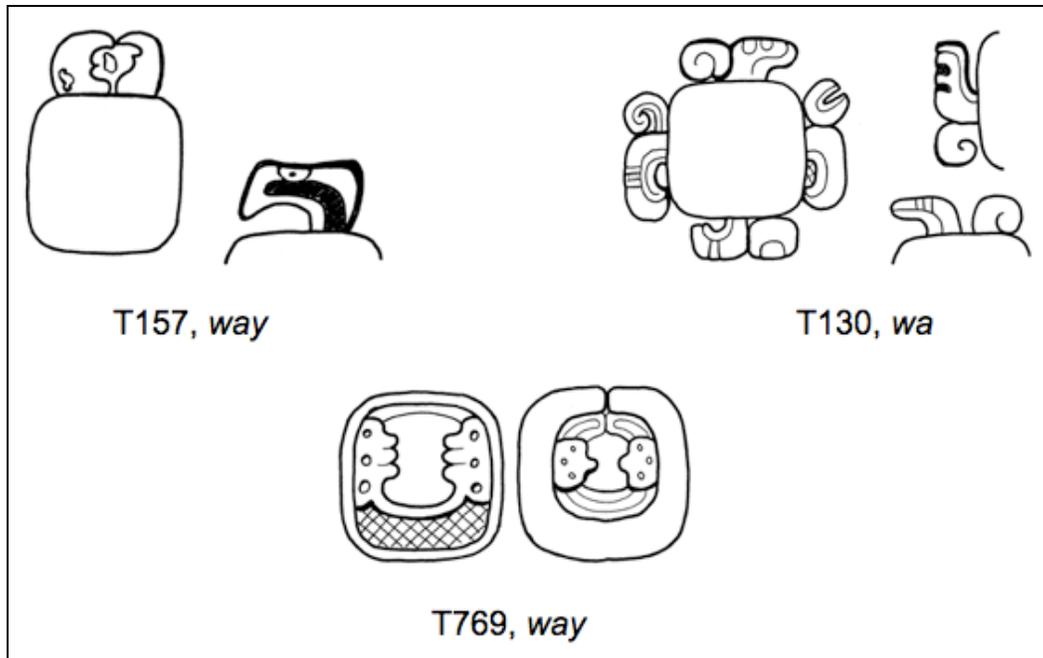


Figure 8-3. The catalogued forms of T157, T130, and T769. Note that T157 and T130 are imaged as affixes of a main sign, conventionally represented as the rounded square. Drawings by Lucas Martindale Johnson, after Thompson (1962:369, 447, 448), reproduced with permission.

MacLeod's evidence for this reading included the use of *wa* and *ya* as phonetic complements regularly affixed to T157/T769 to clarify its logographic value, as well as the sign's morphological resemblance to representations that depict the descent or opening into the underworld. Such examples include the Palenque Sarcophagus Lid, interpreted as depicting the king Pakal descending down the tree of life into the jaws of the underworld, shown as a skeletal serpent being (Figure 8-4).<sup>11</sup> That is, MacLeod interprets the skeletal jaws of the underworld to be an iconic equivalent to, or basis for, the T157/T769 signs. In closing the April 1989 document, MacLeod summarized the theme of her new interpretation in these terms:

<sup>11</sup> Citing Schele and Miller's interpretation of Pakal's sarcophagus lid as just one example, historian of religion David Carrasco (1995:439) considers the cosmic jaw that consumes human subjects in their passage to the underworld "a widespread motif in Mesoamerican religion."

The root way in Chilan and Yucatecan may be understood as referring to (1) the place of dreams (dreamtime, dreamspace) (2) a person (sorcerer, shaman) who frequents this place (3) a nagual of a sorcerer or shaman who dwells in that place. The cenote/dragon maw refers to the Dreamtime (in the Aussie sense but with Mayan content)

**Way:**

= nagual  
cave mouth  
temple doorway  
axis mundi  
underworld  
dreams, visions  
place/process of transformation  
“altered states”

Formulated within a series of rather Orientalist interpretations of the ancient Maya rulers as quasi-supernaturals obsessed with ceremonies such as bloodletting and “vision quests” (following Schele and Miller 1986), this seems to be a profoundly consequential interpretation. The *way*, or spirit companion (also known as a co-essence, spirit double or – to use the closely linked Nahuatl term – *nagual* or *nahual*) is a Mesoamerican cultural phenomenon that has received a great deal of attention among Mayanists (e.g. Brinton 1894; Foster 1944; Gossen 1975, 1996; Guiteras-Homes 1961; Saler 1964; Villa Rojas 1947; Vogt 1970). Yet it had not been documented in the hieroglyphic record.

Fully grasping the significance of this new reading requires some additional background on MacLeod’s research motivations as well as significant themes in Maya cosmology. MacLeod’s interest in Mesoamerican research was originally inspired in 1970 when she traveled on a caving expedition to Belize and noticed that the caves were full of archaeological artifacts.<sup>12</sup> Though she proceeded to pursue linguistic, rather than archaeological, research into the Maya, MacLeod retained her fascination with

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<sup>12</sup> Macleod, B. Personal Interview. 23 July 2007.

caves. She took a particular interest in caves as important ritual sites, construed as portals into or material manifestations of the Maya underworld.

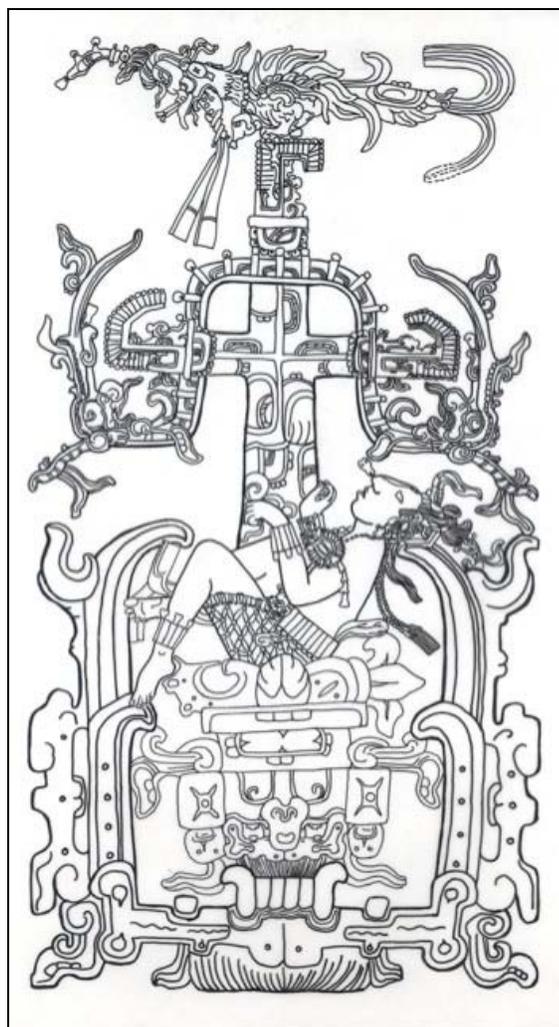


Figure 8-4. The central motif on the Sarcophagus Lid in the Temple of the Inscriptions at Palenque. Drawing by Susan D. Gillespie, reproduced with permission.

Indeed, as ethnographer Evon Vogt and David Stuart (2005:155) summarize it, “caves are ambivalent, boundary-line settings (between this world and the supernatural world) that, as posited by Edmund Leach and Victor Turner, actively generate ritual.” The status of the underworld as a kind of liminal location is apparent, for example, in conventional interpretations of the story of the “Hero Twins,” Hunahpu and Xbalanque, recorded in the sacred Kiche’ Maya colonial book, nicknamed the *Popol Vuh* by

scholars (Tedlock 1985). According to these interpretations, the Hero Twins are classic tricksters who, upon their descent and death in the underworld, survive an attempt by the Lords of Xibalba (the underworld) to permanently kill them. They proceed to fool the Lords into being killed themselves. Some scholars have read this story as an allegory for the passage of Maya souls from the middle world of life to the underworld of death and then, potentially, to the upper world of the cosmos, or the sky (Freidel et al. 1993; Schele and Miller 1986).

Thus, MacLeod had personal interests in how caves provided the ritual conditions for establishing contact between the worlds of the living and the dead, or between different states of consciousness. A paper coauthored by MacLeod and Dennis E. Puleston (1979) and presented at the third Palenque *Mesa Redonda* summarizes ethnographic, ethnohistoric, and archaeological inquiry into caves as Maya ritual sites. The paper additionally describes MacLeod's personal experiment with self-imposed sensory deprivation in a cave.

As I have established in my reading of Schele's *balan-ahau* interpretation, in the 1980s Maya hieroglyphic studies began to push beyond the world of history as manifest in the documentation of dynastic lineages and came to reposition Maya kings as religious beings with clear access to other physical or metaphysical domains. This disciplinary backdrop opened a space in which the cosmology-oriented claims of scholars such as MacLeod could find disciplinary legitimacy. With the aid of hieroglyphic texts, this epistemic field made it possible for MacLeod, Schele, David Freidel and others to legitimately research the multiple forms of spiritual life that inhabited and defined the ancient Maya cosmos. It was only appropriate that the well-

documented ethnographic phenomenon of spirit companionship, or the existence of a roaming, often zoomorphic, alter-ego, would have served an important social and cosmological function among the ancient Maya elite. Indeed, arguments that hieroglyphic signs held the value of *way* functioned to help further consolidate epigraphers' increasing control over the field of Maya studies. Whereas the *balan-ahau* reading marked a metaphysical turn in epigraphy, the *way* reading marked a trans-historical turn, as epigraphers increasingly made the case for three millennia of continuity in Maya ritual practice (e.g. Freidel et al. 1993; see also Chapter 6).

I have taken a necessary detour here into the enrollment of *way* and the range of contemporary cultural practices that travel with this concept into epigraphic interpretations. While this detour may seem methodologically costly in that it moves us away from the travels of T539, it has also provided necessary background into what this sign was to become in late 1989. Thus, the remainder of this section turns to the final articulation traced in this chapter, the coupling of T539 with *way*.

After MacLeod's March and April 1989 communications with Schele, the concept of *way* lost its status as a matter of concern within the body of documents to which I have empirical access. The *way* phenomenon had not yet garnered enough allies for the argument that it was an organizing principle of ancient Maya life to achieve any significant status within the field. Evidence for this claim comes in the form of a passage in the transcript of Schele's 1992 Maya Hieroglyphic Workshop in which she discusses the jaw of the zoomorphic underworld being depicted on Palenque's Temple of the Inscriptions Sarcophagus Lid (Figure 8-4):

It's [the jaw] the thing that Pakal falls down. He is in that bowl [the jaw] as he falls down the **Xibal Be** [the "road of awe," or Xibalba]. He falls into this

thing. It's called in the glyphs the "White Boned Serpent." It is also the south end of the tree. It's the hole that goes into the Otherworld.

Barbara MacLeod four or five years ago told me that thing was **way**. She showed me a whole bunch of places where **ya** was attached to the bottom of this thing. She showed me one where there was an **u** [a possessive particle: "his," "her," or "its"] up over the top of the hole and she proposed to me that it was a **way** transformation play. I didn't like it until last summer when I saw a new spelling of **Wayeb** on La Mar Stela 1 where here is the structure. Here is the **ya** and there's the **ah**. That seals it. It is **wayal** (Schele 1992b:122).

While Schele here attributes her changing evaluation of the merits of MacLeod's *way* reading to her encounter with a hieroglyph that bears closer resemblance to the imagery of the passageway into the Underworld, it is also clear that in late 1989 the viability and consequentiality of interpretations of spirit companionship among the ancient Maya elite had transformed profoundly.

In her 1990 Maya Hieroglyphic Workshop, as recorded in the transcript, Schele described the transformation of the *way* glyph in late 1989 as "a series of incredible events" (Schele 1990:31). From her position within the field of scholars, these events began in October, when she and David Freidel submitted their proposal for the book manuscript that would become *Maya Cosmos: Three Thousand Years on the Shaman's Path* (Freidel et al. 1993) to the non-academic New York publishing house William Morrow and Company, Inc. This marked a rising interest among Mayanists in the religious and cosmological practices of the ancient Maya, as well as a newfound interest by Schele in how contemporary Maya practices might elucidate the Classic Maya world. After submitting this proposal, Schele and Freidel attended a symposium on October 8 and 9 at the Dumbarton Oaks Research Library and Collection in Washington D.C. (which gave rise to an edited volume; Sabloff and Henderson 1993). There, according to her account at the Workshop on Maya Hieroglyphic Writing, Freidel informed Stuart

about the proposed “book on shamanism,” and Stuart, in turn, responded, “Look, we just found a glyph for sorcerer” (Schele 1990:31). This glyph, of course, is this chapter’s central analytical object: T539. Employing the same voice of revelation that marks the pages of her personal narratives in *Maya Cosmos*, Schele informed Maya Workshop attendees that, “I knew right away that David Stuart was right because I had so many examples of these in my head” (Schele 1990:31). She then claims to have elicited a letter from Stuart and Houston so that she and Freidel could cite it and use the reading (evidence of the high epistemic value attributed at this time to written personal communications among the small epigraphy collective). Schele proceeds to claim that she received the letter a mere 24 hours before an independent achievement of the same reading from Nikolai Grube arrived on her desk.

A series of documents in Linda Schele’s papers provide evidence for the general contours of her claims. The earliest dated document to which I have access that attests to the reading of T539 as *way*, “sorcery, nagual, transformation,” is a September 10, 1989 research paper handwritten by Grube (and sent from Germany with a letter dated October 19 and apparently received one day after the letter that she elicited from Stuart and Houston, described below). In the September document, Grube introduces T539 as a glyph “christened ‘Balam Ahau’” by Schele (1985) and accepts her assertion that the “glyph is part of the nominal sequence of the underworld supernaturals.” He proceeds to show examples of the glyph affixed with the same *wa* and *ya* phonetic complements that MacLeod located in her reading of T157/T769 as *way*. Mayanist epigraphers interpret phonetic complements as indicators of the logographic value of main signs, the scribes’ clues to how non-phonetic signs should be read. These

complements, according to Grube, “point to a reading WAY of T539.” In the letter, Grube reiterates this point: “the phonetic complements **wa** and **ya** clearly indicated that **WAY** is the only possible reading. This gives us really deep insights into Maya religion and connects it with recent ethnographic data.”<sup>13</sup>

While Grube’s paper was not published, Schele produced part of the draft as evidence of the T539-way reading for the audience of her Maya Hieroglyphic Workshop. After outlining the details of the argument in her workshop performance, Schele sought to allay any remaining witnesses’ misgivings by producing three pages of Grube’s paper as images projected onto the screen, reproduced as figures in the transcript. Remarkably, my comparison of the transcript’s figure of Grube’s first page with my copy of the original document archived in Schele’s correspondence points to how the production of public assent around these “really deep insights” entailed practices of cutting or erasing, quite literally, the non-hieroglyph actors participating in this relational field. Specifically, her imaging practices in the 1990 workshop deferred the controversy surrounding the politics and ethics of analyzing ceramic vessels that lacked provenience or were sold in the antiquities market.

In the original document, the last two hieroglyphs on the first page are labeled as follows: “MARBLE BOWL, UNKNOWN PROV. [provenience]” and “SOTHEBY VASE (1986, NOV. 24, #132).” In the document reproduced as a figure, Schele inconspicuously erased the line specifying the marble bowl’s unknown provenience and erased the entire label noting how the final hieroglyph on the page was simulated from a

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<sup>13</sup> Grube to Schele, 19 October 1989.

vessel sold in 1986 at the famous fine art auction house Sotheby's.<sup>14</sup> These are the only apparent modifications of the three pages reproduced during her performance. In other words, the practice of ethically producing objective knowledge of the past required a literal cut preventing the possibility that witnesses at the Maya Workshop might trace the network back to objects construable as sites in a controversy over complicity with the antiquities trade. Within the closed system of communication maintained among Schele and her professional peers, ethical matters concerning the antiquities market did not infringe on epistemological claims, as they had, in reducing hieroglyphs to writing, effectively extricated the signs from their sets of original associations. Since such an issue might have provoked audience members' intervention into Schele's narrative, she may have censored the document, potentially deferring ethical concerns in constituting hieroglyphs as pure objects of knowledge untainted by their political economic objectification.

Of course, Grube does not comment on the viability and ethics of using such signs as evidence in his list of examples. In the paper, he turns from these signs, aesthetically extricated from their complex sets of associations, directly to the semantic complex of the term *way* in the Yucatec language and ethnographic anecdotes attesting to twentieth-century shamanic practices associated with this term. For example he cites the legal pardoning of a man who killed his neighbor, a shaman (*hmen* in Yucatec), while the neighbor was in the form of a cat, his *way* (Bolles 1982). In the October 19 letter he further clarifies the ethnographic basis of his reasoning: "The idea for this reading came to me in August when I talked to various Mayans in [the state of]

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<sup>14</sup> It appears that this particular artifact was sold in the November 24, 1986 auction of Inca and Maya "Pre-Columbian Art" at the New York location of Sotheby's.

Quintana Roo who told me of a sorcerer who is able to transform himself into a cat or a spider monkey. They called the animals in which the sorcerer transformed himself *u way* ‘his nagual.’” Having detoured into ethnographic evidence in the research paper, Grube’s argument turns back to the linguistic. The semantic complexes associated with this term lead him to claim that “This is a Proto-Maya word (\*WAY) for ‘nagual’ and ‘brujo’ [witch] and is related to \*WAY ‘sueño’ [dream].” This claim, in turn, leads to the implication that if T539 “is WAY “nagual,” the glyph U WAY on the pottery scenes refers to the underworld monsters as the NAGUALS of the AHAUS and titles in the final part of the nominal texts.” Grube then proceeds to summarize some of the specific examples of scenes that invoke the *wayob* (the term’s plural) of specific lords, speculating on whether dynastic lineages shared individual *wayob*.

Grube’s interpretation – which he had apparently conveyed to Schele in an earlier communication that month – apparently sparked Schele’s interest, causing her to inquire about the concept of the *way* to David Stuart. This inquiry served as the rationale for a letter that she received from Houston, also dated October 19, 1989. While Grube’s September 10 document and October 19 letter outline an argument for the status of T539 as a sign for *way*, the opening sentence of Houston’s letter presumes that the semantic value of this glyph was then a settled matter of fact: “David tells me of your interest in the nagual glyph.”<sup>15</sup> Houston’s explanation of the issue entails an introduction in which he attributes the definition of the *way* as a “‘co-essence,’ ‘an animal or celestial phenomenon...that is believed to share in the consciousness of the person who “owns” it’” to the ethnographer and ethnohistorian John Monaghan. He notes ethnographic evidence of the “unusual behavior” and “aberrant” nature of co-

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<sup>15</sup> Houston to Schele, 19 October 1989.

essences: “They are like creatures of nature, and are indeed thought to reside in the monte [a Castilian term for the hills or forest]. But they are not animals per se.”<sup>16</sup> He further emphasizes George Foster’s (1944) claims that co-essences emerge in a period of trance and are represented by masked figures in Maya dances, apparently the rationale for colonial authorities’ prohibition on both masks and dances.

Thus, like Grube, Houston takes the colonial and contemporary existence of co-essences among Mayan-language speakers as a matter of fact. As I have emphasized, the field’s ontological politics of the 1980s had given rise to claims to the trans-historical qualities of Maya culture. Situated within this politics Houston’s letter bespeaks how he was compelled to construct the ostensible absence of references to *wayob* in the hieroglyphic corpus as an “enormously puzzling” problem that “suggests that perhaps we have not looked for evidence in the right places.”<sup>17</sup> If co-essences exist in the present, they apparently *must* have existed in the past. Houston then turns to T539, the half-spotted *ahau*, claiming that “Several months ago I concluded that its reading must be *way*.”<sup>18</sup> As in Grube’s evidence, the basis for this claim rests on the patterns of affixes attending T539 as a main sign.

Further, like Grube, Houston puts forth the probable notion that in Classic Maya usage, members of a patrilineage may have shared a single co-essence. Grube and Houston independently came to question whether Classic Maya patrilineages shared *wayob*, and they independently likened this practice to the culturally distinctive totemism ethnographically documented among patrilineages (*onen*) of the Lacandon Maya of

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<sup>16</sup> Houston to Schele, 19 October 1989.

<sup>17</sup> Houston to Schele, 19 October 1989.

<sup>18</sup> Houston to Schele, 19 October 1989.

Chiapas (e.g. Bruce 1975).<sup>19</sup> Finally, Houston's letter also makes reference to Stuart's interest in texts that refer to Maya temples as *waybil*, read as a designation of the "sleeping places" or "tombs" of either lords or gods.<sup>20</sup> Schele's response to Houston concedes the dissolution of the *balan-ahau* reading in the face of this new set of claims, noting that her argument was limited by the then-undeciphered status of the *ya* phonetic complement (T126). She concludes the letter by calling the *way* reading a "remarkable discovery" over which she is "a little awestruck."<sup>21</sup>

Grube's and Houston's independent accomplishment of this "remarkable discovery" is further evidenced in a letter that Grube sent to Houston and Stuart and carbon copied to Schele (a common practice among this group of scholars). Grube opens the letter by stating "from your recent mailings I realize that we were working on the same thing again and more or less with the same results. What a coincidence!"<sup>22</sup> Grube proceeds to take up the *waybil* theme, adding further examples of *way* scenes and *waybil* references to the discussion.

The evidence was mounting. The matter-of-fact reading of T539 as *balan-ahau* had crumbled beneath the weight of the T539-*way* articulation. In November 1989, Houston and Stuart submitted the argument that they had been working out and collaboratively sharing during the preceding two months to the series Research Reports on Ancient Maya Writing. In December the series published the paper, "The *Way* Glyph: Evidence for Co-Essences Among the Classic Maya" (Houston and Stuart 1989).

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<sup>19</sup> Grube to Stuart and Houston, November 15, 1989.

<sup>20</sup> Houston to Schele, 19 October 1989.

<sup>21</sup> Schele to Houston, 30 October 1989.

<sup>22</sup> Grube to Houston and Stuart, 15 November 1989

The profound alteration of the epistemic field that they attribute to this reading is utterly apparent in Houston and Stuart's (1989:13) concluding paragraph:

In our judgement, the way decipherment fundamentally changes our understanding of Classic Maya iconography and belief. It indicates that many of the supernatural figures, once described as "gods," "underworld denizens," or "deities," are instead co-essences of supernaturals or humans. More than ever, then, Classic Maya beliefs would seem to coincide with the general patterns of Mesoamerican thought. We are also convinced that the reading undermines the "mortuary" or "underworld" hypothesis of Maya vase painting. Elsewhere we have shown that pottery texts, once held to be descriptions of the heroic conquest of death, record more mundane matters, such as vessel types and beverages (see Houston, Stuart, & Taube 1989) [sic]. It appears now that much of the imagery on ceramics relates to Maya perceptions of self. As a result, death and the afterlife can no longer be regarded as the dominant themes of Maya pottery art. Our final point concerns the certainty with which Maya lords identified their co-essences. Today, this skill is restricted to the more powerful and well-educated members of traditional Maya society (Villa Rojas 1947:583). For the Classic Maya, such self-knowledge may well have been an important marker of elite status.

In my interpretation, this summation reveals how the epistemological legitimacy of reducing T539 to a single morpheme turns on how such readings articulate the hieroglyph with a multitude of relationally distinct actors within the particular material-semiotic field of Maya studies. T539 itself produced Houston and Stuart as mediators aiding in its articulation with the way concept, which – as evidenced by MacLeod, Schele, and Freidel's concerns with shamanism – inhabited the collective imaginary, if not the "superorganic" (Kroeber 1917), that gave meaning and legitimacy to the epistemic work of decipherment.

Strengthening this reading entailed more than T539's acquired capacity to diffusively proliferate alliances, as in the conceptual models that Latour (1987, 1988) produced in his earlier research (cf. Haraway 1997; Strathern 1996). Indeed it also entailed the negative work of cutting away associations, including the ethical claims of

those opposed to the antiquities market, and the various material, aesthetic, archaeological and political-economic actors associated with the wide range of semiotic artifacts designated by Thompson's "T539" gloss. This material-semiotic work also intervened into Schele's (1985) *balan-ahau* reading, partially undermining her argument, in favor of an alliance that extended the sign's articulation with a suite of Maya and Mesoamerican cultural practices with tremendous cultural-geographical breadth and historical depth, including invaluable connections to the present.

T539's newfound connectedness to a diffuse assemblage of shamanistic practice has not gone entirely uncontested. For example, in a critique of the application of the concept of "shamanism" to Mesoamerican Art, art historian Cecelia F. Klein et al. (2002:392) suggest that interpreting the term *way* as "co-essence" rather than simply "sleep" or "dream" fits within broadly ethnocentric (and, in my view, Orientalist) generalizations of the concept of shamanism, for which liberal application of the term *nagual* is clear evidence. I acknowledge that Klein et al. (2002) give short shrift to the linguistic basis of the term's meanings beyond direct references to sleeping and dreaming, as Stuart points out in his reply to their article (Klein et al. 2002:410-1). Indeed, my primary claim is not that the *way* reading fits into a prefabricated "savage slot" (Trouillot 1991), but, rather, that the epistemological viability of such readings depends on how they articulate alliances with appropriate co-actors within the disciplinary space of Mayanist knowledge production. Without cutting away the epigraphers' access to the supernatural, the *way* reading also opened up connections with the present. These connections, however, are sites that demand close attention

and critique, including ethically complex challenges to the notion of a single coherent Mayaness.

### Conclusions

Epigraphers and hieroglyphs mutually expanded their influence throughout the assemblage of Maya studies by mediating ancient Maya elites' associations with actors to which they were previously unrelated or unclearly related: Maya gods and contemporary Maya practices. In this chapter I have traced this set of emergent mediations through the transformations of one particular actor (T539) and its allies. It is thus a demonstration of how a Latourian perspective, an onto-epistemology distinct from both conventional realist and anti-realist stances, has significant potential to illustrate the material and semiotic workings of innovation and transformation, or – in a very literal sense – world-making.

In staying close to the movements of T539 as it transformed from “half-spotted *ahau*” to “*balan-ahau*,” and then “*way*,” I have illustrated the kinds of positive and negative work entailed in the constitution of a single complex sign as an object of knowledge. This method presents an object-oriented, empirical alternative to the modified realism embraced by Houston, Mazariegos, and Stuart (2001). My perspective demonstrates how the interpretation of “T539” is an aesthetic and material reduction of a multiplicity of distinct and very real objects into a categorical type (of material-aesthetic manifestations amenable to reproduction) that makes sense within a structuralist scientific logic of categorization and a linguistic ideology that foregrounds the semiotic transparency, literalness, and context-independence of text. These complicating dimensions of hieroglyphic knowledge production also entail distinct ethical problems, particularly in a context where the value of the *way* reading issues largely

from how this interpretation closely connects the practices of those constituted as past and present Maya subjects. In the final chapter of this study, I turn to the broader implications of these complex ethical problems.

## CHAPTER 9 CONCLUSIONS

### **The Jungle and the Ant Colony**

The cover of Merle Greene Robertson's (2006) autobiography boasts the title *Never in Fear*. To me, this forthright declaration seems to provoke potential readers to question the author's certainty. Moreover, the title calls us to ask "fear of what?" What foes did Robertson face with such imperturbable poise, and how might the actors in question have incited the trepidation of a less confident peer? Fear of what? Readers find an incipient answer in the first two paragraphs of her book:

His bones were found two years later. His Hasselblad camera was there also. This German photographer did not know the jungle rules. Never, never sit anyplace without being sure it is safe. That is – no snakes. Never go off a trail by yourself. This man had undoubtedly never been to Tikal before, nor had he told anyone where he was going. He was off doing his own thing, by himself, and he needed to change film in his camera. He walked off the trail, his first no-no. He then hunted for a place to sit. He sat on this stump to change film, and was undoubtedly bitten by a coral snake.

Yes, I did go off into the jungle by myself at Tikal, but I never went off a trail or what looked like a trail. As I proceeded further along these faint traces that often looked like abandoned army ant trails, I would turn around and draw a sketch of the trees and anything of note that I could see. Each time I came to another questionable trail, I did the same thing, so that I would be able to find my way back. And of course I told someone in what general direction I would be going on these excursions into the jungle.

Fear the jungle. Its foreboding trees shadow threatening alterities, including Nature's rather Judeo-Christian metonym, the snake. Robertson's fearlessness in the face of this encompassing peril does not merely arise from modernist self-certainty or confidence in the power of positive thinking, though there was plenty of that too. It is something more, perhaps a confidence in art, art as a tool for survival. Sketches save lives?

This may be the case. And, yet, despite her appeal to reasoned careful consideration, given form as realist depiction, there is something purposefully naïve in Robertson's negation of fear. There is a youthfulness to her prose, which couches her self-styling as explorer, that figure of the triumphant commander who refuses to change course even when all rational deliberations demand it. I cannot help to think that this certainty obscures a less assured ambivalence just barely beneath the surface. Should I have feared?

I lack recourse to the certainty of Robertson's title, and for more reasons than my innumerable artistic inabilities. I fear that I might not be right. Of course, I know better. I know my uncertainty, my epistemological anxiety, to be a resource, and even a provocation to the self. Self-certainty entails little experimentation, in science and elsewhere. For this reason, many of those in science studies know that the best science combats closure, resists suffocating propositions by reducing them to "facts." Of course, even the best scholars sometimes fall victim to the allure of facts dressed in different clothes. Perhaps Bruno Latour (1999d) was onto something with his quippy term "factish," an attempt to give due to the material production of facts by foregrounding the etymological entwinement of the words "fact" and "fetish."

But I am turning down a different path too quickly here, as there is more to Robertson's opening paragraphs than a narrative heralding the certainties of artistic skill. It is, of course, the allegorical summation of a successful career in ancient Maya studies. The transient, individual German photographer is a foil for Robertson's dedicated and sustained documentation of sites such as Tikal through rubbings and photography. Unlike the German, Robertson seized upon the impression of human

action into the landscape, the trace of predecessors' repeated movements. On her trail to the ancient, she progressed down that great chain of beings, tracing the footprints of humans, then perhaps the hoof marks of deer, before finding herself in the shadows of ants, insect companions of the lowliest order.

Robertson's journey into the jungle and escape from the dwellings of modernity was a collective endeavor. She followed the pathways of past travelers, human and nonhuman alike. Such walking entails intimate engagement with the particularities of place. As Alice Legat (2008:47) puts it in a chapter on the linkage between footprints and stories among the Tłı̄cho of Canada's Northwest Territories, "To walk is to pay close and careful attention to one's surroundings while thinking with the multitude of stories one has heard. Similarly, following footprints is about gaining knowledge through action and the ability to use that knowledge." Robertson's trail walking is a matter of thinking with stories, and of gaining and using knowledge. Among Mayanists, this a distributed knowledge, an epistemic field that pays little heed to the distinction between past and present, and, for that matter, human and insect. Her coordination with present actors allies the care of a less vulnerable human peer. Her coordination with past actors affirms the practices, the pathways, of archaeologists and ants who have made Tikal's "vibrant matter" (Bennett 2010) all the more vibrant. Her simple stroll down a jungle path emerges as an intricately coordinated set of actions. As such, it sums up the ethos of ancient Maya studies in the era of hieroglyphic decipherment.

This study has followed only a few of the pathways that Robertson and her peers have inscribed into Maya studies' epistemic terrain. In tracing the coordinated actions of Mayanists re-inscribing ancient inscriptions, retracing pathways past, I have tried to

stay true to the ontological imperatives of Latour's methodology. In his 2005 book *Reassembling the Social*, Latour reinstated the previously "recalled" (Latour 1999c) acronym "ANT:" "I was ready to drop this label for more elaborate ones like 'sociology of translation,' 'actant-rhizome ontology,' 'sociology of innovation,' and so on, until someone pointed out to me that the acronym A.N.T. was perfectly fit for a blind, myopic, workaholic, trail-sniffing, and collective traveler. An ant writing for other ants, this fits my project very well!" (Latour 2005b:9). "An ant writing for other ants;" Latour proffers his figure of the actor-network theorist in arthropodic terms that embrace an utterly ethnographic ethic. "Follow the actors themselves" (Latour 2005b:12). Follow them following other actors, following other actors in turn.

Where does following Robertson lead us? To Tikal's monumental structures, with temples that rise above the jungle's canopy? Or, perhaps, to insects' equivalents, anthills? Ants at Tikal and elsewhere in Central America build hills of giant proportions relative to their body-size. When recognized for what they are, this architecture of sand and dirt powerfully disrupts many North American travelers' entomological sensibilities. I think, with some reservation, that Latour's conceptual architecture is not unlike the ants' collective endeavors. But I also recognize that Latour's concept of ants is rather anthropocentric. He has chosen an odd figure for his flat ontology. Ant "colonies" house "societies" organized into "castes." While conspicuously applying this mimetic and hierarchical rhetoric, biologists have noted a positive correlation between colony size and social differentiation (Anderson and McShea 2000); among themselves ants are hardly democratic.

I want to encourage readers to consider the organization of the four analytical chapters in this study through this metaphor of the ant colony, even if the metaphor manages to seem both overdetermined and underspecified. I made a conscious effort to research and depict the bustle of multiple, differentially-interested actors gathering in the Austin and Antigua workshops, where I was just one among the ants following along. In contrast, I did not realize that the sections of my final two analytical chapters were devoted primarily to and titled for *individuals* until I was completing their authorship. It is as if I fatefully balanced the analytical scale with the collective on one side and the individual on the other. Multiplicity and singularity. Is this not merely an indulgence of the exasperating and overwrought opposition between the collective and the individual that still marks social theory that struggles with the legacies of Marxist and structuralist epistemologies?

Perhaps it is, but unintentionally so. What I have tried to craft is something other entirely. It is a mode of thinking that stays true to the democratic ethic of Latour's methodology, while it also attends to how specific gatherings and media perform the dual work of inclusion and exclusion, allying and cutting, all in the effort to craft and circulate a vivid and salable image of "the Maya." The strength of this image rests on its connectedness to individual producers, and its attendant status as an unassailable object for critics who do not possess or embrace epigraphy's distinctive and ever-changing methodological tools. But my intention is not to critique. My intention is to open up the blackboxes of Maya epigraphy, to reveal, ethnographically, the actual processes by which images of the Maya have emerged and acquired popular traction. If anything differentiates this project from the critical stances carried along by the

opposition between the collective and the individual, it is my attempt to work through a different set of empirical-ontological tools, particularly a commitment to destabilizing the human-object distinction, and attending closely to how Maya hieroglyphs themselves enact worlds. The hieroglyphs have always existed in relation with different collectives of humans, and through articulations with Mayanists they have acquired new properties and new forms.

If I am uncertain or uneasy about anything here, it may be how much attention I have devoted to humans in a study that claims to contribute to an ontology resisting “human exceptionalism” (Haraway 2008). But bear in mind that the lesson may be that any attempt to understand humans must apprehend the vital ecology of things that animates them. Hieroglyphs have clearly played an active role in this ecology. To use philosopher Jane Bennett’s (2010:x) words, hieroglyphs are among “the vital materialities that flow through and around us.”

In the chapters on Austin and Antigua workshops I attempt to hash out the constitution and translation of these vital things as matters of concern around which specific publics formed and became integral contributors to ancient Maya studies through the adoption of shared material practices. These workshops are tangible locations where the concept of a singular, coherent Maya culture achieved public life. To return one last time to my allegory, this structure of knowledge production is not totally anathema to the structure of life in the ant colony.

Biologists term ants a “eusocial” species. This term designates a distribution of reproductive labor. Namely, the queen ant produces offspring that are subsequently cared for by members of nonreproductive castes. Schele and her peers in the

professional ranks of Maya studies produced (and were produced by) their historical propositions, and in the process they established acceptable methods for rendering viable knowledge of the ancient Maya. The attendees embodied and cared for these methods and historical propositions, as their modes of thinking were channeled by simulated hieroglyphs. In Austin, they produced an assemblage of quasi-scholarly media to document their own contributions to understanding the ancient Maya through the inscriptions' mediation. In Antigua, activists and community leaders achieved an understanding of the basic principles of hieroglyphic writing and mobilized these principles to help secure a sense of historical and cultural unity. These lower-caste ants relayed between the colony and the world beyond, bringing back sustenance for the colony's permanent occupants and caring for the ancient world that they imaginatively crafted.

How does this busy world fit with the final two analytical chapters, which focus less on the public life of epigraphy? The ethos shaping ancient Maya studies during the 1970s and 1980s was prominently and self-consciously collaborative. So what should I make of my close attention to how the field has been shaped by the thoroughly specific and personal contributions of individuals? In these final two chapters, I have followed actors into the subterranean depths of the colony, tracing how specific material-aesthetic practices have thoroughly shaped what Maya hieroglyphs could become. Original hieroglyphs exercise their own powers, and function as knowable objects through their tangible, direct associations with the worlds in which they were and are situated. The secondary originals, or simulations, condition entirely new possibilities.

Particularly, they equate hieroglyphs with the kinds of standardized, modernist writing conditioned by the invention of the Gutenberg press in 1440 (see McLuhan 1962).

In rendering Maya hieroglyphs a singular object, and their status as text an unassailable matter of fact, epigraphers have closed a wide range of possibilities for understanding what kinds of semiotic, affective, and social functions these objects performed. While I do not deny that they functioned in ways that resemble contemporary writing, I also recognize that their vital powers to gather and constitute new assemblages in the present hints at how they are complex objects irreducible to contemporary notions of writing. As a matter of ethical and ontological concern, the argument for their status as writing is thoroughly embroiled with a set of popular representations of the Maya that invite speculation about the ethical effects of this singularization. Thus I now turn to these broader questions about the ethics and consequences of the rise of epigraphy.

### **Companion Species in an Age of Empire**

In her book *When Species Meet*, Donna Haraway (2008) grapples with how laboratory scientists might better conceptualize responsibilities to their animal co-workers. In the process, she also marks her divergence from humanist, Marxist, and feminist frameworks that unconditionally oppose experimental lab practices subjecting animals to pain or death. Yet, Haraway seeks to cultivate an ethical sensibility and sensitivity towards our companion species. Such sensibility should resist anthropomorphism and entail recognizing how our lives impinge on the organisms around us. Bluntly summarized, killing others is a necessary condition of life. Such recognition, however, should not leave us unfazed. Rather, the necessary and costly entanglement of our lives and livelihoods with human and nonhuman others should

leave us in a perpetual state of discomfort, dis-ease. Playing with the Latin root of the term “companion” – *cum panis*, meaning “with bread” – Haraway thinks that our impingements on others, our experiments on others, and our consumption of others should leave us in a state of indigestion. We must learn to care better, to kill better, and to share in the suffering that we enact.

My reading of Haraway’s *When Species Meet* (2008) took place in a laboratory where no experimental animal is likely to meet its fate any time soon: the University of Florida’s “Mesoamerican Archaeology and Iconography Laboratory.” There, I came face to face with the Maya culture crafted by Mayanist epigraphers and their imaging technologies. It appeared that the ancient Maya elite became warlords who commanded their city-states’ violent conflicts, and indulged in forms of ritual self-mutilation.

Problematically, hieroglyph experts’ image of savage, sanguinary ancient Maya began to achieve academic and popular attention in the mid-1980s. At the time, Guatemalans were experiencing the wake of the most violent episode of a 36-year civil war. In March 1982, a CIA-backed coup d’état overthrew the elected president, Ángel Aníbal Guevara Rodríguez, and established Efraín Ríos Montt as the de facto Guatemalan dictator. Ríos Montt instituted a genocidal scorched earth campaign responsible for the bulk of the 200,000 people killed or disappeared and the 1.5 million people displaced during the war. Most of those killed and displaced were Maya-language speakers. Many lacked any affiliation with the leftist insurgency. The United Nation-sponsored Commission for Historical Clarification later declared that “The massacres, scorched earth operations, forced disappearances and executions of

Mayan authorities, leaders and spiritual guides were not only an attempt to destroy the social base of the guerrillas, but above all, to destroy the cultural values that ensured cohesion and collective action in Mayan communities” (quoted in Manz 2004:3-4).

As discussed in Chapter 6, the civil war subsided in the late 1980s and class-based solidarities gave way to more explicitly ethnic politics. At the time, some leaders of the rising pan-Maya movement expressed their discomfort with the trope of the “savage Maya.”<sup>138</sup> But, in general, their criticisms took a back seat to their attempts to gain access to intellectual tools necessary to interpret Maya hieroglyphs for themselves, which I have described, in part, in Chapter 6.

At this same time, a range of Maya and non-Maya scholars working in academia began to accuse hieroglyph experts of reproducing colonial stereotypes. Mayanists professionally vested in bellicose narratives have reacted harshly to such accusations. This ethical controversy emerges as the analytical crux that should motivate subsequent research in science studies-oriented analyses of decipherment. I am non-innocent and may very well be wrong. But, I think that an ethics of care heavily inflected by the nonhumanist positionality of postcolonial science studies provides tools to articulate an alternative to these ostensibly contradictory desires. How might we accommodate epigraphers’ efforts to represent the ancient Maya in ways that seem attentive to existing semiotic and material traces without eschewing the present consequences and complicities of painting these ancient landscapes red? I argue that both the critics and the criticized must meet three conditions for any reconciliation to take place. First, extending Haraway (2008) and Latour (e.g. 2004a, 2005), epigraphers and their critics must open their ontological and ethical fields to respond to nonhumans – including

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<sup>138</sup> García Matzar, L. P. Personal Interview. 28 January 2008.

*former humans* – as legitimate actors. Second both collectives must recognize the distinction between representations that indulge the ubiquitous act of killing and representations that become complicit in rendering categories of beings – here, principally Maya humans – killable. Third, both collectives must express willingness to share in the suffering enacted directly or indirectly by their interpretations and critiques.

### **Ethical Binds over the Savage Maya**

In May 1986, the Kimbell Art Museum in Fort Worth, Texas opened an exhibit on the ancient Maya. “The Blood of Kings: A New Interpretation of Maya Art” stylized trends in understanding rulership and artistic production in Maya city-states for a public audience. With dust-jacket reviews by Claude Lévi-Strauss and Octavio Paz, the exhibit’s catalogue marked its place as something more than a scholarly publication. It proposed a popular face for the ancient Maya. The catalogue postulates that hieroglyphic decipherment salvaged the romanticized Maya kings from the obscurities of prehistory. As museum director Emily Sano puts it in her “Forward,” “It is now possible to show the Maya were a real people, who lived, loved, hated, created, possessed and destroyed in a manner characteristic of human beings” (Sano 1986:ix).

Some were quick to protest that the epistemologically troubled attempt to make the Maya more “real” and more “historical” through literalist interpretations of their inscriptions as texts had resulted in well-worn stereotypes of the “savage Indian.” Attempting to clarify disputes surrounding the exhibit and catalogue in a 1988 review article, art historian Cecilia Klein (1988:42) detailed a litany of objections to the *The Blood of Kings*, including the authors’ proclivity to cater to a public audience with violent romanticism. Schele and Miller (1988) responded to Klein’s attacks with vitriol, accusing her of a presentist, ethnocentric “Western, Marxist presumption.” Such mutual

accusations of ethnocentrism effectively nullified any attempt to cultivate a dialogue between hieroglyph experts and their critics.

Indeed, a parallel ethical controversy emerged again in the past decade. In 2002, the University of British Columbia hosted a symposium entitled “Towards a More Ethical Mayanist Archaeology.” The symposium explored the notion that Mayanist archaeologists lagged behind peers working in other regions – particularly North America – in addressing the effects of their studies on contemporary descendent communities. The symposium’s webpage framed the event with “An Example of the Problem.” The example interpreted a touristic poster which depicts architecture from the Maya site of Caracol (in Belize) in the background, a ball court marker in the middle, and violent Maya warriors bursting from the foreground.<sup>139</sup> Visitors to the website could read the organizers’ commentary, which cited Schele and David Freidel’s claim that the ball court marker’s short text related Caracol’s “conquest” of Tikal. The organizers conclude their example with two questions: “What primitivist tropes has the artist used to make the ancient Maya seem savage and barbaric?” and “[h]ow do you think ancient Maya people should be presented to the touristic public in ways that are respectful of the needs and aspirations of contemporary Maya people?”

The symposium’s invitation upset scholars invested in the epigraphic interpretations under attack. In their reflections on “Maya Epigraphy at the Millenium,” Stephen D. Houston and Alfonso Lacadena (2004:118) point out the “acerbic and, in our view, unfair rhetoric” of symposium organizer and art historian Marvin Cohodas. They call the discussion “an emotional minefield” and argue that it amounts to a direct assault

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<sup>139</sup> The website, <http://ethical.arts.ubc.ca/Introduction.html> (accessed February 8, 2009), has since been taken down.

on their scholarly freedom. Likewise, in his contribution to a 2007 edited volume on *The Taking and Displaying of Human Body Parts as Trophies by Amerindians*, Mayanist archaeologist Arthur Demarest (2007) accuses some critics of adopting “denialist” perspectives towards violent cultural practices. This denialism, he further claims, issues from critics’ own culturally specific attitudes towards death.

Such controversies seem to demand that their “virtual witnesses” (Shapin and Schaffer 1985) take a side; either epigraphy succumbs to ethnocentrism or cultural criticism does. My position holds that a nonhumanist, consequentialist ethic – and, perhaps, epistemology – of care offers an alternative to this mutually sustaining ethical bind. In the remainder of this section, I provide a summary of how the three conditions that I have proposed may ground such an alternative.

### **Caring for the Formerly Human**

First, epigraphers and their critics must open their ontological and ethical fields to respond to nonhumans – including *former humans* – as legitimate actors. Both the experts and their critics seem to presume that the subjects of ancient Maya historical narrative are no longer present. In my position, death and the dissolution or dispersion of the body may alter the *kinds* of actions that humans perform. But it does not negate the subject’s reality and capacity to act. This alternative entails diverging from the secular humanist perspective that circumscribes the person at the body’s limits and assumes these limits to be immutable. The production of historical knowledge necessarily entails the assumption that the person is dispersed throughout, or at least conditioned by, its material associations. That the ancient Maya are no longer operating as subjects defined by our biopolitical field does not mean that they are absent. They

are present through their material artifacts and, as a result, these artifacts demand forms of ethical response.

For those directly studying artifacts, such ethical response should entail direct acknowledgement that the Maya continue to collaborate in producing partial knowledge of their sociomaterial worlds. Pressed into a contemporary historical aesthetic, such artifacts and their subjects are making history, but not under conditions of their own design (to paraphrase Haraway paraphrasing Marx). For those who criticize epigraphers and archaeologists, this redefinition of the field of actors may require an even more profound response. Namely, the analysis of nonliving historical actors should not be interpreted as a matter of secondary importance to work with living Maya.

An implication of this position is that “community archaeology” (e.g. Ardren 2002; Marshall 2002; Pyburn 2008) – or the incorporation of community members into the execution of archaeological projects and the interpretation of excavated materials – and epigraphers’ collaborations with Maya language-speakers remain insufficient substitutes for grappling with how scholars have played mediating roles constituting the troubled ethnic, linguistic, and biopolitical category “Maya.” Further, ethnographers’ common complaints that greater popular attention is paid to the ancient Maya than the living Maya (e.g. Manz 2004:7) fails to apprehend the profound political significance of the ancient within the present and perpetuates a modern biopolitical circumscription of “life itself.”

Second, both collectives must recognize the distinction between representations that indulge the ubiquitous act of killing and representations that render or become complicit in rendering categories of beings killable. I agree with scholars of the ancient

Maya, such as Demarest (2007), that there is nothing intrinsically problematic with representing ancient Maya violence. As critics such as Cohodas have emphasized, such representation requires sensitivity to its potential reproduction of colonial and racist stereotypes. However, critiques that solely amount to identifying and responding to “primitivist tropes” may ultimately trivialize the political, material, and bodily consequences of control over historical narrative by aestheticizing the issue. Here, instead of engaging in critique, the Mayanist institution requires a multiparty effort to trace the consequences effected by circulating historical representations. Maya scholar Victor Montejo (1993:15) has asserted that representations of ancient violence indulged by scholars such as Demarest and the *Blood of Kings* authors aided Ríos Montt and the Guatemalan government’s efforts to morally legitimate genocide. Sensitivity to stereotypes is an insufficient countermeasure to such possible complicity. Instead, an institutionalized shift towards ethical consequentialism in Maya historical knowledge production is required.

This leads to my third position. Both collectives must express willingness to share in the suffering enacted by their interpretations and critiques. Scholars of the ancient Maya, and perhaps even their critics, will assert that they cannot control subsequent uses of their representations. Indeed, as Latour (1987; Latour and Woolgar 1986[1979]) has shown, the translation of attached artifacts into detached facts is fundamental to knowledge production. Feminist science studies scholars such as Haraway and Sandra Harding (2008) have responded to this detachment by advocating standards of “strong objectivity,” in which acknowledging human involvement in fact-

production is held to increase rather than decrease objectivity. Such attachment is a necessary condition of sharing suffering in historical knowledge practices.

Minimizing and sharing suffering in a laboratory setting may be easier to imagine than performing an analogous task within the field of Maya cultural politics. But I am certain, at least, that mutual accusations of ethnocentrism fail to sufficiently address the problem. Perhaps more explicit attention to the performative effects of discussing “the Maya” as a transhistorical cultural entity is required. Such a position may exist in a relation of irresolvable friction with Maya activists’ “strategic essentialism” (see Warren 1998). But, to minimize and share suffering in such a dispersed field of historical discourse will require more careful attention to the movement of images of the Maya from academic publications into popular media and government channels. Ultimately, there may be a point of convergence and productive tension between the positions of strategic essentialists and my own claim that Maya artifacts are consequential contemporary actors that demand forms of ethical response.

Indeed, the critics might better achieve their goals by working *with* hieroglyph experts to trace how historical representations enact suffering in different contexts. Such a renewed companionship would, no doubt, entail mutual compromise and more than a little indigestion.

### **Cuts, Care, and Future Directions**

In the past decade, the epistemological politics of Mayanist epigraphy have changed markedly. Particularly, the passing of Linda Schele in 1998 and the subsequent generation’s heavy emphasis on linguistic methods (see Wichmann 2004) have altered both the contemporary politics of decipherment and the representations of the elite ancient Maya world. The ancient Maya world that Schele imagined revolved

around ceremonies, rituals, and elite engagements with the otherworld, a reading that culminated in an argument for three millennia of ritual continuity among the Maya (Freidel et al. 1993). This was the world that publics witnessed and helped to perform in the Maya Meetings and Antigua Linguistics Workshops. It was a world in which glyphs such as T539 played a central, defining role, due to their capacity to provide direct access to the metaphysical, manifest as Maya beliefs. While this cosmological turn in epigraphy continues to shape representations of the Maya, there is growing emphasis on the interpolity dynamics of war and rule (e.g. Martin and Grube 2008). It is hard to read this trend in ancient Maya studies as fully extricable from post-Cold War shifts in political economy and attendant notions of the person.

In other words, as the public, collaborative ethos – if not practice – of ancient Maya studies in the 1980s and 1990s has given way to a research field characterized by a greater degree of individual proprietorship, have the ancient Maya elites also begun to look like rational individualists? As this manifests in the practice of collaboration among Mayanists, what are the politics of engaging contemporary Mayan-language speakers through public workshops? How does this act of “inclusion” configure with the imperial history of social scientific research in Mexico (Tenorio Trillo 1999), the troubled and troubling idiom of “tolerance” in the United States (Brown 2006), and the evisceration of political and economic opposition to oligarchy through the depoliticizing relativism of “neoliberal multiculturalism” (Hale 2005)? In this study I hope to have provided some substantial bases for addressing these questions more thoroughly in future work.

I am further committed to raising and addressing such problems through the conceptual and empirical tools of science studies, even if this entails a perpetual tension with the interdisciplinary field's close adherence to biosciences and technosciences. Indeed, this study has explored how the anthropology of science should not be limited in principle or in practice to the analysis of sciences such as genomics, physics, and biomedicine. If the Maya activists' interest in the interpretive apparatuses of linguists and epigraphers can be taken as evidence, any hierarchy defining which sciences matter more to "the public" is an inevitably local, national, and regional affair. As scholars continue to extend cultural studies of science to assemblages outside the Euroamerican laboratory, they should take into consideration what kinds of knowledge play significant roles in local practices that intertwine the inseparable worlds of science and politics.

I have offered some suggestions for how we might enact postcolonial science studies differently. I think that this entails attending to how histories, objects of unquestionable importance in the making of contact zones between cultures (Pratt 1992a) and species (Haraway 2008), emerge through tangible material practices. Mayanist epigraphy is a valuable site of this extension for a variety of reasons. Among these reasons is the rather pervasive casting of Latin America as a "laboratory" for the social sciences, a term that could just as well be applied to the controlled experimentation and public witnessing staged in workshops. Furthermore, it is clear how Maya hieroglyphs have become laboratory objects, translated through technological simulations, and distanced from their original contexts of production through disjunctures of both space and time. As epigraphers have increasingly styled

their field as a branch of scientific linguistics – a field with its own imperial history (Errington 2007) – these practices of abstraction and detachment become even more pressing sites for ethnographic intervention. Perhaps a central, irreducible ethic for ethnographic studies of science in action should be to witness, question, and – if necessary – counteract practices of cutting or detachment with practices of care.

I have begun to hint at the many consequential practices that have made ancient Maya studies into the field that it is today, and I have explicitly chosen to avoid the field's complicated developments over the course of the past decade. I have also attended to a careful selection of the practices defining the rise of Mayanist epigraphy in tandem with the solidification of a coherent and unassailable notion of a single trans-historical Maya culture. This leaves open a wide range of problems that I have only addressed obliquely. As I authored this study I became interested in how institutions other than the University of Texas have strongly shaped recent trends in the constitution of a coherent, singular Maya culture. I think that a meritorious subsequent study could focus on the institutional role of Harvard University in the mutual constitution of the ancient and contemporary Maya. Particularly, I would find great value in a historical study that specifically locates the common institutional and cultural process shaping two Harvard projects: the Corpus of Maya Hieroglyphic Inscriptions, which is an attempt to create a comprehensive set of sources documenting monumental Maya inscriptions, and the Harvard Chiapas Project, which was the most extensive ethnographic study of a single community in the Americas (Rus 2004; Vogt 1994). The interplay between these projects' respective historical and contemporary analytical objects certainly helped to

establish the cultural singularity that Schele's work helped to consolidate and give public form.

Furthermore, I have left open the recent developments in epigraphy for critical and careful examination as they become less politically sensitive. In this vein, I have opened up the possibility for tracing close linkages between the institutional spaces in which knowledge of the ancient Maya has been crafted and the state- and community-level sites of cultural politics through which these objects and discourses circulate today. It would be compelling to examine how the workshop form has taken on a life of its own, as spin-off workshops have emerged in the United States, and cultural-linguistic activists have sought to institute their own workshops in Guatemalan communities.

Wherever this leaves us, I think that ethnographic research into Mayanist knowledge production, and historical knowledge production more broadly, should emerge as an ethical and epistemological necessity. Today's sites of knowledge production, the laboratories where experts innovate epistemic things, have become thoroughly enmeshed in the worlds that surround them. It is no longer possible to ignore the deeply public and consequential characters of sites where experts and non-experts together produce past, present, and future worlds. While the past worlds that we produce can lose epistemic legitimacy within a particularly scholarly field, they live on in new forms outside the academy. Past actors, the ancient Maya included, are inevitably a mishmash of evidential imperatives and creative imagination. It is our responsibility to live with the worlds that we have imagined, and to imagine these worlds differently. Above all, it is our responsibility to care for and care with the ancient Maya. Such acts of care must entail innovating new ways to engage with contemporary Maya

subjects of all types as well. In these acts of breaking bread inside and outside the anthill, companion spirits and companion species should all have their seat at the table.

APPENDIX A  
INTERVIEWS CONDUCTED

Cuma Chávez (Kawoq), Baldomero. Interview by author. Antigua, Guatemala, January 16, 2008.

England, Nora. Interview by author. Austin, Texas, March 24, 2008.

García Matzar, Lolmay Pedro. Interviews by author. Antigua Guatemala, January 10, 2008 and January 28, 2008

Keeler, Peter. Interview by author. Austin, Texas, July 10, 2007.

MacLeod, Barbara. Interview by author. Austin, Texas, July 23, 2007.

Manning-Schwartz, Lynda. Interview by author. Austin, Texas, March 18, 2008.

Morales Marquez, Moises. Interview by author. Palenque, Mexico, July 8, 2006.

Noble, Sandra. Interview by author. Crystal River, Florida, April 18, 2007.

Pérez, Juventino. Interview by author. Antigua, Guatemala, January 17, 2008.

Scanlon, Michael. Interview by author. Austin, Texas, July 12, 2007.

Schele, David. Interview by author. Austin, Texas, July 12, 2007.

Troiike, Nancy. Interview by author. Austin, Texas, July 11, 2007.

Wakefield, Walter. Interview by author. Austin, Texas, July 17, 2007.

Williams, Father Robert. Interview by author. Austin, Texas, July 14, 2007.

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## BIOGRAPHICAL SKETCH

Matthew Clay Watson grew up in the Wabash College community in Crawfordsville, Indiana. From 1999-2003 he attended Grinnell College, where he majored in anthropology and Spanish. Watson also engaged actively in the college community, playing soccer, serving in student government, participating in service trips to Arizona and Tijuana, and co-founding Grinnell's chapter of the American Civil Liberties Union.

At Grinnell, Watson began to develop an interest in further pursuing anthropological research and writing. He first participated in an archaeological excavation in Spring 2002, while he was studying abroad through the ACM Tropical Research Program in Costa Rica. His interest in the production of historical knowledge emerged through a subsequent historical ethnography tracing the intersections of kinship, enslavement, and class among his nineteenth century North Carolina ancestors.

These research experiences motivated Watson to pursue a graduate education in anthropology at the University of Florida. Before beginning his doctoral work, Watson received an M.A. in 2005 for his ethnographic research on representations of history in a small Salvadoran community. In addition to anthropological research, Watson enjoys competitive Scrabble and not-so-competitive juggling.