

FIELDWORK IS NOT WHAT IT USED TO BE

Learning Anthropology's Method
in a Time of Transition

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PHANTOM EPISTEMOLOGIES*Kristin Peterson*

In 2001, I attended the annual African Studies Association meetings a few months after returning from dissertation field research in Nigeria. I went to a panel where a scholar gave a talk about Nigerian capital flight and transnational corruption. I wanted a copy of the paper for a couple of reasons. One is that even though I had just completed research on AIDS, development aid, and the intellectual property law that governs HIV treatment access, the paper and certainly my own fieldwork experience stimulated my thinking about a troubling Orientalist notion that distinguishes between legitimate (transnational business and aid practices) and illegitimate (internal state corruption) capital flight. A second reason I wanted the paper was because the author had quantified the level of capital flight—upwards of several trillion dollars over the last ten years. She presented numbers that sounded firm and confident when all I had heard while in and out of Nigeria was only speculative, though it indeed involved incredible and varying sums of money. How much was stolen out of Nigeria was anyone's guess. I wanted to see what this certainty was all about.

The oral presentation critiqued the usual calculation of capital flight—calculations that ignore off-the-books or shadow economic dynamics. To put a value on capital flight, to explicitly calculate it, means to do so with information that can be accessed and abstracted. Such approaches cannot account for secret deals, hidden Geneva-based bank accounts, or the way fleeing capital comes back into the country to fund government privatization schemes or other lucrative deals. As such, the literature tends to fetishize calculable material objects and spaces such as the state treasury, which is described by Nigerians and outsiders

(and I don't dispute this) as being looted by state officials. The implication is that the "native" state alone possesses agency or that the state is a monolithic entity without internal contests over the fiscal management of any given political geography (Emaigwali 2001; Krugman 1998). The discourse here is about indigenous greed and not the many different national and international monetary and legal policies or European and North American banks and actors at work. The sole focus on indigenous activities contributes to a phenomenon that easily misses a form of state and transnational consumption held over from the days of indirect rule. What sort of analysis would be possible if the murky realms of capital movement were just as significant as the visible ones?

One of the objectives of the conference that launched this volume was to understand how apprentice ethnography largely encounters the "already known" while developing and crafting ethnographic training. Yet my feeling is that the "already known" has been overstated—data, results, and surprises are buttressed by pre-existing expectations, which empty the prospect of something not yet in view. To get at something else lying within our own methodological frameworks, I propose the idea of "phantom epistemologies." Specifically, I refer to empirical elusiveness, unspoken common sense, a politics of (in)commensurability, and how the presence of any "ghost" becomes viewable to those who believe—these are the ethnographic entryways into what is knowable. Yet what is knowable will often lie in the realm of uncertainty; that is, the very thing of empiricism cannot be absolutely defined in the presence of phantoms and unknowable possibility. My thinking is partly informed by "shadows" theorized by Carolyn Nordstrom and James Ferguson. Both refer to "shadow" political economies that are liminal, hidden, or simply unaccounted for. James Ferguson writes:

A shadow is not only a dim or empty likeness. It also implies a bond and a relationship. A shadow, after all, is not a copy by an attached twin—a shadow is what sticks with you. Likeness here implies not only resemblance but also a connection, a proximity, an equivalent, even an identity. A shadow, in this sense, is not simply a negative space, a space of absence; it is a likeness, an inseparable other-who-is-also-oneself to whom one is bound. (2006, 17)

The desire here is directed toward recognition, not one marked by an empiricism that provides a finiteness or certainty of something, but a recognition of some hidden essence, implication, connection—one that sits, for a moment, underneath or beside our apprehension. While the shadow waits to become known in some concrete form, a phantom epistemology does not count on revelation. Rather than dismissing our quandaries of "not having all the data" that may not be gotten, a phantom can inhabit the data in ways that do not

always desire the fullest of answers—one that allows the unknowable to remain as powerful an analytical figure as the known. That is, the phantom—the stuff of familiarity, yet also the stuff of the unknowable—is the ethnographic object of inquiry, rather than being some shadow whose materializing requires further patience and digging. Finding one's way through (or even detecting at all) the presence of such ghosts is difficult, as the latter occur at different levels of scale, not only in fieldwork but at the level of interpretive circulation. Indeed, both capital flight and the conference paper I was seeking were "phantom indexes" of an AIDS epidemic in which capital and drugs were made scarce via political-economic processes that generated a common sense about political leadership, development aid, and elusive practices of governance.

After we both returned home from the meetings, I e-mailed the presenter asking for her paper. She responded by stating that I should get the original unpublished paper from which she derived her own talk—but she needed to get permission. Once that was done, I was told that I could not use the author's real name or institutional affiliation. The author of the original paper claimed that due to the sensitive nature of the material many powerful people would be exposed, which would cause trouble for him and his place of work. I understood, agreed to these terms completely, and anxiously read the paper.

The truly remarkable aspect of this unpublished piece was that it recounted the same stories I had heard on the street from people selling fruit, from taxi and bus drivers, from the home of one of Nigeria's wealthiest men, from Nigeria's most senior UN officials and, indeed, in articles about post-military dictatorship Nigeria. These are stories such as: a governor of one of the oil producing delta states fears he could lose his bid for reelection and calls in a trusted ally, one of the transnational oil companies, for help. As soon as the polls close, the governor's men are in place and clear the way for the oil company's helicopter to land. The ballots are stolen, the helicopter flies them off to the governor's mansion, and later that evening he is reelected. Or this one: a newly installed military general travels to Geneva, meets with a top bank official, and inside his office pulls out a gun and demands, "Give me the names of all of those from the previous regime with secret bank accounts?" The bank official drops to his knees and declares that he would rather die right there than trade away the identity of his clients. The gun-toting man then pulls out a suitcase filled with money in large denominations, puts his gun away, chuckles to himself, and says, "You can have my account and look after my money?"

The point of the paper was to argue that the sociopolitical truths about corruption are often anecdotal data; and, too often, the very definitions of corruption do not take into account its reciprocal role, one that begins to widen the predominant understandings of capital flight. That is, "what happens" in Geneva

or London manages to circulate back to the Nigerian city and village street in a way that indicates something far more than the stereotypical idea that Nigeria is the most corrupt place on earth. Rather, Nigeria is an open society in terms of public political debates combined with long-term windfall oil profits where the state remains the primary form of accumulation. Such a scenario allows a view inside a more secretive and perhaps predominant form of capital mobility and accumulation that represents a huge proportion of the world's wealth; this mobility is rarely accounted for, even though authors such as Graham Hancock (1994), Carolyn Nordstrom (2004), and William Reno (1999) have pointed us in these directions.

The empirical is difficult to tag here because realities and elusiveness exist in the same space. The reality is that capital flees Nigeria and international banks provide secretive and safe harbor, that state officials are investigated for corruption, that wealthy elites perform grandiose displays of their own material worlds in public places—in the view and earshot of those who have not eaten for days. Indeed, Daniel Smith (2006) has eloquently pointed out that the prevailing ideas and discourses of corruption are launched via inequalities on national, community, and individual levels. Hunger and the thin line between public/private performance together facilitate the circulation and scale of the stories themselves, and so do development workers and academics. Their elusiveness consists in their being stories resonating at the level of common sense, circulating in the most powerful ways, that have literally defined a sense of national public discourse and what it means to be Nigerian. Historian Luise White argues that the stories of what rumor need to be taken at “face value, as everyday descriptions or ordinary occurrences” (2000, 5). In referring to accounts of bloodsucking during the colonial period in Africa she writes, “the inaccuracies of these stories make them exceptionally reliable historical sources as well: they offer historians a way to see the world the way the storytellers did, as a world of vulnerability and unreasonable relationships” (5). Stories of capital flight are equally instructive because they provide a space for ordinary people to narrate inequality and how they engage the state (see Gupta 1995), and, indeed, provide profound insight into the dynamics of both militarism and, more recently, civilian rule, often referred to as “democracy.” These “knowables” emerge in secondhand stories that reflect a reality that truly cannot be unreal. Yet, the contours of “reality” are also not always traceable.

These phenomena are distinct from partial or situated knowledge, which recognizes that the possibilities of knowing never lie within an imagined totality; that knowing can be achieved only in parts—and apprehending such parts is shaped by positionality and subjectivity. Douglas Holmes and George Marcus (2005) employ the term “para-ethnography,” with which they identify the ethnographic knowledge of central bankers such as Alan Greenspan—a knowledge that, more

often than not, emerges in the form of anecdote. Anecdote, as a crucial form of data, is gathered by such experts in order to produce reports on the status of the national American economy, but it disappears as acknowledged data in the reports themselves. Para-ethnography functions at the level of both method and knowledge and, like phantom epistemologies, must determine and enter into the shadows of knowledge and systems. Phantom epistemologies do not seek out parts in order to fit into a whole that is imagined to exist out there somewhere. Rumor, anecdote, stories, evasiveness, and not being able to ever know are their own sets of data and knowledge. They point us not in the direction of desired concreteness, as in “facts,” but rather offer an analytical opening to something just as fascinating and analytically provocative as a traditional sense of the empirical. That is, the phantom asks us to rethink the very essence and contours of the “empirical” itself.

When the rumor on the street in Lagos, for example, is that forty percent of all Nigerian crude oil “goes missing” every year, it is not a matter of believing or not believing the numbers. Many of these numbers simply do not exist as something that has already been calculated; or if they do, the numbers are completely different among official and unofficial quarters for any given community, institution, or individual who has something at stake in them. For example, Benue state, located in the central part of the country, reported the highest percentage of HIV infection in 1999 at eighteen percent. The numbers did not garner any additional funding or declarations for special emergency relief. Rather, they created the stigmatization of an entire state. Two years later, Benue reported that its infection rates were below ten percent without providing an explanation of, or hypothesis for, the supposed decline.

Another example: Alhaji Umaru Dikko was a Nigerian minister and had the sole power to grant import permits. Wealthy expatriates took suitcases of money to his back door in order to keep their businesses going. Some say he collected over US\$5 billion. Once Dikko's Nigerian colleagues heard about the amount they were not getting, they set out forcibly to take their share (the trickle down of payments inside any business/government agency is expected and a standard procedure). After escaping across the Nigerian border (the anecdote: by tossing wads of cash out both windows as he approached armed border guards, distracting them and zipping on past into the Republic of Benin), he was eventually found and kidnapped in London (the anecdote: while an Israeli aesthetist kept him unconscious—suspicious customs officials detected and found them inside a crate; it was suspected the Nigerian High Commission ordered the kidnapping). Nigeria requested his extradition on the grounds that he had stolen over US\$5 billion. Publicly the United Kingdom invited Nigeria to submit the necessary paperwork to initiate extradition proceedings. But anecdotal knowledge and

rumor claimed that if extradition procedures were commenced on the grounds that he stole money, the U.K. threatened to release the names of all those Nigerians holding more than US\$5 billion in British banks. Dikko was never extradited and remains in London.¹

While drawing on Foucault's concept of epistemes and Thomas Kuhn's notions of limits and paradigms, Nancy Schepher-Hughes has provocatively argued that the structure and norms of research are situated within naturalized paradigms of positivist research practices (Foucault 1973; Kuhn 1962; Schepher-Hughes 1997). She has called for a "demography without numbers" that would render both the social and the biological powerful analytical indicators and constraints to demonstrate what gets missed or passed over in conventional quantitative research. Yet not even numbers can always constitute the "factual." Numbers can work the same way that stories and anecdotes do—capital flight and HIV infection rates calculated with only cursory sets of numbers do not take into account the shifting alliances that produce numerical data in the first place; and here it is productive to think about the relationship between secrets and official numbers or official numbers as secrets. Secrets are crucial data points because they must operate at public and private levels. As the Dikko story above shows, the implications inside the anecdotal knowledge are that capital flight, generated by corruption, is not condemned but highly encouraged. The secret is juxtaposed against an idea publicly articulated by banks and government that corruption is a no-no. Here, the anecdote reveals a productive tension between the allowable and the condemned. The tension functions as yet another shadow that masks a rarely accounted-for capital mobility. Therefore, numbers can often only remain inside the common sense of a national consciousness that seeks to explain the conditions of its own existence. This was the crux of the ethnographic challenge for me: not discerning what was newly knowable inside the givens, but figuring out how possible it was to discern the givens themselves.

Revisiting Multi-sited Ethnography for a Moment

Following elusive "facts" and the traveling fantasia of stories is one of the modes of operation that multi-sited ethnography proposed as its initial experiment.

1. While several scholars (such as White 2000; Geschiere 1997; Ashforth 2000) have demonstrated the power of rumor intersecting with postcolonial realities in Africa, others have analyzed such phenomena as deeply ambivalent relationships to capitalism and inequality (Smith 2001; Comaroff and Comaroff 1993; West 1997; Sanders 1999). Most of the literature analyzes such phenomena via witchcraft, the occult, and vampires. But the public secret and the antedecies of capital flight have their own sensibilities of rumor and anecdote, even if the accumulation of wealth via capital flight is generated by occult activities.

One of the great misunderstandings that I have encountered about multi-sited ethnography is that it is often viewed as a methodological approach that is purely spatial or simply a matter of scaling. That is, multiple physical sites of research tend automatically to count as a thorough rendering of any given object of analysis, which ultimately collapses the "comparative" with the "multi-sited." This collapse misses the point of the *Writing Culture* debates in the 1980s and 1990s, which in large measure were responding to both 1970s critiques that linked anthropology to imperialism and the poststructuralist literature that posed questions about the production of knowledge—what George Marcus and Michael Fischer referred to as the "crisis of representation" (Marcus and Fischer 1986). The response to these critiques (see also Clifford and Marcus 1986; Gupta and Ferguson 1997b) and, subsequently, the schematization of the methodology of multi-sitedness (Marcus 1995) remained inside these questions of epistemology. The bare-bones Malinowskian paradigm of ethnographic research did not go away here. But new changes included how the question of scale, the production of knowledge, and textual strategies especially determined how we understand "culture" (and see Faubion in this volume). While questions of new objects of study have emerged in this context, I am more interested in how the "experimental moment" actually shifted established ideas over what counts as data, method, and knowledge. That is, I am interested in the shift from the spatial as a limiting factor of research design and procedure to spheres of knowledge in which new analytical and epistemological domains have arisen as primary questions of the anthropological project. Given that phantom epistemologies operate at different levels of the real and the fantastical, the question then becomes: What constitutes any given consonance between objects of study and sites of study? How do stories of meetings between Geneva bankers and Nigerian generals suddenly become a point of data even though they remain "empirically" in the shadows?

In the opening of *Fricition*, Anna Tsing describes how her project on environmental social movements became possible when she "stumbled on a curious misunderstanding" (2005, x). She recounts that because she arrived in the aftermath of an original campaign against logging, she had to rely upon tellings of the story of the campaign itself. To her informants' and her own surprise, they all seemed to describe incongruent events, finding each other's descriptions and experiences unreal and completely incommensurable. In thinking about how to handle such an experience methodologically, she states:

How does one do an ethnography of global connections? Because ethnography was originally designed for small communities, this question has puzzled social scientists for some time. My answer has been to focus on zones of awkward engagement, where words mean something different across a divide even as people agree to speak. These zones of

cultural friction are transient; they arise out of encounters and interactions. They reappear in new places with changing events. The only ways I can think of to study them are patchwork and haphazard. The result of such research may not be a classical ethnography, but it can be deeply ethnographic in the sense of drawing from the learning experiences of the ethnographer. (Tsing 2005, xi)

The interconnection here is one that may be a matter of scale where the process of scaling is methodologically always presenting itself—the haphazard and zones of awkward engagement. Like most research, mine poses questions that are already situated within distinct frames of analysis, even when my examples may stand in stark contrast to the norm of theorization. Yet I am interested in locating those people, movements, objects, and the things germane to understanding the system itself that are constructed as the most deeply at stake, as being the most generative—yet quite invisible—dynamics of political economies. But narrowing down who and what are in play—that is, accounting for a range of actors and institutions whose actions are consequential for the larger system of things at multiple scales—makes the question of consonance between objects and sites of study perhaps one of the most important in ethnographic fieldwork. Marcus has addressed the crisis of representation by suggesting that multi-sited ethnography treat methods as firmly embedded in the research conceptualization and design itself (1995). Similarly, the notion of consonance becomes a matter of both critique and method where neither can be pulled into separate parts.

Tsing suggests that consonance is haphazard, that zones of awkward engagement rise up out of fractured ethnographic forms, where consonance puts questions of method itself at stake. I would suggest that consonance and scaling are still carefully articulated methods even if one trips over them in a moment of surprise, leading data collection down an altogether unexpected path. In *Advocacy After Bhopal*, Kim Fortun first confronts the problem of how to demarcate the site of research that tracks and understands a disaster such as the Union Carbide gas leak in Bhopal, India (Fortun 2001). She points out that the Bhopal disaster lingers nonspatially as the dire health effects of the gas leak continue in time. One of the keys in determining the conceptual and physical scaling of such a project lies in Fortun's shifting of discursive contexts and frameworks inside her research questions—a strategy that begins to open up an entire range of new questions and insights that are “givens” already firmly embedded in her existing frames of analysis. For example, in making sense of advocacy, Fortun decides to use the term “enunciatory communities” instead of the development and policy term “stakeholder.” For Fortun “stakeholder” is static; it assumes consensus, agreement, and an existence prior to the disaster itself, whereas enunciatory

communities show shifting articulations, contradictions within “groups”; they cannot exist prior to the double binds in which they live (see her essay in this volume). While she holds to the stakeholder model—there is something at stake for different communities—the shift to enunciatory communities allows her to see new subject formations take place while existing ones undergo their own shifts and changes. At the same time, it allows for an accounting of different and contradictory epistemologies, discourses, and forms of advocacy. The very physical and linguistic encounter with Bhopal brings legal issues and legal language into everyday living; notions of “health, fairness, and progress” are radically redrawn, giving rise to new and shifting social formations. It is these articulations across time and borders, which move in and out of Bhopal and in and out of India, that demand understanding of how a “shifting world order” emerges out of a single disastrous moment. As such, through ethnographic, conceptual, and textual strategies, Fortun shows how the Bhopal disaster articulates India's own globalization policies and the various contradictions (of citizenship and national identity in particular) that arise as a result.

I use these examples of (in)congruence and scale to make particular claims about phantom epistemologies and the ethnographic practices used to apprehend them. The key moments in both Tsing's and Fortun's work hang on a particular explosive event: a spatially bound logged forest intersected by competing epistemologies and disparate apprehensions; a single disastrous gaseous moment that spirals out across time and space, hailing new subjects and subjectivities into both necessary and unpleasant consonance. My own research shows a different problem of scale that does not emanate out of an experience that demands the immediate attention of everyone and everything in its path. Both Fortun and Tsing mark the trace of past and current presences. But what kind of attention is needed when such dramas do not exist or mark the very thing we wish to understand?

Revitalizing Failure: Tracing “Not Knowing”

I first went to Nigeria and Cameroon in January 2000 to conduct preliminary dissertation research, which ultimately led to my first career-defining failure. I set out to understand new transnational institutional assemblages between several U.S. and West African universities, militaries, NGOs, and government institutions, all of which were bioprospecting forests across the Nigeria/Cameroonian border—endeavors that aimed to find new therapies for infectious diseases. I was particularly fascinated by the problem of how trade-related intellectual property law would play out across nation-state and institutional “partnerships” in the

wake of a recently formed World Trade Organization. I was working with one of the NGOs, which was directing these activities, and my research project fell apart after about six months. Part of the failure had to do with internal politics, but a greater part was due to implicit expectations that I would be writing a journalistic account of the various bioprospecting programs. In fact, toward the end, one of the project leaders requested that my dissertation be a narrative and not an analysis of the program. Complying with this request of course was not possible. So, after three years of graduate school, two grants, and no project, I had to rethink what I was doing.

My time as a graduate student in the anthropology department at Rice University was instrumental in the action I took. Much of our training as students in and out of the classroom stressed a rigorous thinking of the “conceptual writ large” framed squarely inside a unique pedagogy—one that did not micromanage our thinking or doing, but one that constantly opened up space to reassess and rethink our objects of inquiry. In the classroom and before the field experience, this meant, for example, writing and rewriting many times a “research imaginary” as first-year graduate students. I remember after I gave George Marcus the fifth draft of such a piece in the pro-seminar, he handed it back to me and said, “Good, now do it again, and this time don’t use the word ‘globalization’”—an exercise that produced new realms of thinking over the question at hand. Moreover, my long and extensive e-mails to Jim Faubion and George Marcus from the field documenting the slow and painful demise of the first project were circulated to the first-year students in the pro-seminar as an example of the generativity of failure. At first, I was mortified at the thought. But when students told me what they got out of the mailings, I realized that they treated them as objects of analysis—that the collapse of a project was not something to be feared, but rather that any outcome of our initial research efforts has meaning. Moreover, it helped me rethink the politics, social dynamics, and materialization of the very assemblages I was studying.

The graduate students also had their own offices on the same floor; late nights included trips into one another’s offices where chalkboards were rigorously used to get feedback on one another’s thinking. It also meant weekly reading and writing groups conducted in cafés that gladly gave us the remnants of chocolate cake at closing time. Students plugged in and out of such activities as they pleased, but I do not think that these endeavors we initiated on our own would even have been imaginable without the pedagogical style that framed and inculcated us as researchers.

There were a number of issues at play at the heart of this practice. The primary one was decentering power relations within the department itself, certainly not at the level of administration but rather at the level of pedagogy. This had

its ups and downs. On the one hand, students looking to get training in a particular area or subfield were often on their own, meaning that the training was not about instructing students on what to read in order to prepare for the field and, indeed, it never included this sort of management at all. On the other hand, a space was created in order always to engage in dialogue about the research at hand. This did not often come in the form of committee meetings (I actually only had one such meeting, and it was my dissertation defense), but rather in the spirit of generous open-door policies, meetings in cafés, hallway chit-chat, and dinners. Even after a few cocktails, one always came away with something new to think about, to tweak, to open up, to explore. Another issue included the fact that all students were funded equally upon entering the program. While I have no evidence for this sociological observation, my feeling (via conversations with colleagues at other universities) indicates that when there is less competition for funding, more cooperative and theoretically engaged interactions take place among students.

With this training as a backdrop, I was torn between two interesting problems I wanted to explore: one was the legal and policy conundrums brought forth by bioprospecting and the other was the question of Nigeria itself. The former took me on a detour to Quito, Ecuador, in the hopes of scouting out a project to be conducted within legal organizations that had begun to construct newly conceptual and theoretical frameworks over the intersection of intellectual property and bioprospecting in fascinating ways. But I was pulled quickly back to Nigeria, not only because I had covered a great deal of ground on my initial two-month visit, but perhaps because I was trained to be curious about things not traditionally located in area (African) studies as well as anthropology. Before I first arrived in 2000, my attention was drawn more to conceptualizing assemblages with less of an emphasis on theorizing the place that made such linkages possible. That soon changed.

Although I was following Nigerian politics closely, not least because the country had recently transitioned from military to civilian rule, I was not prepared for what I would encounter on the streets of Lagos. I was fascinated by the hope and hype of “democracy” which in everyday discourse was interchangeable with “civilian rule.” But democracy was the freedom to articulate sufferings; how “nothing works,” the inability to eat and procure medical care; “how things are getting worse.” Democracy was also about social redemption. In fact, both the public and the government have used the term “democracy dividends”—the hopeful means to alleviate suffering and to retroactively cash in on an imagined social contract—from the mid-1960s to this moment.

Unlike suffering, democracy in Nigeria was elusive, intangible, idealized; yet both suffering and democracy immediately depended upon and referred back to each other in every space where they were discussed. These discourses piqued my

curiosity and although I did not know what any of this would mean in the larger scheme of things, it felt right to follow; I decided to stay.

I switched my project topic to HIV drugs and AIDS activism and policy-making. As the new civilian government took power, development and other international organizations (which left the country after the Abacha government executed Ken Saro Wiwa and the Ogoni 8) returned. Along with the government, these actors rediscovered AIDS and began to redirect their programming in this direction. Social movement activism against dictatorship quickly gave way to the formation of NGOs, which established business relationships with their donors and “partnerships” with the state and multinational corporations. In this snippet of time, the discourse of democracy was not simply something that was fascinating or to be witnessed at a moment of vast political transition. It turned out to be the glue, the thing that harnessed these new assemblages and structures to one another. What did democracy dividends mean in the context of an AIDS activism that was demanding access to treatment based upon one’s HIV status? What did it mean in the context of a restructuring state, one that was privatizing, signing numerous trade agreements, while promising more security to its citizenry?

To trace the contradictions rising out of these scenarios, I decided to return to my questions on intellectual property and trade by doing an ethnographic analysis of Nigeria coming into compliance with the World Trade Organization. In my fondness for scale and complexity, I encountered my first slough of phantoms. I specifically focused on the Trade Related Intellectual Property (TRIPs) Agreement, which governs the global circulation of pharmaceuticals. The country signed the agreement under Abacha, during one of the worst military dictatorships; now it had to negotiate the implications of its implementation as a civilian actor.

For Nigeria, as with many other TRIPs signatories, compliance requires an overhaul of the country’s intellectual property (IP) law. The struggle over the actual anatomy of a new IP law was a largely subdued and unnoticed affair, especially in 1999–2004, when actual debates and conferences on the issue began to take shape. The task at hand was to use the space of TRIPs debates, housed in conferences and workshops, taking place among selected, invited policymakers (and others) located within specific networks. Among these actors, some of the most invested interests and views of what I assumed to be at stake were rarely articulated in public, but rather were whispered in corridors and behind closed office doors—a more secretive para-ethnography and one that did not inform the outcome of legal proceedings and documents in the way Greenspan’s methods and practices did.

It was clear that an ethnography of compliance had to happen at several levels. Getting at the real and unreal would turn out to be a daunting task, as even

those working in the highest or any other level of government did not know how events were transpiring beyond their immediate environments. Moreover, with few exceptions (many of whom I interviewed), neither government elites, invested in particular avenues of compliance, nor the public, including AIDS advocates, had much awareness of the global debates on trade-related intellectual property. There are several reasons for this. A chief one concerns the direction of activism itself. International development organizations constructed the contours of activism (via funding mechanisms) that fit into the immediate needs of newly configured AIDS activists. These included addressing traumatic stigma and discrimination, initial encounters of which were in hospitals; creating support groups for HIV-positive people; and finding ways to access treatment—at the time, there was virtually no antiretroviral medication in the country. Even with the burgeoning hundreds of workshops conducted on these topics, however, no major donor ever set out to provide education on the implications of treatment access in the context of various TRIPs compliance scenarios (with the exception of United Nations Development Program, which worked with government policymakers in several African countries, but not Nigeria).

In terms of business, the dialogue on intellectual property enforcement mechanisms was largely restricted to the in-country circulation of goods. Only Nigerian lawyers representing the interests of multinational companies articulated interest in developing intellectual-property protection mechanisms for such actors. Moreover, all government and private actors I interviewed claimed that there was virtually no communication between Nigerian negotiators at the WTO and the relevant ministries. No dialogue ever took place among private business actors on the shape or context of TRIPs compliance.

Thus, the Nigerian public and private sectors did not have an analysis of their own national context. So the objectives of my project began elsewhere, with the need to ask how to conduct an ethnography of “not knowing,” that is, to understand how phantom epistemologies existed in layers at multiple levels. “Not knowing” about a fundamental shift in the future trajectory of HIV treatment and its landscapes exists in a field of affects within which outcomes are partially known and maneuvered in a narrow field of vision. This observation, of course, was informed by my own sense of what was important, derived from what I knew from international debates. When I began to listen more to what was important to Nigerians, which was highly contested, questions of property and compliance began to make more sense to me, as did my larger questions of “not knowing.”

Another aspect of my own “not knowing” developed into a curious para-ethnographic intersubjectivity: tracking down the latest IP draft or bill proved to be a difficult endeavor. There was always more than one draft floating around different ministries or the U.S. government (which was highly involved in the

rewriting of Nigerian law) in divided parts—no one could ever confirm which was valid or authentic, and I was constantly chasing phantom drafts.

The global battles over TRIPs have made international headlines over the years and have incorporated what were once little-known concepts in patent law into social movement slogans; it is not my intention to elaborate on these events. I do, however, want to draw attention to the fact that a great majority of the literature on the subject constructs a dichotomy between two main players caught in a struggle of power—wealthy pharmaceutical corporations versus poor people worldwide who face few treatment options. This picture is not entirely accurate, which became more than clear once an ethnographic approach to compliance was well under way. There are numerous national and international actors and institutions enmeshed in cooperation and competition over far more complicated discourses and practices that lie outside of this dichotomy, something that global AIDS and anti-globalization activism often misses. Moreover, the TRIPs Agreement in the past has been constructed as one of the fundamental obstacles in generating and distributing generic drugs. This is not accurate either, because the Agreement does make clear provisions for the generic drug market at least to exist, if not flourish. TRIPs patenting rules actually create a blueprint for technologies of negotiation and subjectivation. The rules themselves are not necessarily at stake, but the anatomies of negotiation feed into already existing histories of inequalities. Inequality here should not be thought of as strictly a divide between have and have-not nation-states. Rather, inequality can be detected in commonplace statements such as that made by a lawyer I interviewed, who commented on TRIPs compliance: “If we don’t comply, there could be trade sanctions. To that extent, we will benefit, if we comply.” The lens of a subjectification that focuses the conferral of benefits is precisely the lens through which the production of knowledge can shape decisions about TRIPs compliance and ultimately the future trajectories of the drug market. It may even shed light on the more complex characteristics of power and sovereignty.

By the time TRIPs arrived—after dictatorship and severe economic decline—all that was known, and articulated to me, was this: telephones do not work in the ministries; computers and Internet access are nonexistent in government and policymaking offices, making it impossible to access the politics of trade and intellectual property or even IP databases; the patent office has no compiled databases—if you have filled out the two-page form properly, you get your patent, a practice hardly competitive with forces in Europe and the United States; Nigeria is on the “wrong side” of digital technology; billions of dollars can be made in the commercialization of folklore, music, and genetic resources, all of which slips out of Nigeria and Africa’s grasp; globalization, for some, compressed space and time; something needed to compete in the global patent race,

but in Nigeria, time and space are painfully elaborated, making the “benefits of globalization”—a common phrase among policymakers—difficult to reap; and TRIPs compliance was the answer to bringing Nigeria into its “rightful place in the world.” Like privatization, multilateral trade-related intellectual property—for those who organized conferences and agendas—was the magic bullet, and it was this imaginary of the bullet that left no time or space for “not knowing.” By analyzing Nigeria’s ongoing steps toward TRIPs/WTO compliance, my purpose here has been to think about a post-practice where recent political economic artifacts inform and motivate strategies toward manifesting what “appears” to be known.

My experience of not being able to count on the “factual” as an avenue of entry into ethnographic fieldwork as well as a site of analysis has provided a methodological exploration of what is possible within ethnography. The phantom epistemologies that my research inhabits have led me to understand how policy, capital, and development function at different levels of scale, but more important, how they show up and reveal other nonintuitive linkages. Pulling together aggregate knowledge such as that of economy, dictatorship, intellectual-property policies, and drug politics in the context of macro-politics and economics is not a process that is always or necessarily shaped by a factual archive—which should make us question in turn the very reasoning and ontology of empiricism. Elusiveness, common sense, and phantom epistemologies are the things that provide a different kind of gateway into and outside of the givens in our analytical frames.