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Dedicated to Aunt B.

Even though she would probably not have approved.

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Chapter One

Introduction

Introduction

> "It … would be unjust to consider Latin American archaeology as a passive reflection of foreign, essentially North American, influences. Local archaeologists have developed original methods and generated their own models and conceptual frameworks. Certainly archaeological practices have adopted theoretical questions and methods from foreign intellectual traditions. This is simply because, as with any research in the Western world, Latin American archaeologists are engaged as part of open scientific communities, exposed to intellectual movements generated in other countries." (Politis 2003: 116)

What does it means to see oneself as working within 'open scientific communities'? To be open implies ease of movement; that ideas, facts, and people can circulate without obstacle. For this to be possible it also implies that the things that are circulating freely are equally understandable everywhere they go: that ideas, facts, and people are in some way comprehensible and comparable, able to land in new places with the same meanings attached. But also that one does not close oneself off, nor restrict access to others: that everyone is willing to 'expose' themselves.

Politis' review of Latin American Archaeology, published in 2003 in a special edition of *American Antiquity* devoted to 'Mapping the Terrain of Americanist Archaeology', ended with a criticism of North American archaeologists who work in Central and South America but are not open to the ideas, people, and facts they find there. He argued that although Latin American archaeologists are influenced by the work of archaeologists from Europe and North America, the reverse rarely happens. Moreover, although the *data* from excavations carried out by Latin American archaeologists might be taken up by foreign colleagues, the larger intellectual and methodological contexts out of which data are derived are largely ignored. Europeans and North Americans working in the South are “willing to scavenge data but ignore engaging with Latin American researchers in equal scientific debate.” (Politis 2003: 130-1)

This critique is an accusation that a bargain has not been kept. Latin American archaeologists have faithfully exposed themselves to the work of their colleagues in the North, but those colleagues have not been willing to reciprocate. This is an accusation of ‘closedness’ that, in one sense, upends a more familiar complaint. To a certain extent the article itself is a rebuttal of a more commonly heard accusation: that Latin American archaeology has ignored the major paradigm shifts of processualism and postprocessualism that occurred in the discipline during the latter half of the twentieth century because it is closed off from the rest of the world.

The circulation of scientific knowledge is a subject that the social studies of science have considered for some time. The continued legacy of centuries of European colonialism and the resulting power disparity between the Global North and South is, arguably, a concern that lies at the heart of anthropology. Looking at the practice of archaeology as a science that moves between and brings together the North and South, allows us to consider the intersection of these two concerns. In this dissertation I consider the unequal movement of archaeological ideas and data between the North and South, and address the question that Politis leaves unanswered: namely, why this is the case and what broader effect it has, beyond the discipline itself.

This project explores the globalization of science, asking how scientific research is practiced and valued differently in the Global North and the Global South. Specifically, I seek to understand the nature of transnational collaborations between archaeologists from the US, Chile, and Bolivia who work together on excavation sites in the Bolivian Andes and Chilean Tarapacá desert and the nature of the knowledge they produce. Echoing Anderson’s (1987) concept of the national imagined community, I will argue that the circulation of mostly English-language academic journals and books creates an 'imagined supranational community' out of scholars who all believe themselves to be reading the same texts in the same ways despite their cultural and geographic dispersion. However, I will show that the situation on the ground is quite different: archaeology does not have a single, shared community of practice that can unproblematically transcend national borders (Tsing 2000, Gal 2003). Instead of a single international epistemic community, whose members are held together despite their physical distance by shared participation in massive experiments, electronic communications, shared publications, or the migration patterns of individuals (e.g., Knorr Cetina 2007 and forthcoming), there are multiple epistemic communities located in different countries, whose practices and institutions are structured according to very specific historical and contemporary contexts. On the one hand I will be making an argument that 'archaeology' as a field science relies on the embodied expertise of individuals, rather than on technology, to bring-into-being the material relationships that are its epistemic objects—and in this case I will be referring to the discipline as a whole in comparison to other scientific disciplines. On the other hand, I will simultaneously be demonstrating that there are multiple, nationally structured epistemic cultures of archaeology, whose sociality, means of reproducing themselves, understandings of labor and expertise, and political investments are not universal, even as they are structured in the similar ways. Moving between two scales of comparison—within archaeology, and between archaeology and other sciences—will enable me to highlight what is unique, puzzling, or challenging about archaeology's means of creating and warranting expert knowledge; particularly as it contributes to debates concerning the nature of embodied scientific expertise and the practice of science in post- and neocolonial contexts.

In essence, my argument in this dissertation is that to practice archaeology in either

Bolivia, Chile, or the US is to be involved in a specific social and intellectual project—and these projects are not necessarily commensurable. While this is likely to also be the case for other sciences, in archaeology this becomes a particularly pertinent problem because archaeology has appeared to lack many of the features classically associated with 'science'—replicable experiments that allow multiple witness to attest to the security of matters-of-fact; laboratories that enhance, purify, or model nature; complex, black-boxable technology that can standardize, mediate, or purify vision (see, e.g. Latour and Woolgar 1979, Shapin and Schaffer 1985, Daston and Galison 2007). Over the course of the discipline's history this has led to cyclical debates among archaeologists about the security of knowledge claims, accompanied by calls to model the experimental and natural sciences as a means of reaching epistemic security through mechanical-objectivity (c.f. Daston and Galison 2007). The most influential of these movements occurred during the late 1960s and 1970s in North America and the UK, and became known as New Archaeology or Processualism.

In this dissertation I demonstrate that, indeed, archaeology lacks many of the means of warranting knowledge that are found in other sciences and particularly in laboratory or experimental science. However, I take this as a starting point to illustrate how, despite this, authoritative knowledge is created and warranted through practice in and out of the field. Archaeology's objects of knowledge are produced during excavations through the embodied expertise of individuals—not through complex technology or machines that can be standardized. Groups of people use their eyes, hands, backs, and memories to turn material objects encountered in field sites into written records and databases. In the process, the way these people are organized and structured on an excavation turns them into archaeology's knowledge producing machinery. It is people who become the 'inscriptive device' (Latour and Woolgar 1986 [1979]:

51) through which knowledge is produced. A feature of inscriptive devices, however, is that their inner workings must be bracketed out—blackboxed—if the knowledge produced is to appear inherent and 'natural' (ibid). But unlike, for instance, high energy physicists (Porter 1995: 222) , this reliance on tacit knowledge in archaeology is a source of epistemic anxiety.

Partly as a result of this anxiety, both national differences in epistemic culture within the same discipline and the reliance on non-standardizable bodies are elided by epistemic and political commitments to meritocracy and universalism—in other words, the belief that facts are, or should be, value-free and bare no trace of the social processes of their production. They *ought* to be independent. On the one hand this leads to an under-appreciation of the means through which archaeological knowledge is, in fact, produced. But it also has implications when archaeologists from different countries work together. As I will demonstrate, the mismatch between the desire for and expectation of standardizable universalism, and the reality of archaeology as a far more contingent and variable practice, causes otherwise well-meaning transnational collaborations to fail. This is particularly the case, I will argue, when archaeologists from the US work in South America. *Not* accounting for cultural difference when assessing scientific practice or knowledge—in essence, being 'culture-blind'—is held as a positive value by US scientists. As feminist standpoint theorists have argued in relation to the gendering of objectivity, however, the ability to successfully claim an objective 'view from nowhere' derives from already inhabiting a position of power (Harding 2008, Gal 1995: 418). Collaborations with archaeologists from the politically and economically dominant US thus already involve a power imbalance that cannot be entirely bracketed out.

In exploring this power differential I will develop a concept that I term *supranationalism*: an expectation that scientists from the North (and particularly the US) are able to transcend their national and cultural contexts and speak to a decontextualized, global, supranational public sphere, more readily and naturally than scientists from the South—that their knowledge claims are thus more 'objective' and able to travel in the ways Politis questions above. Through the example of collaborations between North American archaeologists who travel to Chile and Bolivia to conduct excavations projects, I will argue that otherwise well meaning collaborations can sour because of the conscious or implicit assumption that 'local' archaeologists will always potentially be too 'nationalist'—a position that is framed in opposition to the inherently more objective, disinterested, or 'global' outsiders. While it is possible that this attitude exists in other sciences (Harding 2008, Shepherd 2005, Lowe 2004), the question is particularly urgent for archaeologists because archaeology is considered to be simultaneously 'world heritage' and a means of legitimating local, indigenous, and national identities (Smith 2004).

Before describing the outline of the dissertation in more detail, I will briefly open up a discussion of what exactly it is that archaeologists are looking for—i.e., the epistemic object of archaeology—as a way of illustrating archaeology's problems of blackboxing the field, authorizing embodied knowledge, and making field-derived knowledge universal. I will then show why these concerns are significant beyond the discipline itself. The example of archaeology, I will argue, challenges existing models of scientific knowledge production that are based on natural and laboratory sciences in the Global North.

Archaeology's epistemic object and subject

I'm digging with a team of Chilean archaeologists in the dry sands of the Tarapacá desert and Emilio is trying to teach me the patience necessary for working in—literally—shifting sands.

Tired from several weeks working on a prior project, he prefers to sit under the shade of our one beach umbrella and handle the paperwork while I attempt to excavate the 1 x 1 meter square test pit. I squirm and puff in frustration as the sand spills under my neatly measured out square of string faster than I can remove it. Later I ask him pointed questions about how we can tell the difference between the layers, when seconds after I glimpse them they are smothered by the riverlettes that pour in from all four corners. "Relax," he says, "it's all pretty much the same anyway". We only distinguish between the sand collapsing into the unit and that being excavated when, finally finished, we try to identify the 'sterile' soil by the absence of charcoal. But the charcoal flecks are precisely what have been tormenting me all afternoon. Black dots that smudge when crushed, speckling the gritty ubiquity of the yellow sand. They ought *to be an indication of something 'cultural', a sign that the earth in this particular area has not lain here for thousands of years undisturbed by human activity, but once was exposed to (at the very least) the ashes that fell from someone's fire. But it is just as likely that the charcoal was blown in this same afternoon by the incessant wind—the wind that eventually sends the beach umbrella bounding across the desert, leaving a weird set of footprints each time it bounces off the ground. I set off running with a handful of charcoal from the screen still in my fist, and I almost catch it—my fingertips briefly graze the bright colored cloth before it tumbles and twirls away faster than Emilio can sprint, even though he keeps running until he and the umbrella look themselves like small dots against the shimmering dunes. When I turn around and walk back to the test pit, the walls have collapsed completely—but I place the charcoal in a numbered and labeled bag anyway. These little charcoal dots are always potentially something and nothing, hovering between being evidence and not: reminding us that they exist independently of anything we may try to do to them out here in this vast expanse of sand and wind.*

On another project in Bolivia a few years earlier, Robert spends a similarly long afternoon hunched over the intersection of two potential pit features. He carefully prods in each direction while agonizing over the subtle gradations of soil color and texture that refuse to organize themselves into clear-cut boundaries. The maestro—an archaeological worker from the local indigenous community working as his assistant—looks gently amused at Robert's indecision. Sitting on the opposite side of this potential pit the maestro says he has found his *edge and begins to follow it around, pulling the ashy soil inward from where he sees it intersect with the redder, more friable soil, occasionally glancing over as Robert tentatively stabs the earth with the point of his trowel and unhappily picks at the tiny clumps that are flicked up. "Pits", Robert later tells me, "are elusive". Watching his frustration I realize that you could spend hours agonizing over these pits, only to find when you think it is done that they have completely evaded you, and 10 cms further down into the ground all trace of the distinction you eventually thought you saw has disappeared. Robert would prefer the whole processes were mechanized, reduced to something automatic so the burden of responsibility was taken off the archaeologist. "Pits will trick you," he says, and some archaeologists working here privately suggest that the best thing to do would be to admit defeat and stop trying to get any 'control' over them at all.*

Archaeological objects are "hard to control". Whether artifacts-objects like ceramic shards and bones, or spatial-objects like an ashy trash pit or a 'cultural' layer in the sand, objects have a stubbornly independent existence out there in the world before, during, and after their entanglement with archaeologists. Although archaeology involves complex, expensive, and time-consuming interventions in that world, it would be misleading to see this as a process of transforming or purifying: a matter of control, modeling, or distillation.

Moreover, despite the hold they have over archaeologists and the amount of time, energy, and attention poured into their management, objects themselves are not the end-point of archaeological work. Once an excavation or survey in the field is over these objects have either been destroyed (in the case of spatial-objects) or are put into storerooms that may or may not ever be visited again. What leaves the field are records—vast accumulations of databases, forms, and images—that do indeed record objects, but more specifically they record the *relationships between* objects. Robert knew that there were two different pits, and he could have just emptied them both out. But the problem was deciding what the relationship between them was: was the ashy soil that filled the first pit stratigraphically[1] higher or lower, and thus younger or older, than the reddish soil that filled the second pit? Each aspect of this relationship would be recorded in exquisite detail on multiple forms and databases.

Archaeologists sometimes refer to records as a textual representation of the site. Theoretically at least, one ought to be able to read the records at a later date and understand the site as the excavator knew it. Archaeology has often been described as the process of systematically destroying something, and as such the record is the preserved version of the lost materiality (invoking images of an instruction manual on how to re-assemble the contents of this now gaping 5 x 5 meter hole in the ground). But although, in theory, this is how 'the record' is created, in practice the relationship between the physical thing encountered in the field and the written description of it is more complex. On another Chilean excavation, the team returned to a site that the director Matías had excavated several years previously. The team excavated Matías' old 1 by 1 meter square test units with the same attention to detail as the 'new' material around it.

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1 Stratigraphy is a concept and term borrowed from geology. In archaeology it refers to the principle that a layer of soil *above* will be of more recent date than a layer of soil *below*, because the the lower one must have been in situ before the higher one was lain on top.

One of the excavators, Luciano, chatted with me about how important it is to keep detailed records so that other archaeologists in the future (and particularly students who might study your material for their dissertation projects) could make use of your data. He told me there was a discussion taking place in Chile at that time about the best way to make records both physically accessible (stored in public places) and conceptually accessible (understandable by other readers). So I asked Luciano why he was currently re-excavating Matías' old units, when he could instead just look at the report from the previous excavation that was sitting in the truck. He replied, "it's better to see it with your own eyes than to read about it".

This throw away comment suggests a quite different relationship between the material things found during an excavation and the records that leave the site. It puts into question whether it is possible to know archaeology's epistemic objects (the relationships between material things) only through texts, or whether it is instead necessary to have an embodied, material engagement in order to understand them.

As another example, two US graduate students discussed with concern a project a friend was working on, where all the original members of the excavation team have moved-on to new projects and positions, leaving their friend (who was never physically present on the site) to write the report from the textual records alone. Similarly, a director justified the promotion of one archaeologist over another to a supervisory role, because only one of them would be returning to the same university as her and would spend the next year helping write the report. The archaeologist promoted to supervisor would, the director explained, need to have as much experience as possible of what was happening on site here and now, if he was going to be able to make sense of the records later. Although in an ideal world records should be textual equivalents of the now dismantled web of relationships between thousands of individual material things, in practice they function more like *mimetic devices*. They serve to help archaeologists remember back home at a later date what they saw and touched, and the decisions they made months or years previously while attempting to control objects in the field. The practice of knowledge making in archaeology thus relies heavily on a single tool: the archaeologist's body. And this body remains raced, gendered, class-bound, and nationalized, even as it is conceptualized as a *scientific* tool. The social and epistemic tensions that arise as a result of this situation are the subject of this dissertation.

Geographic terms used to describe archaeologists

This research is based on over two years of ethnographic fieldwork in excavations, universities, and conferences in Chile, Bolivia, the US, and Canada conducted between 2008-11. All of the personal names used are pseudonyms, as are many of the place names and all the project names. I followed three communities of archaeologists: 1) North American archaeologists running excavation projects in the Bolivian Andes, with the assistance of Bolivian archaeologists and local indigenous people employed as technicians; 2) Chilean archaeologists working independently in the Tarapacá region of Chile; and 3) US archaeologists who attempted to set up a field school in Tarapacá, but relocated to Peru after the collaboration with their Chilean colleagues broke down.

A few words of explanation are necessary to understand the way I am referring to these groups. By 'North American' I mean archaeologists trained in the tradition of US academic culture. This includes former and current graduate students who might not hold US nationality and those currently based in Canadian universities but trained in the US. I take this somewhat open definition because members of this community see themselves as sharing a common perspective, training, and disciplinary identity as a result of their shared training in US academic culture, rather than a shared nationality. For instance, some of the 'North American Andeanist Archaeologists' I discuss are originally from South America. Having been trained and worked entirely in the US, however, they see themselves as having little in common with 'Peruvian Archaeologists' or 'Bolivian Archaeologists,' and were described by other archaeologists who are trained and based in South America as being 'Gringos'. To be a North American Andeanist is thus to see oneself as coming out of a particular disciplinary community, but also to divide one's working life into periods of cyclical movement in and out of a field-site that is located in a different continent to one's home institution. North American Andeanist archaeologists are based in US and Canadian universities, but travel to Peru, Chile, and Bolivia once a year during their summers.

The 'Andes' in 'Andean archaeology' is also bounded in a specific way. The geographic area in which members of this community work does not necessarily correlate with the mountain range also known as the Andes. Collegiality and friendship between members of specific archaeological projects are more likely to define what geographic places are included or not. The region roughly encompasses the northern altiplano of Bolivia, the south of Peru, and increasingly the coastal area of Peru. The North American Field School project described in later chapters attempted to include northern Chile within this region and encountered severe problems in doing so, as I will discuss. This region is conceived of as being connected, but depends more on personal connections between directors and archaeologists than it does on either geography or the kind of archaeology being excavated. Leading on from this is the observation that national borders are remarkably easy for North American archaeologists to traverse. Crossing the border to Peru for a weekend to get a visa re-stamped—and in the process catching up with a friend at their excavation site—is very common. In contrast, hardly any of the North American archaeologists I met who worked in the north of Bolivia had visited the south of that country.

Bolivian, Peruvian, and Chilean archaeologist, however, are bound by their national borders. They tended to talk of 'Bolivian archaeology' or 'Chilean archaeology', rather than using the term 'Andean archaeology' as the North Americans did. When I asked whether they would be interested in working in a country other than their own, most Bolivians and Chileans I interviewed turned the question back around: "Why would one work outside one's own country? To want to do so implies a colonialist mentality." In later chapters I will unpack in more detail the meaning of the terms 'colonialist' or 'imperialist' to describe other scientists, in opposition to accusations of 'nationalist' or 'peripheral' sciences.

I was particularly interested in why Chilean archaeology students who appeared to have quite wistful and romantic visions of Bolivian had never visited there. This surprised me, given that Bolivia is not particularly difficult to visit from Chile, either economically or in terms of visa requirements. The exchange rate between the two countries is strongly in Chileans' favor, the cost of living in Santiago being not that dissimilar to the US. A few students expressed concern that Bolivia might be dangerous, but for the most part it appeared that it had just not occurred to them that they might be able to go, despite many having experience of foreign travel to places like Argentina. Discussing this tendency with one of the professors in the department, he commented that the difference is not so much a matter of geography, as it is that Argentina is culturally and politically closer to Chile than Bolivia. There are therefore layers of intraregional conflict that are important, but not extensively discussed in this dissertation because of lack of space: particularly the relationships between Chilean and Bolivian archaeologists, between Santiago and the north in

Chile, and between the altiplano regions and the lowlands in Bolivia. I intend to discuss these relationships in the future, but they are not explored in detail in this dissertation.

Outline of the dissertation

The core ethnographic finding of *Uneven Fields* is that conflicts arise within international collaborations when colleagues from different countries mistakenly assume they share a single understandings of what archaeology is as a social and epistemic project: that they share a single epistemic culture. Although from an anthropological perspective this should not be a controversial statement (Evasdottir 2005: 8), there is a strong feeling that science, and by extension scientists, should be impartially rational and able to 'overcome' national or cultural differences. They should inhabit what Traweek (1992) has termed the 'culture of no-culture'. In recent years increasing attention has been paid to the practice of science in postcolonial contexts, for instance Harding (2008) and Anderson (2002), but this field is still small and relatively unknown outside of science studies. Despite much excellent research on the relationships between archaeologists and indigenous communities/nation states by anthropologically-inclined archaeologists such as Abu El-Haj (2001), Murray (2011), and Ayala (2007), attention has not been paid to the differences in epistemic culture between archaeologists from different countries (although see Joyce 2008). Particularly lacking is research into the disparity in political and economic resources, and in epistemic authority, between archaeologists from the Global North and South.

Through a historically grounded ethnography *Uneven Fields* addresses these problems, by showing how an epistemic and political commitment to 'supranationalism' requires bracketing out the very forms of knowledge production that give archaeological field data its legitimacy. Such commitments, when coupled to an imagination of archaeology as informal and friendship-based (essentially a meritocracy), make it impossible to acknowledge the difficulties Chilean and Bolivian archaeologists face in having their expertise recognized by Northern colleagues.

The first half of the dissertation explores the consequences of this situation at the micro scale of excavation sites in Bolivia and Chile. Through attention to the how implicit hierarchies of gender, race, and nationality interpolate only certain individuals as *able to know* archaeologically, I trace the micro-social practices that constitute archaeological data, 'the field' as a specific kind of epistemic and social space, and archaeologists as themselves experts. In essence I describe the lifeworld of the field—a space in which otherwise mundane objects in the landscape are conceptualized as archaeological knowledge and people are recognized as experts through professional practice. At the same time I draw attention to national variations in the understanding of this lifeworld. Practices of knowledge production are to a large extent tacit because they are based less on formal rules, standardizable techniques, or non-human machines/spaces; and more on subtle, 'common sense' ideas of the evidentiary nature of archaeological knowledge and the appropriate performance of expertise. Precisely because they remain at the level of tacit 'common sense', these skills and forms of expertise are difficult to translate across national borders.

The second half of the dissertation poses the question of scalability by following the same conceptual problems at a national and international level. Specifically, I outline the constitution of an 'imagined supranational community' of archaeologists through Chilean and North American universities and archaeological conferences. In these chapters I pay attention to the broader national and international contexts within which archaeologists work, and specifically the institutions that structure their practice as a form of labor and/or vocation. The analysis here is both ethnographic and historical, as I trace how the universities within which most archaeological work is practiced (and where the disciplinary community reproduces itself through the education of new members) provide a structure or context for each epistemic culture. Through the example of Chilean archaeologists' struggles to maintain academic autonomy during both the dictatorship and the contemporary democratic era—in the training of students, the right to professional representation, and the regulation of commercial archaeology—I show how the debates about universalism, standardization, and meritocracy described in the first half of the dissertation 'scale up' at the national and transnational level.

The penultimate chapter uses the example of a failed collaboration between Chilean and US archaeologists to trace the movement and translation of people and facts between supranational, international, and national contexts. Through this case study I will show that simply assuming that supranationalism is a more objective, rational, and scientifically valid position is not sufficient to blackbox the historical legacy of centuries of North-South inequality. The box keeps breaking open, and the unresolved questions of race, colonialism, and nationalism that were bracketed out of discussions of knowledge-production within the excavations described earlier have not gone away. They linger in North American archaeologists' presupposition that their Chilean and Bolivian colleagues are insufficiently 'global', and the suggestion that only disinterested, supranational (i.e., foreign) archaeologists can occupy an objective 'view from nowhere'. In the concluding chapter I situate this concept of supranationalism within the existing literature on postcolonial and neocolonial science studies and 'Southern Theory'.

Uneven Fields thus weaves together three related theoretical concerns: how appeals to disinterestedness and scientific rationality are grounded in claims to speak to/for an imagined supranational community; the construction of the Global South as perpetually local in comparison to an inherently more global North; and the means by which archaeological knowledge is created and warranted when it is produced through the embodied expertise of raced, gendered, and nationally localized scientists.

Chapter Two

"A Well Oiled Machine": Hierarchy, labor, and archaeological excavation

Introduction: excavation methodologies

Near the beginning of the 2008 field season I sat down to talk with Olivia, one of the co-directors of the Tiwanaku project, about her experiences of field work.

> Mary: When you first started digging, was it something you enjoyed straight away or something you had to get into? Do you still enjoy it?
>
> Olivia: I do like it. But it's kind of one of these... I never... I never feel competent when I start. I am always, you know, **it's always a very humbling experience.** I didn't-- I never went to field school. ... [On my first excavation] I remember the first task they gave me was, [the director] wanted me to draw a section of a stone wall. To see if there had been a niche in it that had been closed. And I was too short to do it. And I realized in retrospect that I couldn't get the right view. If you put the drawing square [over the wall], the grid, you have to be facing it head on. So you could just sketch it. But it was above me, so I couldn't get it right, no matter what I did. And [the director] was very frustrated. And I felt very, you know, kind of humiliated. And um. The ceramics there were really boring, so that was, you know, nothing. But I liked the excavation, in the end, you know I liked it. And it was frustrating to excavate, because I was just digging through a wall fall. So it just meant pulling stones out of the ground, pretty much. So I never had straight excavation. Because it was just like stones that were lining-- **and it was disturbing because it doesn't feel like a text book.** So... But I did enjoy it in the end, and I really enjoyed excavation when I was working at [the next project]. Because we were excavating much more architecture. … [It] was like removing wall fall, but we were exposing room, room, room, room. And I moved a lot of rock! And it was great stuff, these beautifully preserved structures. And it was a lot of fun. That was like-- really. And we were treated well by [the director] and we were well fed. And I was given responsibility and I was able to live up to it. Which was-- it was a perfect balance.
>
> Mary: So it was more satisfying.

> Olivia: Yeah. And then I had to, after-- then it was a shock when I came here [to this region of Bolivia] because there was no stone architecture. It was all eroded adobe. And it was a different system. And um. **I had to just, swim. And kind of learn.**
>
> Mary: Was that difficult?
>
> Olivia: A little bit, at first. Yeah. Yeah. And um. Yeah I remember the first time I got adobe, it was like this really weird soil, that was all like, lots of different colors. And it felt kind of strange. And the maestro said to me, "Senorita, es adobe!" Yeah! And I encountered stuff that no-one expected to encounter. And so. **And I felt like I was being exposed to the more, kind of, text book kind of archaeology.** Yeah.
>
> Mary: So it felt more like the "real thing"!
>
> Olivia: Yeah! And it was hard. It was like, completely new. And I was left on my own. Just to do it. Yeah. And I mean everyone else was on the other side of the site. And that was a strange year. [My emphases]

Field work—and specifically excavation—is the action that defines archaeology for both archaeologists and the public at large (Holtorf 2006, although c.f. Lucas 2001: 2 in conversation with Tilley 1989). But excavation is not considered to be a skilled specialization in the way that, for instance, ceramic analysis or GIS is. Archaeologists who could potentially be seen as 'field specialists' are those who excavate year-round in commercial archaeology, but their work is considered to be non-expert and much lower status (Everill 2009). In Chile, where until recently there was only one university in the country offering a degree in archaeology, all students take a standard course in excavation and survey methodology that has been taught by the same professor for many years. Although to a certain extent this course was described to me by both professors and students as being rather perfunctory, its existence implies a focus on field techniques as a skill that future archaeologists need to be trained in. It also means there is unusual consistency among different archaeological projects because all professional archaeologists have completed the same basic training. The Chilean situation, however, is highly unusual. The US archaeological community is an order of magnitude larger than that of Chile, with hundreds of different educational institutions offering BAs, MAs, and PhDs in archaeology, so there is significantly more variation in the way students are taught. As Olivia described above, field schools are not compulsory and when they are offered they are frequently framed as study abroad programs, a means of socializing students into the discipline, or a means of staffing under-funded research projects rather than as professional training for future archaeologists (Baxter 2009, Walker and Saitta 2002: 200). In the US professionalization occurs at the MA and PhD level, but it is unusual for graduate students to receive any formal teaching in field methods. Graduate students learn how to write, how to analyze materials, and to read archaeological texts, but not how to excavate. In fact, a senior North American archaeologist with several decades of teaching experience told me that the only course she had ever had to cancel due to lack of interest was a graduate seminar on excavation methods: not a single student signed up. When they go into the field to take part in other people's projects or to do their own dissertation research, graduate students are expected to 'swim', as Olivia phrased it. The implication is that excavation is not a professional skill—even though, as Olivia and many other archaeologists reported, they still feel anxiety about their field competency (another described the first few years working in this region as a "good experience, but I was still kind of flailing.").

Olivia's comment that her first experiences of field work did not feel like real 'textbook' archaeology are also revealing, however. Archaeology knowledge as it appears in textual form contains very little discussion or description of archaeological practice in the field. The processes through which archaeological knowledge comes into being, both methodologically and socially, are rarely formally discussed. As such, a student on her first field project could be forgiven for her confusion and surprise at encountering a process that was far more difficult, messy, and confusing than the texts she had studied in her classes would have her believe. Descriptions of excavation method and practice are absent from formal accounts of archaeological work, for instance in conference presentations, journal articles or academic books.[1] This is because, to a large extent, the means through which archaeological knowledge is brought into being are irrelevant for the way that knowledge is used, how it circulates, or how it is evaluated as relevant or trustworthy at a later date. The interpretation of data might be called into question—at a Chilean conference, for instance, presenters included images of their artifact databases, to support statistical arguments about the significance of distribution patterns. But the way data is gathered during excavation is not. What this speaks to is the nature of *trust*, by which I mean the extent to which knowledge statements within an epistemic community are taken for granted or open to being challenged (Collins 2001). The interpretation of the significance of artifact distribution patterns is an appropriately 'open' problem that might be challenged. How those artifacts were excavated is not—it is taken on 'trust'; or to use Latour's phrase, the production of data through excavation is 'blackboxed'.

> Mary: So it sounds like, um, you really enjoyed the excavation, and the being-in-the-field side of it. Would you say that's the thing that really drew you into [archaeology]?
>
> Olivia: Originally.
>
> Mary: Originally. So is it not now, so much?

1 This does not mean that there is no talk of 'methods', however. On the contrary, textual archaeological products such as articles, books, and dissertations devote specific sections to extensive discussions of methodology and research design. These discussions, however, focus on the macro scale, or specific forms of artifact analysis. GIS and ceramic analysis, both mentioned as 'specializations' above, would be documented as 'methods'. But discussions of how the site will be excavated and by whom, what kind of recording methods will be utilized as a result of this excavation and why, remain unusual—particularly beyond the stage of a dissertation.

Olivia: Er... I enjoyed being in the field, excavating a lot, at [the site I studied for my dissertation]. But um... I don't have the time-- I think when you are excavating, you have to be totally immersed in it. Like you have to live and breathe it. Day after day. And you just kind of understand it like, it's your child, and you know all its birthmarks and blemishes and freckles. And I can't do that [now I am the director of the project]. I just can't. So I miss it. I just kind of... yeah. Although sometimes when I look at someone's units, I think-- God, I don't know, if I could do it anymore! If I could, you know. I mean is it like riding a bicycle?

Mary: Because we were saying this morning, when we were talking, that you were saying that now that you are directing there's no time to excavate, you have all these other responsibilities.

Olivia: Yeah. You know I feel sometimes *very* distant from the data. And it's only-- and it bothers me. Because … I was [my dissertation adviser's] research assistant for so many years, so. I input all the forms. I organized *everything.* And I understood all the data, back and forth. I knew everyone's excavation units. And I feel like I don't know my own. My own project. And it's only when the reports come in, that I can--

Mary: It's top down--

Olivia: --see--. Yeah. So. That's ok. I mean, it's having to learn to, to trust other people's interpretations. As [my adviser] did with me. He wouldn't have been able to interpret my excavations. That trust. I suppose it's a transition. Sometimes I wish that sometime in a few years, I can have a little project. Something I can pick away at!

The lack of formal discussion of excavation practice in written texts might imply that excavation methodology is standardized and that there is little variation in how people work in different countries or sub-disciplines. In practice, however, there is a great deal of variety in excavation methodology as a result of different contexts of practice. Moreover, these contexts and methods result in very different ways of conceptualizing what the status of archaeological knowledge object are and how they can be known—including whose statements can be taken on trust, when and why. When we look closely at the micro-scale of field practice it becomes clear that excavation methods are entirely bound up with the idea of what archaeological objects *are and are not*, the extent to which they *can be known*, and *who* is able to know them. Methods are thus fundamentally epistemic and ontological concerns. Variation between different communities of archaeologists is not simply a matter of doing things a bit differently, but a matter of different conceptualizations of what archaeological knowledge *is*, at a quite fundamental level. This suggests that there are multiple epistemic cultures of archaeology, rather than one that is shared across disciplinary and national borders.

But as Olivia's comments about the trust she has to place in the archaeologists who work for her suggest, the circumstances within which one works might not be ideal. She believes that it is necessary to know the archaeology like a child, to live and breathe it, in order to understand it—in order to really know what is happening, you need to have a close intimacy with every part of the process from excavation through to data analysis. This is not possible, however, in the circumstances in which she works in Tiwanaku. As the director her role is to oversee the work of others, leaving little time to immerse herself in the details they produce. This draws attention to the tension that exists between how a system is meant to function, and specifically the ability to know the object that is implied by that system, and what the archaeologists believe they need to really *know* their objects of study.

This tension between the ideal and the experienced is the subject of this and the following chapter. In this chapter I will use the examples of Andeanist archaeologists working in Bolivia and British archaeologists working in the UK to explore the context of practice: the social, economic, and intellectual conditions that structure how archaeology is carried out in the field, in terms of the organization of skilled and unskilled labor, the methods and technologies employed, and how the object of investigation is conceptualized. My argument is that the tension between the ideal and the experienced comes from the relative ability to control or account for this context. While British archaeologists have more freedom to shape the economic and labor conditions of their practice, in the Andes archaeologists must work within constraints imposed by working in a foreign country and specifically their obligations to employ large numbers of indigenous communities.

Running through this analysis will be an emphasis on an additional important point, namely the image of archaeology as an informal and meritocratic discipline and the experienced reality of unspoken hierarchies of expertise. This is essentially a question of sociality, so might at first appear to be unconnected to the more abstract question of the ontological nature of archaeological knowledge. I will argue, however, that the two are entwined by focusing on the practices in the field out of which knowledge is created through tacitly unspoken hierarchies of knowing and doing.

Empowering the excavator: ethnographic and historic discourses of British archaeology

There are several reasons for using British archaeology as a comparative example to Andean archaeology. It is unusually homogeneous, the UK is not a settler-colonial context so there are no ethical or practical considerations involving indigenous communities to consider, and it has been extensively documented by ethnographers and historians of archaeology. In addition, I am interested in the extent to which English language archaeological texts have circulated around the world and therefore demonstrate that principle I described above: that textual presentations of archaeological knowledge do not need to include discussion or description of the context of practice.[2]

The major postprocessual texts that came out of British archaeology in the 80s and 90s are part of particular tradition of British archaeological practice. These texts, and particularly the work of Ian Hodder, and Michael Shanks and Chris Tilley, were staples of the 'History and Theory of Archaeology' courses I encountered in each of the Chilean, Canadian, Bolivia, and US universities I visited. On discovering I was from the UK a surprisingly large number of archaeologists in South America would want to talk about these texts with me. Hodder's book *Reading the Past* (1986), for instance, has been translated into Japanese, Spanish, Italian, Polish, Lithuanian, Greek, Macedonian, Chinese, Korean, and Turkish. The Bolivian and North American archaeologists working at the archaeological site of Tiwanaku, Bolivia were familiar with such texts, whether or not they agreed with them. In the year I studied the Tiwanaku Project the directors were also experimenting with a new recording technology that also had a British origin—something known as a 'Harris Matrix'. Papers produced by the directors of the Tiwanaku project could easily find themselves sitting in the same journals as those produced by a British archaeologists like Hodder. But none of these papers would include a discussion of their methods—whether, for instance, they used a Harris Matrix and if so how/why, because the knowledge produced through these methods is considered to be commensurable without this level of detail. The texts assume a shared readership—a single community of practice with shared understanding of what archaeological knowledge is, who can access is, and how it comes into being—and thus a shared 'trust' in the security of knowledge products. If we open the blackbox,

2 I could have looked at US archaeology for the same reason, but US archaeology is not as homogeneous nor as well studied by ethnographers of archaeology. British archaeology is also a community of practice that the Andean archaeologists I studied in Tiwanaku had no personal experience of, and therefore highlights the extent to which the texts are considered 'readable' when separated from the cultural and epistemic context of their production.

however, Andean and British archaeology appear to be built on subtlety different conceptualizations of the archaeological object, and different notions of trust at the level of recognizing the labor and expertise of specific kinds of people: 'workers' versus 'archaeologists'.

While there are countless ethnographic studies of the relationships archaeologists have with indigenous communities,[3] historical studies of most countries and sub-disciplines, and a smaller number of studies of archaeological practice,[4] British archaeology has attracted by far the largest amount of attention from ethnographers and has been subject to many detailed auto-critiques. A strong critical discourse emerged in the 90s surrounding the role of the excavator in terms of both his or her centrality to the epistemic process and his or her class, and as a result a great deal of attention has been paid to the structural and social organization of excavations in relation to the ontological status of the archaeological object, that is related to the kind of postprocessual critiques of archaeological hermeneutics that emerged from the UK in the 1980s. The studies of British archaeology are thus very valuable to the argument I wish to make here.[5]

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3 E.g. Hamilakis (2011), Ayala (2007), Ardren (2002), Derry and Malloy (2002), Breglia (2005, 2007), Kraemer (2008), Castañeda (2009), and Murray (2011) to name only a tiny fraction.

4 E.g. Israel-Palestine:Abu El-Haj (1998), Bolivia: Yates (2010) and Kojan and Angelo (2005), Chile: Scattolin (2006), Hawaii: Kawelu (2007); Argentina and the US: Gero (1996) and in response Politis (2001) China: Evasdottir (2005) Australia: Moser (1996, 2007) and Germany: Roveland (2000).

5 It is important not to overstate the size of this written discourse in the UK however, as much as it is necessary to draw attention to the personal and professional connections between the authors. The most extensive and detailed critique of archaeological practice in the UK and US is Gavin Lucas' *Critical Approaches to Fieldwork: Contemporary and Historical Archaeological Practice* (2001). A small number of detailed ethnographies of archaeological practice, notably the work of the archaeologists Matthew Edgeworth (2003, 2006) and Paul Everill (2007, 2009), and the social anthropologist Thomas Yarrow (2003, 2006), have focused on the practice of British commercial archaeology, as has my earlier work (Leighton 2010). Other notable ethnographies of archaeology that are more concerned with the relationship between archaeologists and local communities, also have a British connection—for example Yannis Hamilakis (2011) works primarily in Greece but was trained in and is based in the UK, and Cornelius Holtorf (2006) was also trained and taught in the UK. Meanwhile Çatalhöyük, a site that has become known for its extensive use of ethnography of archaeology as a means of self-reflexive critique (e.g. Hodder et al. 2000, Shankland 1996, Erdur 2006), is directed by the British archaeologist Ian Hodder (although the North American archaeologist Christine Hastorf was jointly responsible for initiating the Çatalhöyük archaeological project, before she returned to her research in the Andes). Çatalhöyük also employs professional archaeological excavators from the Cambridge Archaeological Unit (CAU), a commercial archaeological company based in Cambridge and nominally associated with the University of Cambridge. Gavin

There are two major points to emphasize about British archaeology. The first is that British archaeologists are concerned with class and the idea of 'democratic digging'. This arises from the specific British context—class is the major heuristic through which social critique is made in the UK (in contrast to, for example, race or gender), and structures the way people experience and discuss daily life. Rather than assuming the existence of a meritocracy, which I will argue is the position US archaeologists begin with, In the UK there is an assumption of class-based inequality that always already needs to be taken into account. But the concern with democratic digging is also based on an argument for authority grounded in individual experience and this is tied to the second point: that British archaeology requires/assumes a professional labor force, and methodology is thus based on the assumption of a professional, expert knower. In this way, methods, labor practices, conversations about class, and discussions of archaeological theory are all bound together, and a concern for documenting *who* the excavator is and whether they are able to move up the ranks, is incorporated into the debate surrounding recording technologies and means of understanding the nature of archaeological knowledge.

A brief history of British excavation

Early archaeology was the pursuit of gentlemen antiquarian-archaeologists, who would not excavate or collect material themselves but instead rely on others to bring material back to them (Lucas 2001: 3-5). By the beginnings of the twentieth century fieldwork gradually became

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Lucas, Paul Everill, and Thomas Yarrow all worked for the CAU during the late 90s or early 2000s. I was first trained and subsequently employed as an archaeologist by the CAU as well, between 1998-2003. Like Lucas, Everill, and Yarrow my subsequent work has been strongly influenced by conversations I had with CAU archaeologists such as Mark Knight, Lesley McFadyen, Roddy Regan, and Duncan Garrow.

associated with travel and the cultivation of a (usually masculine) scientific persona (Lucas 2001: 7; c.f. Kuklick 1997). As figures such as Heinrich Schliemann and Howard Carter gave fieldwork an aura of romance, similarly upper-class men like General Pitt Rivers and Flinders Petrie worked to develop it into a modern science. Pitt Rivers epitomizes the image of this period: a charismatic aristocrat, directing large teams of workmen excavating prehistoric barrows found on his own estates.

Both Kathleen Kenyon and Mortimer Wheeler's excavation techniques in the 1920s and 30s reflected a new focus on understanding structures and the layout of individual sites (reflecting a concern with "culture*s*") rather than only artifact sequences (a concern with seriating "culture") (Lucas 2001: 37-47). But Wheeler was particularly influential in terms of his military emphasis on order and discipline on site, and for being the first to introduce 'site supervisors' who were trained as archaeologists and thus had a chance of eventually becoming directors themselves (Collis 2001: 9).

> Wheeler introduced on to his excavations a hierarchical organisation of trained supervisors whose responsibility it was to label finds, to label and draw sections, and to keep records in a site note-book linking this information together with numbering and descriptions of layers. The actual excavation was still largely the domain of the paid workman, though supervisors were expected to dig as well – indeed, this was part of their training. The use of the trench meant that diggers could be broken down into small groups and their work closely monitored. (Collis 2001: 9)

After the 1950s a 'revolution' took place in archaeology as a result of both technical and social change: namely, the use of mechanical diggers that allowed larger areas to be worked on simultaneously; and the rapid expansion of access to higher education, which generated an abundance of archaeology students willing to work for little or no money (Collis 2001: 11-16).

The shift from staffing excavations with student-volunteers rather than paid workmen, and then later with skilled professionals, has been described by historians of archaeology as both the *professionalization* and *democratization* of archaeology, as directors and their supervisors were no longer considered to be the only experts on sites. Lucas (2001: 8) argues that John Coles' 1972 manual for directors, with its frequent reminders that the director must be professional, humane, and organized, while the volunteer-worker is positioned as both valuable and subordinate, was probably one of the last of its kind.

> The organization and conduct of a competent field project in archaeology generally requires a variety of talents: a leader, specialists in various disciplines, supervisors who have experience in the field of study, and assistants who are generally called volunteers or, simply, workers, to distinguish them from the others. … [Workers are valued for their local knowledge, but are expected to be disciplined.] … In a relatively large project, the workers' role is simple, to follow the instructions of their supervisor, to learn any necessary techniques from him or her, to gain an insight into the particular problems of the site through work, observation, and thought. (Coles 1972: 5)

During the 1960s and 70s New Archaeology/Processualism transformed the kinds of questions archaeologists asked and focused attention on the scientific rigor of the methodologies they employed to answer them—in the UK archaeologists like Phil Barker argued for more 'controlled' excavation techniques (Barker 1977). However, in the UK these theoretical debates occurred within a dramatically shifting social and economic context. Following the Second World War a rapidly growing number of large urban excavations needed to be undertaken quickly because the sites were soon to be built over or destroyed; and the university graduates who staffed them increasingly sought paid careers. These factors were as significant in bringing about the new 'professional era' of excavation as the growing interest in later time periods, urban contexts, and socio-economic questions about the past.

Although new, 'systematic' excavation methods were being debated by many processual archaeologists at this time, the establishment of the Winchester Research Unit (WRU) in 1961, is now seen as the birth of the current 'professional' system. During the excavations at Winchester by Biddle and Kjolbye-Biddle, open-area phase excavation became a new norm[6] and Edward Harris developed the 'Harris matrix system' in tandem with the 'single-context planning sheet' (suggested by Lawrence Keen and developed by Harris and Patrick Ottoway [Harris and Ottoway 1976; Harris 1989: 95, referenced in Lucas 2001: 75]), as a way of solving the problem of complex post-excavation analysis in the large, urban, multi-phase excavations undertaken by the WRU.

A Harris matrix is a diagrammatic representation of archaeological events arranged according to stratigraphic[7] relationships that must follow strict rules; single-context planning is a form-based system of recording archaeological events; and open-area phase excavation is the method of excavating whereby all periods of a site are systematically excavated in their entirety, rather than in parts (these are described in more detail below). Open area excavation, single-context planning and the Harris matrix are part of the same methodological system: each depends on the logic of the other, although (as we will see later, when looking to Bolivia) they are all often used separately without full understanding of how they are inherently interdependent.

6 Phase excavation means digging the entire site according to the 'phases' of occupation in the past, each in reverse chronological order as they are encountered by the archaeologists digging down. This is in contrast to previous excavations that would be looking for a specific period, e.g., the Roman period, and would ignore anything earlier or later, by either rapidly taking off the more recent periods, or leaving the site once that which is of interest has been completed.

7 Stratigraphy is a concept and term borrowed from geology. In archaeology it refers to the principle that a layer of soil *above* will be of more recent date than a layer of soil *below*. Excavating according to stratigraphic relationships therefore implies excavating with attention to the layering of soil sediments, with the understanding that these relate to the depositional history.

The combination of these methods and technologies, however, enabled/lead to *paid skilled excavators* who were responsible for both recording and interpreting the section of the archaeological site they worked on, rather than large teams of volunteer-workers who were overseen by an archaeologist supervisor who both told them what to do and recorded/interpreted what they produced. By the end of the 1970s, and particularly after the introduction of neoliberalized commercial archaeology in the 1980s (Everill 2007, 2009), nearly all excavation work in Britain was done by full-time, professional 'field archaeologists' who had university degrees and followed a remarkably standardized combination of the Harris matrix + single-context planning + open-area phase excavation method.

The Harris matrix, single-context planning, and the professional expert archaeologist

The Harris matrix plus single-context planning system requires both a specific conceptualization of the knowability of the archaeological record, and of the archaeologist as an expert knower. A context sheet and a field-diary, for instance, stand in opposition to each other as methods of recording. They assume different kinds of trust or trained-judgment in the person filling them in. A field-diary is attached to a single archaeologist, who records everything that they (or the workers they supervise) excavate. In many cases the field-diary is organized like a journal: the archaeologist writes what they do each day, recording details of the area they are working on sequentially. In other cases, as for instance on Kenyon's excavations in the early twentieth century, the diary is organized so that a single page corresponds to part of the excavation: a grid, or a feature, per page (Lucas 2001: 56). The archaeologist moves back and forwards between the pages but writes in free-form: the page is presented as a blank for them to fill in as they deem fit.

The innovation of the context sheet lies in its standardization and the non-sequential nature of its use. A single standardized sheet is created with boxes for specific information about the entity being described, the conditions of excavation, and a large space for interpretation/description. Forms are numbered and each archaeological 'context' on site gets its own form. What a context *is* is significant, as I will discuss below. The archaeologist excavating the entity fills in the form, then returns it to a central file overseen by a site supervisor.

The difference between the diary and the form is thus both a shift in the way the archaeological entity is seen in relation to the rest of the site, and the way the archaeologist holding the pen is framed in relation to the person holding the trowel. The logic of a diary is that the excavation of the site needs to be understood from the perspective of a limited number of individuals who are likely to make mistakes and will need to go back to see what they thought/did previously. A record needs to be kept of the changing understanding of the site as a whole, as the work progresses. The logic of a context-sheet is that there are many excavators, each of whom can be trusted to understand, excavate, record, and interpret the spatial-object they are working on individually; and therefore the process of excavation is a matter of gathering information from these many, separate units of analysis. The WRA method was thus developed specifically for complex but time-sensitive excavations, within which a large number of professionals could work together to get the job done quickly and efficiently without having to check up on each other. The system implies 'trust' in the professional expertise of each individual excavator.

The use of context-sheets is called 'single context planning' or 'single context recording' because of its combination with the Harris matrix logic of stratigraphic units. This system was developed during the Winchester excavations, later taken up by the Department of Urban

Archaeology in London (later called MoLAS [Museum of London Archaeological Service]) in 1975, and quickly established as standard throughout the UK by the 1980s.

> The primary consideration in the excavation and recording of sites using this method is the stratigraphic sequence. … The basis of the approach is to divide up the site into discrete units or contexts which are excavated and recorded according to their stratigraphic position: '*Any single action, whether it leaves a positive or negative record within the sequence, is known as a "context"* (Westman 1997: 1.2, emphasis in original). The definition of a unit is thus in terms of its physical, that is spatial, limits which are defined either negatively (e.g., a cut) or positively (e.g. a deposit) and interpreted as representative of a single action or event. … Each unit then has a stratigraphic position – that is a position relative to other units in terms of the sequence – and this is represented in terms of the Harris matrix (Harris 1989). (Lucas, 2001: 58)

'Single context' means that each unit—either positive or negative (see below)—gets its own context number and thus its own context form. The significance of this is that it requires that *the entire context is excavated as a whole*. Excavating only part of the context—i.e., to excavate in standardized 10 or 20cm sections cut through the whole, or to excavate half of it separately to the rest—is *not logically possible*, if the Harris matrix is to be used. This is because the system is based on a specific conceptualization of *what* is being excavated.

There are three different ways of conceptualizing what is being excavated which are usually represented by what entities are given numbers. The first is that one is excavating and therefore assigns numbers to *excavation events*. So for instance, we could mark out a 1x1 meter square on the ground and remove all the earth within this square to a depth of 10cm, then assign a number to this 1m x 1m x 10cm entity. In this case we would be conceptualizing entities that need to be recorded and tracked based on *something the archaeologist has done*.

The second conceptualization is that what one is excavating are *archaeological entities*. In this case, for instance, we could designate one type of soil as a trash pit in contrast to the soil around it, remove all this soil, and assign a number that represents the soil before it was moved out of the ground (this would be written on drawings showing it before it was removed, but probably also drawings showing the hole it left behind) *and* the soil out of the ground as it is processed. In this case we would be conceptualizing *bounded material objects* as things that were made in the past and still exist in the present.

The third conceptualization forms the core of Harris' innovation. It stipulates that what one is recording are *archaeological events*. We can record events that are either 'positive' or 'negative', based on the kind of material trace they leave in the archaeological record. 'Positive events' are events that leave something material behind. 'Negative events' are events that take something material away. So if we found the same trash pit as above, we would assign it two numbers. The first represents the event in the past of digging the trash pit (the 'cut'), which left the material trace of a hole in the ground (a record of a negative event because earth was removed to make a hole in the ground). The context sheet would then be a record of the shape of the hole cut during *the event of digging a trash pit in the past*. The second event from the past is that of filling in the pit with trash and dirt (the 'fill'), which leaves the material trace of the soil now compacted into the shape of the hole (a record of a positive event because soil is left behind for us to find). This would get a separate number, and a separate context sheet that records the material properties of the soil, including its compacted shape with the understanding that what is being recorded is *the event of filling in a pit with trash*. So there is overlap—both the pit and its fill are the same shape—but one is a hole and the other is soil, and the two correspond to separate *events* that may not necessarily have occurred at the same time in the past. Within this model, what the archaeologist is recording are *actions in the past that left material traces*. Each context is recorded separately on its own sheet of paper and with its own number: hence the name 'single-context' planning or recording.

Logically and practically, these three conceptualizations can't overlap. Logically because each system rests upon a different conceptualization of *what* is being excavated and recorded. Practically, because it causes confusion between different numbering systems. The principle of the Harris matrix requires the third system, because it is founded entirely on the idea of recording *events* rather than *entities*. However, combinations of these three conceptualizations can be found in many different archaeological communities around the world.

Coming back to the question of *who* excavates, we can see why this distinction is important to questions of professionalization and hierarchy. The impulse to use field-diaries that preserve a chronological narrative of the excavation, and the recording of excavation events through divisions of space into systematic units (e.g., in 10cm levels, which are referred to as 'arbitrary' as opposed to 'natural' levels), rests on a different level of trust in the excavator. Single-context planning requires that the entire context is removed and recorded in one go, given a single-number, and taken out entirely so that the context underneath it can then be seen, removed, and recorded in its turn. For this to be possible, one needs to believe that the excavator is capable of recognizing the context when he or she sees/touches/excavates it, and of removing it without either over- or under-excavating. Digging in arbitrary units of excavation is a way of controlling for excavator error because if they make a mistake, incremental stages will have been recorded ('saved') separately.

Single-context planning and empowerment

The difference in trust between the two methods has been considered from a slightly different direction by some British writers, who have debated whether this method of excavating results in a more 'empowered' archaeologist. This question has been posed as both an epistemological concern and as a form of class critique. The argument in favor is that single-context planning is empowering because every excavator on site holds both a pen and a trowel. In the old model, a single director or supervisor filled in the field diary and thus only one person had the power to write the definitive interpretation of the entities that the silent excavators were working to uncover and create. Tellingly, the excavators were often working-class laborers in relation to middle-class supervisors and upper-class directors (e.g., Berggren and Hodder 2003). The counter-argument is that single-context recording is *not* empowering because *in practice* the process of using a standardized form restricts the excavator and interpolates him or her as one of many, as interchangeable, and as him or herself standardizable (e.g., Yarrow 2003, 2006).

This debate undoubtedly draws on the history of the separation between field archaeologist and academic archaeologist, the manual worker and the writer, that dates back to the 19th century antiquarian collectors described by Lucas. But the distinction between those who write and those who dig was also a central theme of the postprocessual critique of the relationship between interpretation and data description. If archaeological knowledge making was not a matter of an archaeologist either subjectively or systematically interpreting inherently objective data, then archaeological practice could not be divided into technical, mechanical excavation and active, creative writing. Instead interpretation began "at the trowel's edge": all archaeological work was interpretative (Hodder 1999) and excavation itself is a 'craft' (Shanks and McGuire

1996).

For instance, Åsa Berggren and Hodder argue that although the use of single-context planning was a recognition of the professionalism of excavating archaeologists, and thus a step up from what they imply was a colonial use of workers (2003: 422), the forms quickly became so codified and the balance of description to interpretation so skewed in favor of the former, that they became associated with unskilled labor (2003: 424). The digger is shut out, 'voiceless', and no longer able to contribute his or her interpretation. Beyond being a social injustice and perpetuating class-inflected inequalities that relate to the value of manual versus mental labor, they argue that this is damaging for the production of accurate archaeological knowledge:

> So with these new technologies there is the potential for the long history of a gap between the excavator and the interpretation to be continued. Once again the moment of discovery at the trowel's edge gets separated from the moment of study. Data are separated from analysis and interpretation. The excavator is forever pushed back into the realms of the 'unskilled', a provider rather than a producer of data. (Berggren and Hodder 2003: 425)

One of Hodder's solutions was to develop a form of 'reflexive methodology' at his site of Çatalhöyük in Turkey, where (at least originally) the majority of the excavators were full-time professional field archaeologists from the Cambridge Archaeological Unit. Various strategies were used to shed critical light on the excavation process including the use of video diaries and ethnographers (Hodder et el. 2000). The major conclusion from this process focused on a perceived gap between the artifact specialists and the field excavators. A paper written collectively by the excavators, however, challenged this conclusion. Quoting Hodder's (1997) desire to increase communication between excavators and specialists and thus achieve "a re-empowering or re-centering of the field excavator", Shahina Farid and her fellow Çatalhöyük excavators argued that:

> ...the empowering tool was already in the hands of the excavator by way of defining the archaeological unit. All data analyzed by the laboratory teams were reliant on the definition of the unit and its stratigraphic relationship as the excavator perceived it. The field person created the unit and this could be regarded as an assumption to be tested by the laboratory staff, but in practice the initial interpretation was rarely contradicted. (Farid et al. 2003: 24)

Gavin Lucas falls uneasily between the two sides of the arguments. He argues that power is being “concealed and dispersed”, for instance through the box at the top of the form that requires a supervisor to sign that they have checked it.

> Currently, each excavator (or 'site assistant' as they are often called) can be responsible for the whole process from digging to recording a feature on a site and the director/supervisor merely co-ordinators; this gives the impression of democratic digging – yet the excavator's focus is very narrow and the level of interpretation minimal (for example, 'this is a pit'). This is not to denigrate the skill involved, but it is an illusion to think that the average excavator of today has necessarily any more *power* on a site than had the labourer of a century ago. In the end, the supervisor/director, the one who writes the report, determines the interpretation of the site not only by being in possession of the 'wider picture', but by selecting *where* and *what* the site assistant excavates. In many ways, site assistants are completely interchangable – he or she is not a person but a digging machine, and although some assistants may be more efficient than others, their 'local knowledge' or personality is often ignored and certainly never mentioned in any contemporary manuals on fieldwork. (Lucas 2001: 8-9)

That the majority of ethnographies of archaeological practice to date have come out of the UK should no longer be surprising, then, as archaeologists took up this methodology as a form of auto-critique that aimed to understand excavation as an interpretative act. For instance, the archaeologist Matthew Edgeworth’s ethnography of a commercial excavation in Britain in the late 1980s (2003). In Edgeworth’s account, over time the single excavator is assumed to have more authoritative understanding of an object, through tactile engagement with it. On site, supervisors come by to talk to individual diggers throughout the day. On initial visits they primarily give the digger instructions—but on later visits, once the digger has engaged with a context, the supervisor gives more credence to what the digger has to say. The digger gains authority from being the only one to have *material experience* of the context, as opposed to *looking* at it.

> For it is well-known on site that the person who has actually dealt with the evidence at first-hand, has witnessed the emergence of it through time, and indeed has brought about its emergence through his own actions upon the material field, knows more about that particular pattern of evidence – in a practical sense – than anyone else. The importance of touching and manipulating the evidence, as opposed to merely visually observing it, is illustrated by the way in which supervisors visiting a field invariably pick up a trowel and engage in a brief material transaction ..., in order to get some additional idea of the all-important 'feel' of the material. (Edgeworth 2003: 47)

By 2003 ethnographers of archaeology such as Edgeworth were referring to something that became 'common-sense' in the UK as a result of the WRU method of excavating; namely that *the only person who can fully understand a context is the person who has physically excavated it.* This appears at first glance to echo Olivia's comment above that you need to know your data like your child. But in the kind of archaeological practice she is describing there are always already several layers of expertise between her and the trowel, so the data she imagines knowing is a step removed from that Edgeworth assumes. Moreover, while Olivia expresses this view, the system she works within (as we will see) is structured in such a way that this necessity is not taken into consideration.

This both underlines and complicates the concept of trust inherent in the single-context planning model described above *and* its alternatives. The arbitrary-level/field diary notion versus single-context planning form *could* be interpreted as a distinction between thinking of the object as unproblematically and mechanically knowable versus an object known through trained-judgment (Daston and Galison 2007)—but this would not be correct. Both systems in their *written form* allow for no ambiguity: the object either exists or does not; it is marked by a clear, unambiguous line drawn on a map and a precise measurement recorded in a form. In both cases a hard and fast decision must be made and there is not the possibility of recording an entity's existence as uncertain or problematic. But the arbitrary-level/field diary model builds into itself a modified trust of the excavator and an assumption they will make mistakes that must later be dealt with by someone else (e.g., the director or supervisor). The single-context planning model is a matter of recognizing that even though the excavator might make mistakes, the excavator is still always a *more authoritative knower* than someone looking at the textual records retrospectively because of his or her *direct embodied experience* of bringing the object into being.

Yet at the end of the day, what does 'empowerment' really mean? Paul Everill's in-depth study of the state of British commercial archaeology in the 2000s comes from a position sympathetic to Edgeworth, but draws attention to the larger structural problems of essentially vocational professionals working within a neoliberalized, commercial sector. Everill argues that chronically low pay, short term contracts, poor working/living conditions, and little room for promotion or advancement, when coupled with little acknowledgment of excavators' contributions to final reports and publications, mean that excavators are 'invisible' and thus barely better off than the laborers employed by Wheeler in the 1930s. Although he wishes to draw attention to and celebrate the centrality of the excavator in the production of archaeological knowledge, Everill reminds us that this needs to be balanced with a conversation about the lack of actual socio-economic and epistemic power excavators currently have.

"A Well Oiled Machine": Andeanist archaeology in Bolivia

The example of British archaeology draws attention to how epistemological problems and socio-cultural values become entwined, through the debate over the power of the excavator in relation to recording methodologies like forms. But at the same time British archaeology is remarkably uniform in its practices and we cannot extrapolate directly from British archaeology to archaeology in general.[8] If the organization of British archaeological practice is based on a hyper-sensitivity to class-based hierarchies and a post-processual critique of archaeological facticity that arose from the specific cultural history of post-war Britain; then perhaps we ought not to be surprised that something quite different emerges from the processual-inflected, (relatively) social archaeological tradition of late-twentieth century US and Canadian archaeologists working with indigenous communities in the Bolivian Andes. In comparing the two we can see how methodological strategies, specific ethics of hierarchy and democracy within a labor structure, and the ontological status of archaeological facts, are all *presumed by* and *arise out of* each other.

The most noticeable difference in Andean archaeology is that most of the excavation is undertaken by indigenous workers rather than by archaeologists. This arrangement is stipulated by the indigenous communities who are the legal and moral guardians of the land being excavated and who would not grant permission to excavate if they were not also gaining financially from mass employment. More subtly, the methodological system employed in Andean

8 We should therefore also be curious about the circulation of texts that talk of 'archaeology' when in fact they are coming out of a specifically British paradigm of knowledge production—a point I will take up later.

archaeology attaches different meaning to points that are central to British practice: the epistemic and moral value of embodied experience (that the excavator is the one who truly 'knows' the object being studied); the nature of the archaeological record in relationship to what is written/numbered and by whom; the importance of conversations between excavators before written texts are produced; and the structural organization of an excavation in terms of an ethic of egalitarianism versus hierarchy.

The structure of labor relations on the Tiwanaku project

The three co-directors of the Tiwanaku Project—Olivia, Emily and Chloe—were North American (NA) archaeologists, heading up a team of seven Bolivian and four NA archaeologists (not including myself). Of the archaeologists, two worked in the lab, seven were excavators, and one was in charge of the survey. Officially there is a role on all Bolivian sites of *contraparte*: a Bolivian co-director. This role was filled by Pablo, a Bolivian archaeologist who had his *licenciatura*[9] degree and was thus more senior than the other Bolivians who, lacking their licenciatura, were still officially students. But in practice the contraparte role is symbolic and Pablo had very little actual say in decision making.[10] This contraparte role and directorial roles were the only formal difference between the archaeologists, who in theory were otherwise all

9 Not to be confused with the Chilean *licenciatura* described in Chapters 5 and 6. In Bolivia the licenciatura is the highest degree that can be achieved, and is the mark of professionalism. It requires several years of specialized course work and an independent research project resulting in a thesis similar in length and depth to a British PhD thesis.

10 In Peru there is a similar contraparte position, that in Andeanist archaeological circles was also symbolic rather than an actual co-directorship. Recent reforms to the legal requirements of foreign archaeologist have changed this situation, such that there is increased pressure to make the contraparte a real co-directorship. Although I was told of an excavation in Moquegua, in the south of Peru, where the Peruvian contraparte was given more than symbolic power, this still remains a gray area in practice.

equal.

Hierarchy was attended to in interactions with workers, however, although this hierarchy was understood to arise from the indigenous community the archaeologists were working with and who owned the land (referred to here as the Comunidad) and to be against the inclinations of the archaeologists. The archaeologists were entirely outnumbered on site by the local workers drawn from the Comunidad,[11] who would total between 50 and 60 at any one time. The majority of these positions rotated, meaning that each week a different member of the community took on the job, but the roles are also highly compartmentalized: each person has one very specific role.

Groups of indigenous archaeological workers were divided into groups of 4-5 people, known as *quads*. Each quad was headed by a *maestro*—a term that translates as master, and signifies a person in charge by dint of his or her superior skill and experience. There were six excavation quads, three quads connected to the lab, and a quad in charge of archaeobotanical sampling.[12] Like other Aymara communities in this region the Communidad was divided into four *zonas* that relate to hierarchical arrangements of kinship and geography (for general background of Andean community organization see e.g. Allen [1988: 102-119]. For a more recent description describing Tiwanaku specifically, see Sammells [2009: 44-5]). The excavation quads were divided equally between the four different zonas, so that each quad was made up of members of the same community zona.[13] A final position was that of the *facilitator* who served as a go-

11 On this project they were drawn from a single community rather than the established Union of Archaeological Workers of Tiwanaku, which is made up of members of each of the communities in Tiwanaku. I refer to this community as the Comunidad..

12 In addition there was a sewing quad, responsible for making all the cotton draw-string bags that were used for storing artifacts, and four cooks who catered to the archaeologists' domestic life (i.e., cooking, cleaning and laundry).

13 The lab, sewing, and flotation quads had one member from each zona. The cooks, meanwhile, were entirely separate. These women were among the highest paid of all staff, and had in some cases over a decade of experience working for many of the North American archaeological groups in this region. Two new young

between for the archaeologists and the Communidad: negotiating, making deals, and solving problems as they arose.

Only the maestros held permanent positions throughout the field season, although after a great deal of negotiation it was agreed that the positions of *contra-maestro* (the maestro's assistant who was in effect being trained to become a maestro himself) would also be permanent. All other positions changed every week and it was the Comunidad (through the heads of each zona) who decided who would get jobs. The result was a constant turnover of staff, such that new people continually needed to be trained. This system was the source of much tension, as the archaeologists obviously would have preferred to prioritize employing people with experience, or at least enthusiasm and/or physical fitness. For the Comunidad, however, circulating jobs was the fairest means of ensuring everyone benefited economically from archaeological employment—although in practice there was a great deal of intra-community political maneuvering and certain families secured a larger share of more desirable jobs than others.

The archaeologists accepted this situation because they had little choice. Legally and morally they required the Comunidad's permission to excavate and permission rested on accepting this division of labor. Acknowledging the disadvantage of repeatedly having to train new staff, or of having in some cases to make use of people who were too old, young, or disinterested to work, they focused their negotiations on a small number of positions that required a great deal of training. In long meetings at the beginning of each work season, the archaeologists would bargain over other positions, but would sacrifice them to keep control over the most skilled positions. It invariably came down to a struggle over the lab maestro position and the

women were employed in 2008, one of whom was the daughter of the facilitator. But the other two older cooks were from entirely different communities elsewhere in the Tiwanaku region and were highly sought after by NA archaeologists for their friendliness, experience, and ability to cater to US diets.

archaeobotanical flotation positions, which in 2008 were occupied (after a tense debate) by men who had been trained over many years to become experts in archaeobotanical processing and analysis, rather than by rotating workers with no experience at all.[14]

This arrangement also worked for the archaeologists, however, and specifically it allowed an enormous volume of ground to be excavated in one or two months, although the number of people involved meant that the pressure was often intense. If something delicate or intricate was found like a burial, or even just something complicated that took a while to figure out, the maestro and the archaeologist would work under the focused gaze of an increasingly bored team of workers.

These teams, known as *excavation quads*, were made up of five people: the maestro and contra-maestro (always male), a *bucket carrier* (male or female) and two *screeners* (nearly always women but occasionally an elder man). Each excavation quad was under the control of an *excavation archaeologist* (male or female). The excavation maestros were the most prominent men on site, with in some cases decades of experience working on many different archaeological projects. The *contra-maestro* worked as an assistant to the maestro, excavating alongside him with a trowel as necessary. The bucketer's job was to ferry soil backwards and forwards between the excavation unit and the screeners, who waited next to a series of large wooden sieves arranged at the side of the excavation area. The large sieves, mounted on tall wooden tripods and

14 To understand better the stakes of this point: The maestro positions are the most well paid. The lab maestro also gets to sit indoors all day while he works. Therefore prominent members of the Comunidad want to get this 'easy' and well paid job for themselves or a member of their family. They argue that it is not fair that only one person gets hold this lucrative position, because wages should be shared equitably among the different zonas and families. The archaeologists, however, want the job to be occupied by one particular individual because the work involved is skilled, and this individual has been trained over many years to analyze botanical material, and has worked on this particular task for many different archaeologists. He is respected by the archaeologists for his skill and experience, and this is more important to them than fairness, because it would be impossible or difficult for an unskilled person to do the work.

known as *screens*, were used to sieve the soil that was removed from the excavation. The screeners shifted through the objects left in the sieve after the soil was shaken out, looking for artifacts that they then sorted into bags of lithics, bones, and ceramics.

How to make sense of this large catalog of people and positions? One way is to think about it from the perspective of an artifact as it moves through all these hands (c.f. Holtorf's "Life History of a Sherd" approach [2002]). Following an artifact allows me to illustrate the organization of people, because nearly everyone involved in the project touches artifacts at some point. Another narrative could follow the documents that are being produced here, but this would not illustrate the non-archaeologists so well. Only archaeologists interact with and produce texts, a point I will come back to below.

The maestro, with the contra-maestro at his side, excavates an area of soil in consultation with his archaeologist, who stands at the side watching and writing notes as the maestro works. Often archaeologists work in pairs, so one can be watching or helping with the excavation, while the other writes notes and fills in forms and tags. As the maestro carefully pulls his trowel over the ground, loosening and removing the soil in front of him, he pushes it to the side. The contra-maestro leans forward to help sweep the now loose pile of earth into the waiting bucket with a plastic dustpan. The bucketer is crouched a few feet away, watching intently and occasionally chipping into the conversation between the maestro and contra-maestro, or looking around him with some boredom. As the bucket is filled, he picks it up (putting an empty one in its place), and walks briskly over the space of the excavation, climbs up the side to the natural ground surface, and goes over to where his two screeners are waiting. Getting to their feet and interrupting a conversation they had been having with the two women at the adjacent screen, these two young *women pick up the large trowels beside them and steady the screen as the heavy bucket of soil is emptied into it. The bucketer stands for a few moments chatting with them, then remembers to turn back a number on a spiral of small, numbered cards nailed to the screen—a system of recording how many buckets of soil have come out of a particular area, and thus the volume of space removed. The women shake the screen and bash at it with their trowels, forcing earth through the quarter inch mesh and in the process creating a mound of loose soil that grows to a height of several meters by the end of the field season. When their field of vision is cleared somewhat, they start rapidly picking out fragments of bone, ceramic, and 'lithics'*[15] *with their fingers, putting these into three of the cotton draw-stringed bags.*

After a while one of their archaeologists comes over and collects the bags that are full, replacing them with empty ones that are already labeled with hand-written tags. The archaeologists exchanges a few words of greeting with the screeners, then heads back to a spot on the other side of the excavation unit where the archaeologists have left their backpacks and notebooks. Here is also a pile of small paper tags, and permanent marker pens. The archaeologist sits cross legged on the ground and spends a considerable amount of time filling in three identical tags for each bag, double checking the details occasionally on the clipboard of handwritten notes she has in front of her. She is joined by another archaeologist from another quad as she does this, and they pass the time both discussing their work and gossiping, as they do this tedious but essential task.

At the end of the day the bags are taken back to the lab by the archaeologists, and checked in against a master database that is presided over by Natalie, a NA archaeologist who

15 Lithics (worked stone artifacts) are notoriously hard for non-archaeologists to identify. As a result the number of actual lithics recovered as opposed to random stones and pebbles is very variable, particularly when the position of screener is rotated.

Olivia has appointed her main assistant and given a large amount of logistical responsibility. Olivia's aim is both to free up her own time, and to train Natalie to run her projects in the future.[16] *Natalie oversees the storeroom maestros, to make sure that they are keeping track of each bag as it comes in and out of the lab. The next day the bags of artifacts are emptied out by the washers, who dunk each artifact into water, scrub it with toothbrushes, then leave it on a tray to dry in the hot sun. After they are dried, the artifacts are sent in their original bags to the markers, who write a code number on each piece of bone and ceramic with a fine permanent marker pen, before they are bagged up again and taken to the store room. At some future point the storeroom maestros will bring these bags back out again to be analyzed by one of the archaeologists working in the lab, who will enter individual measurements for each ceramic shard or bone into a database. This, however, may well be weeks or even months from now, or in some cases the bags can wait in the storeroom for years, if there is no-one willing and able to do the analysis. On this particular project, however, the directors have graduate students lined up to work on all the material that year.*

In contemplating this complex organization of people and things, involving the circulation of hundreds of forms, thousands of tiny objects, and around 60 new people each week, Olivia joked with me one morning that it worked like "a well oiled machine". This became something of a catch phrase, seeming to capture the sense of precision and concentration required to keep everything moving along. But the machine or factory analogy is very fitting as a means of understanding the implications of the labor divisions this system. Everyone had a specific,

16 Olivia was particularly concerned with explicitly training her female NA graduate students to take on leadership roles, seeing this as a service her adviser had done for her in the past, and as a practical step that could be taken to overcome gender bias in the discipline.

hierarchically arranged role that fit like a cog into the bigger machine. This organization stands in stark contrast to the British example, with its focus on single excavators.

Given the immensely more complicated and hierarchical structure of labor, and the fact that North American Andeanists are only partially able to control this labor structure, what kinds of conceptualizations of the knowability of archaeological objects and the archaeologist as an expert knower, come out of this system? To begin answering this question we can look at *what* is given a number/form, but then I will turn to look again at how divisions of labor are both shaped by, and reflective of, the knowability of archaeological objects.

Forms and numbers

The archaeologists on the Tiwanaku Project used a recording method that combines forms and field diaries. The diaries were individual (each field archaeologist has their own and it moves with them if they work in a different part of the site) and are used to keep an on-going narrative documentation of the work being done. The forms are similar to the single-context planning forms used in the UK in that they consist of a single, separate form for each unit of analysis and prompt the person filling it in to record standardized measurements, descriptions, and a brief interpretation. They differ in that the unit of analysis is called a 'locus' and can refer to either an excavation event, an archaeological event, or an archaeological entity. An additional set of numbers are used that refer to 'features' and these are used to construct a diagram that is understood as being a Harris matrix.

The layout of the forms had changed over the years, as it does on many excavations in the region. If we take a genealogical approach to the director/academic adviser and graduate student relationship, the original versions of the forms could be traced back to great-great-grand-adviser's of the three directors through various branches of the family tree. But other innovations were much more recent and one of the directors was particularly interested in incorporating features from another project she had been involved in, as well as modifications suggested by an archaeologist who had worked on an unrelated German project elsewhere in Bolivia.

The forms were to a certain extent in flux at this period, but this was not unusual. On a related project in a different part of the region the members of the project met each year to revise and refine the recording and excavating methods, sometimes adding new questions to the forms or processes or taking away those no longer needed. The director of this project was particularly open to improving and refining the process, which resulted in a great deal of work but a system that had gained a reputation among Bolivian and North American archaeologist working in Bolivia, including among the directors of the Tiwanaku Project, for its rigor and attention to detail. The extent to which Andean archaeologists are willing to experiment and are open to flexibly changing their recording systems is another contrast with the homogeneous British system. But this more experimental attitude to recording still had to fit itself around the unchangeable labor organization and specifically the use of non-archaeologist workers to do most of the excavating.

In 2007 and 2008 several new features and number systems were added to the Tiwanaku forms, specifically the concept of the 'locus' and a Harris matrix, a combination that requires some explanation. As discussed above, the Winchester system involves a combination of the Harris Matrix, open-area phase excavation, and single-context planning: and it aims to record *archaeological events*. It also requires professional archaeologist excavators who are individually responsible for the interpretive act of recognizing/bringing-into-being archaeological objects. In the Andes excavation is primarily carried out by non-archaeologist workers and uses locus forms that are distinct from single-context planning forms. Within locus forms the distinction between excavation events, archaeological events, and archaeological entities is not clear. One of the directors, for example, argued that the novelty of the Harris Matrix and locus system was that it allowed one to leave all the interpretation of the site until *after* the excavation was complete, but this is the *opposite* to how the Winchester system was designed.

When I asked different archaeologists at this site to describe what a 'locus' was, I received a broad variety of detailed and nuanced responses that reflected on their own practices at this site and elsewhere. One of the North American graduate students who had not worked on the Tiwanaku project before, described being initially confused by the use of loci and features. It was new to him, but not so different that he couldn't make sense of it. I asked him to explain and he described how he used to dig in 'levels', which for him are areas of excavation—either arbitrary or natural—but which are not related to horizontal layers. A level is an area demarcated by the archaeologist: it can mean, for example, the NW area of the excavation unit as opposed to the NE area, or different sides of a wall. He explained that some people mistakenly assumed that levels are the same as 'layers'—that they refer to real stratigraphic layers in the archaeology—but this is not the case. He also referred to the system of 'events' as something he hadn't come across before but could see the point of. In his previous work, he explained, he only worked out the events and features at the end when writing the *informe* (final written report of the excavation, written after the field season is completed): so they were "purely descriptive" in the informe, a matter of describing them post-facto so that they would not appear in the forms at all. In this system, by contrast, the events would appear in the forms as you went along, rather than only in the writing of the informe. This archaeologist, then, was used to describing only *excavation events* and inferring both *archaeological events* and *archaeological entities* post-facto. It isn't clear, however, whether he saw stratigraphic layers in terms of Harris' conceptualization of the archaeological record as only containing *the material traces of events*, even though this term 'event' was being used on the Tiwanaku excavation form as a technical term/numbering system. Given that he would later have the job of writing the informe back at his university on the basis of the other excavators' forms and diaries, the post-facto nature of interpreting otherwise descriptive forms is notable.

I got a different perspective while discussing of loci with another archaeologist, Rob. I asked Rob what he considered a locus to be and whether he assigned locus numbers based on excavation events (i.e., 10cm levels, 'arbitrary' levels). Taking care to point out that this was only his own take on the topic and he couldn't speak for others, he explained that for him a new locus number can be used for something that *looks* like it may be the same as something already uncovered, but might be different. He used only arbitrary levels and locus numbers at the beginning, before it's possible to see things more clearly. It's sometimes the case that he would use arbitrary loci on things that he could tell already were only one thing, Rob explained, but that would depend on the object in question. I asked whether things get lumped together later on or while excavating, and he explained that "the moment it becomes hard is after things have been written down", so sometimes he will postpone making the decision and committing it to paper until he is sure.

I wanted to know when he would decide to put back together something he had decided was the same thing (i.e., how to signify that objects given separate locus numbers are in fact the same entity) if he changed his mind, and he replied that this does sometimes happen after, but not that long after—it depends on the excavator's personal preference. He sees loci as "exploratory" and later explained that loci reflect how people tend to excavate. Some archaeologists tend to use too many locus numbers, dividing things up too much—but he prefers to use less. At a later stage there are *event* numbers and to him these are the numbers that in effect represent 'archaeology' rather than 'excavating'. I asked when he decided about these and Rob explained that it happens later in the process, but ideally the excavator should be thinking about the events as they go along. "The event numbers are what lump the locus numbers together". Finally, as I asked about different ways of recording in forms and notebooks, Rob said that he uses the notebook to make notes as he goes along—the microscale of analysis—and tries to use the forms later to relate to larger stratigraphic moments.

Taken together these two conversations produced no single answer, and the tentative and sometimes confused explanations is partly what I want to convey. There was significant debate and uncertainty about what the units of analysis were, and in the first year the Harris Matrix was included in the recording process there was anxiety about the need to produce a diagrammatic representation of the site along the lines of a Harris matrix of 'events' by the end of the season. A parallel set of numbers had to be produced known as 'event' numbers, but again there was debate over whether these were meant to directly parallel the 'locus' numbers or were another level of interpretation of them that happened post-excavation. Neither locus numbers nor event numbers correspond directly to context numbers, as understood in the Harris-matrix + single-context planning model, although there is a sense that this is what some of the excavators assumed they were following.

The degree of flexibility in modifying recording methods and willingness to adapt or experiment was seen as an advantage—particularly because it had allowed the routine incorporation of various complex analyses such as phytolith sampling and archaeolobotanical sampling. But the variation in experiences coupled with a fast pace of work also bred anxiety. Beyond this particular project, many archaeologists I spoke with in the Andes and elsewhere expressed private doubts and uncertainties about their excavation methods. When I asked why methods are so rarely discussed, many suggested that perhaps they all secretly think that everyone else is doing it better, and thus are afraid of drawing attention to their own mistakes by raising the topic of conversation.

My intention in drawing attention to the recording methods at this one site, and particularly the use of the Harris Matrix, is not to imply that the Andeanist archaeologists are 'doing it wrong'. When seen in light of the willingness and ability to adapt and experiment with recording methods in contrast to the inflexibility of labor organization, the more relevant point is that one component of a complex technical system can be separated out and applied elsewhere (e.g., the Harris Matrix), without also importing the related technologies, the organization of labor and expertise the system relies on, and the ontological assumptions that underlie its use.

From labor divisions to the ontology of archaeological objects

Thursday, 10:35am. I am sitting on the side of the excavation unit. Olivia and James are standing behind me, and in front of us Camila and Trinidad are taking a level.[17] *A moment ago there was a short conversation in English about whether the surface was level—Olivia thought it wasn't, James said it was an optical illusion—and James had suggested putting in temporary datum points so that they could casually measure as they went along without always needing the total*

17 "Taking a level" means measuring the depth of something, usually in this case a surface. These measurements can be done using a machine called a total station, or more casually from a reference point, known as a temporary datum point, using string, a tape measure, and a plumbob.

station. Olivia, James, and I are all watching Camila and Trinidad taking these temporary levels. They are in the excavation unit, about four meters away and a little lower down from where we are all standing on the edge of the unit. Olivia turns to James and tells him that Camila and Trinidad shouldn't be taking the levels, the maestros should be doing it. James replies that they are just doing it now, to check, but Olivia says again that they ought to be having the maestros take measurements. This develops into a larger conversation about how the maestros should be taking all *the levels. "The idea is that the maestro and the contra-maestro should be doing all the things they can, to free up the archaeologists to do what only they can do." The archaeologists should only be doing certain tasks like writing bags and filling in forms, and the maestros should be doing all the rest to take pressure off the archaeologists. James walks over and steps into the unit to tell Camila and Trinidad this. (Although later on in the day I see the two Bolivian women continuing to take levels themselves.)*

Setting aside for a moment the manner in which work was *actually* conducted (for instance, Camila and Trinidad continuing to take levels above), what does the "machine"—the organization of labor, objects, and recording technologies—assume is the appropriate or possible way of knowing the epistemic object of archaeological excavation? An archaeologist in Tiwanaku was not conceptualized as someone who shoveled, carried buckets, picked artefacts out of a sieve, or regularly engaged a trowel with the soil. A good archaeologist was someone able to make decisions, to think quickly, who worked in an orderly manner while managing a large number of people engaged in a variety of tasks, and who kept on top of never ending bureaucracy involving the processing of bags, tags, soils and forms. As with the task of taking measurements, the delineation of roles could shift but tended to do so in the direction of an ever finer emphasis on the archaeologist as someone who writes and manages. The implication is this kind of labor system, a line can be drawn between the act of uncovering entities that have an existence independent of the observer and the act of making an expert assessment of these entities that recognizes and records their relationships to other material entities.

For example, the separation of artifacts from non-artifacts in the screen was conducted by rotating workers who receive *ad hoc* training at the beginning of the week. The fine mesh of a screen and the system of noting the exact volume of earth that goes through it, implies an organization and precision that incorporates the human eye and hand into its systemization. As parts of the machine, the rotating worker's eyes and hands become indistinguishable from the eyes and hands of anyone else, and unlike the maestro they need little to no training. The task of recognizing and selecting artifacts can be conceptualized as non-archaeological in the sense of being non-interpretative, and archaeological artifacts are thus conceptualized as self-evidently visible and knowable.

The way that an archaeologist and a maestro worked together illustrates this division between interpretive and non-interpretive labor further, by showing how non-artifactual objects (e.g., soil features like pits, walls, floors, etc.) are also conceptualized as inherently visible material things—but not necessarily knowable as *archaeological* objects. The majority of conversations between a maestro and his assigned archaeologist were framed in terms of changes in soil texture, color, or compaction. The understanding within this system is that where the maestro sees and describes changes in the earth, the archaeologist will see *archaeological* entities. Both the archaeologist and the maestro look at the same object and see different things, while *knowing and expecting* that the other person will see the same thing differently. There is an acknowledgment on both sides that the material objects are being conceptualized differently, and that *it is this difference in conceptualization* that makes one the maestro and the other the archaeologist.

For instance, the archaeologist gives an instruction to the maestro and this instruction encapsulates the way in which the archaeologist imagines the maestro interprets the earth: e.g. "dig 10cm down" or "dig out the red soil from the black soil". The product of the maestro's engagement with the earth then becomes "the soil 10cm down" or "the black soil". But the archaeologist sees this same physical thing as "level 2" or "a pit". The archaeologist's expertise and their ability to make archaeological facts comes not, therefore, from an encounter with the object (as is understood to be in the British system); but from the ability to conceptualize the object in a certain way once it has been manifest in its materiality by someone else.

Of course, in practice there frequently *is* discussion about where entities begin and end in both British and Andeanist archaeology. In the case described above, Trinidad disagreed that measuring could be separated out and given to the maestro as a non-archaeological task and instead continued to take her own measurements. She was thus claiming that she *did* need to see and hold the tape for herself, in order to understand the object being recorded. But the system at Tiwanaku is structured according to the assumption that objects have an inherent existence that is independent of the person attempting to see or measure them, so anyone in possession of a tape measure would record the same result. Moreover, an individual *archaeologists*' experience with any specific object or area of a site *should* be irrelevant, because archaeologists are also interchangeable: experience and training are not necessary if objects are inherently knowable. *Archaeological* expertise that might require training is a combination of management and organization skills (for managing the labor force and keeping up-to-date with paperwork) and knowledge of archaeological texts (the accumulated knowledge gained through formal education). Archaeologists thus do not need excavation training—and more to the point, their individual experience working in the region or on this particular site or unit is not a distinct necessity. Thus formally all archaeologists are not only equal but interchangeable and potentially increasingly mechanizable (as ever more aspects of their labor are given to other workers). The archaeologists are thus conceptualized less as skilled professionals and more as specialized cogs in the larger machinery.

In practice, however, there is another additional component of excavation practice that is not accounted for in the formal system. After talking with the maestro, individual archaeologists engaged in further conversations with each other but particularly with the directors who came by throughout the day to look and talk. These conversations could become much more fraught than the interactions with the maestro, or even with the object itself through the maestro. Through these conversations, decisions and interpretations were made while looking at and describing what could be seen, but rarely by touching, feeling, or manipulating what has been already made visible by the maestro. The process was visual and discursive, rather than manual or tactile. It also brought to the surface divisions among the archaeologists that were not accounted for or recognized by the structure of the machine, and that revealed more complex understandings of embodied expertise and professional trust than would be expected. Edgeworth's ethnographic description of British archaeology describes how supervisors would, over time, give more credence to the interpretations of the individual archaeologist who actually excavated a feature, because authority in that system is understood to reside in an embodied experience of the object itself.

In contrast, the conversations that took place in the Andes entailed layers of negotiations, compromise, and argumentation and the ability to draw on discursive expertise: having the authority to speak, to agree, to state as opposed to suggest was not a simple matter. Fault lines fell between archaeologists trained in different disciplinary traditions, some of whom are able to articulate themselves in ways that are heard and carried forward, while others are not. Being able to make authoritative statements requires a familiarity with the cultural expectations of how one talks, deports oneself, or socializes, how one marshals evidence by pointing at the ground, calling on previous experiences, or making citations within the conversation. Such familiarity comes from immersion and training in a particular disciplinary culture, bringing into focus again the extent to which practices in the field are related to the larger organizational structure and dicsiplinary culture of the archaeological communities out of which individuals come. In the following chapter, I will look in more detail at this ability to perform expertise in the field at this site.

Conclusions

There is no shared, universal methodology in archaeology and neither is there a shared conceptualization of what archaeological knowledge is and how it can be known. Methods, however, are developed in relation to complex histories and contexts of practice, including legal or moral regulation of the labor force.

The Andeanist model rests upon an understanding that archaeological objects have a finite, independent existence. Whether an artifact like a ceramic sherd or a spatial artifact like a layer of soil, the archaeological object as a physical entity can be unproblematically delineated from the surrounding material world by someone who has the ability to recognize differences in color and texture. Seeing the physical thing does not require the ability to know it as an archaeological entity. The act of interpretation that ascribes *archaeological* meaning occurs later: in talking about it, deciding its significance, and eventually fixing this interpretation in written forms and numbers. But it does not occur at "the trowel's edge", while using one's hands to touch, carve, and manipulate an object in the ground. The act of recognizing *archaeological objects* rather than just *material objects*, is tied up in the act of inscription—at the pen's edge. Further, the moment when the object's status is in flux and therefore where there is controversy over its existence, is not in the encounter between the object itself and the archaeologist, but between different archaeologists who come from different academic traditions.

This stands in contrast to the British model. The assumption underlying both Hodder's theory and Edgeworth, Lucas, and Yarrow's descriptions of practice, is that archaeological objects require an archaeologist to reveal them: the object is not inherently knowable as a physical thing, but nor does it spring entirely from the imagination or creative action of the archaeologist. Rather, it takes an experienced archaeologist to be able to engage with the object and negotiate it into existence. The common-sense notion that the only person who really *knows* the object is the person who excavated it, is fundamental to the insistence that *all* excavation work is archaeological—even the shoveling, troweling, and measuring—because physical engagement is a significant component of the exploratory process. Figuring out the shape and extent of a physical entity whose existence might be slippery, elusive, and shouldn't be taken for granted, *is* theoretical and interpretive.

What happens here to the status of the thing itself? The discussion of locus forms and diaries in Andeanist archaeology indicated that there is no consensus over whether the entities numbered, named, and fixed in forms are excavation events, archaeological entities, or archaeological events. Certainly the use of arbitrary levels and diaries is consistent with the highly hierarchical organization of the team: there is an expectation that workers and archaeologists alike will make mistakes and need to be monitored. The recording of excavation events reinforces this, although it is combined with the use of forms and diagrammatic representations of relationships that look similar to the Harris matrix and single-context planning forms. But the distinction between archaeological events and entities is maintained in some cases and not in others, and covered by the existence of another set of numbers (event numbers) that are assigned at later dates. The lack of consensus on this issue might cause debate and some confusion during the practice of the excavation, but this is not seen as inherently problematic. Anxiety about excavation methods exists, but it is not an epistemic challenge and archaeology as a discipline at the global scale can withstand enormous variation, from an epistemic point of view, because the process of arriving at the epistemic objects (stratigraphy—i.e., material relationships) is not as important as what is later done with them. Moreover, the tensions between the ideal model and the experience of practice can be bracketed out and blackboxed once the field is left behind.

Chapter Three

"To Stand There Silent": the co-production of archaeological experts and archaeological knowledge

Introduction: loudness, quietness, and the ability to speak

At some point in the middle of the 2008 field season, an uneasy tension pervaded the Tiwanaku Project. The pace of work didn't slow and the 'well oiled machine' was still humming along: the pile of carefully filled in forms grew and the shelves of the store room were packed with bulging artifacts bags. But there was an underlying feeling of discomfort among the excavators that never *quite* seemed to burst out as a genuine argument. The North American archaeologists joked and teased each other as much as ever, while the Bolivians generally remained quiet, serious, and apparently intent only on working.

This contrast in noise volume was commented on frequently—"The Gringos are so loud", "The Bolivians are so quiet"—although it was, of course, contextual. During a lunch break one afternoon I went with the Bolivian archaeologists to pay a visit to another archaeological project working in a different part of the town, and noted how one archaeologist in particular (someone I had barely heard talk before) was suddenly chatting, cracking jokes, teasing and slapping friends on the back. While still not as boisterous as the North Americans often were, the change in the Bolivians' demeanor was noticeable when we returned to our own project half an hour later and the sense of subdued silence returned.

Loud, boisterous behavior on North American archaeological projects in the Andes is part of a broader set of performances that I characterize as part of an *ethic of informality*—a sense that archaeologists, and in particular the community of North American Andeanist archaeologists, are fun, joking, brash, and have markedly non-hierarchical relationships with each other that are based on friendship. The constant characterization of themselves and their community as informal, makes it difficult for those who are unable to perform informality in an appropriate manner. They experience this inability as an unspoken hierarchy: one that is more difficult to critique precisely because its existence is denied by the constant discourse of informal friendship-based equality.

The loud, brash Gringo is as much a stereotype as the shy, submissive Bolivian, but these stereotypes do very particular work to make explicable differences that arise from inequalities that are otherwise hard to articulate. In this chapter, looking a little deeper into the expectations of what loudness and silence do and mean will allow me to make a connection between the tensions that arise between archaeologists from differently empowered communities of practice, and the question of how scientific expertise is performed within a discipline that relies entirely on the body of the scientist (or collectively, the *bodies* of scientists and workers) to bring into being its objects of knowledge. Ultimately this is a question about what it means to be a *person who is able to know* archaeologically in relation to the question of *what the epistemic object of archaeology is*.

To understand the significance and interrelatedness of these two questions, we first need to appreciate the specific problem of archaeological knowledge when considered next to other scientific disciplines. Namely, that archaeology does not rely on standardizable and comparable machines or technologies to produce its facts, but on groups of people who use their eyes, hands, backs, and memories to turn material objects encountered in field sites into written records and databases, which then become transformed further into texts that circulate.[1] The way these people are organized and structured on an excavation turns them into archaeology's knowledge producing machinery. It is people who become the 'inscriptive device' (Latour and Woolgar 1986 [1979]: 51) through which knowledge is produced. And like all inscriptive devices, the inner workings must be bracketed out—blackboxed—if the knowledge produced is to appear circulatable.

In the Andes, the organization of the machine requires two interlocking assumptions. The first is that archaeology as a discipline is meritocratic. Within the space of scientific practice, individual archaeologists are equal and judged according to their capabilities and actions, irrespective of their race, gender, class, or nationality. The second is that material objects (including spatial objects) and the relationships between them that are the ultimate focus of archaeological practice are inherently knowable. Material objects have fixed, easily definable boundaries, and the relationships between them can be unproblematically established by any archaeologist. These two principles structure the way that a field site is organized: in terms of the kind of methodologies adopted, labor relations, and the extent to which individuals are interpolated as experts. When there is debate about the correct definition of an object between

1 I have deliberately not referred to technologies like Ground Penetrating Radar (GPS) because they are not a habitual part of practice, and when they are used they do not replace the kinds of excavation processes I am describing here. The tendency to overplay the significance of what few complex and expensive technologies there are in archaeology is perhaps a desire to latch onto the security of non-human machines. Or perhaps, as some of my informants suggested, a fascination with 'expensive toys'. Some observers of archaeological sites have paid a great deal of attention to such technologies, but their use is uncommon partly because they are too expensive for most commercial and academic projects, and partly because they rely so heavily on geologically favorable conditions. In the Andes GPR is next to useless because of the unfavorable soil chemistry and the effects of altitude on the machines' accuracy. The Tiwanaku project, in collaboration with two others in the region, was experimenting with this kind of technology in 2008 and came to the conclusion that it would not be worth repeating.

two or more people, the machine requires that there *is* an inherently correct answer, and an inherently correct knower whose correctness will be discernible irrespective of their individually raced, gendered, or nationalized identity. Tension arises when reality contradicts these assumptions. For instance, when objects do not have clear-cut or easily definable boundaries, or when disagreements between colleagues are perceived to be judged according to that archaeologist's identity, rather than their experience of the object or professional expertise.

In this chapter I will explore the implications of the dissonance between what the machine requires/assumes and the experience of it in practice, with a focus on micro-practices at the field site. In doing so I will open up a debate about the extent to which a scientist's body is imaged to be a standardizable tool, the value of embodied expertise, and the invocation of a meritocracy in North American scientific culture, particularly through the ethic of informality.

Friendly and confident, silent but meticulous

As the tensions among the excavators grew, it was simultaneously apparent that not everyone was aware of it. A particular cause of unhappiness for some of the members of the team was the promotion of one of the North American graduate students to the role of supervisor. Early in the project the directors were worried that one area of the excavation was going far too slowly—the place where two Bolivians, Trinidad and Camila, were working next to one of the North American students, Rob. Frustrated with the slow pace of work, Olivia asked one of her North American students, James, to take charge of the area and act as a kind of supervisor to everyone else.

James had previously worked in this region but this was his first time on this particular excavation site, while several of the Bolivian students and Rob had worked on this same site for several years. When I talked to Olivia about her problems with the Bolivian students' pace of work, she made it clear that she didn't blame them personally, but instead their archaeological training. They are unable to make decisions, she said, and tend to be overly cautious about everything because they were trained on paleolithic sites where every tiny little thing is sacred and you have to work painfully slowly. Here she needed people who were able to make judgment calls quickly and efficiently, who had confidence, and could be more "aggressive" rather than "meticulous". Being aggressive is a matter of being able to make decisions about time, labor, and the archaeology with confidence, she said. The crucial skill was thus the ability to see what was important and act on that decision. The Bolivian students did not seem to be able to act quickly and decisively, and because she could not always be there herself telling them what to do, she was asking James to go over to make those decisions instead. She added another reason later, that James would be helping her write up the report when they returned to their university, so it was important that he knew what was going on over the entire site.

As she gathered the archaeologists together at the side of the site (away from the workers) and explained that they should now go to James if they had a problem, the Bolivians stood in silence, occasionally nodding. About ten minutes later the morning break started, and James came over himself. An incessantly cheerful man by disposition, he greeted the two Bolivian archaeologists and the workers sitting on the ground near them with a big smile and a greeting. Camila turned, and offered a deadpan "Hola Mister Supervisor" in a tone of voice that could implied it was a joke, but likewise could have not. James laughed a little nervously with a noncommittal "Yeah…" that faded into an uncomfortable pause. The conversation turned to whether there were enough candies to give the workers at break, but pretty soon James began asking them how they were excavating and giving them advice. Although uncomfortable with having his new position of authority pointed out to him, James agreed with Olivia's criticisms of how the Bolivians were excavating. His enthusiasm for the archaeology and professional curiosity made it almost impossible for him *not* to comment on how he thought it ought to be done better.

After a while Olivia came back over, and she and James began to walk around the site talking over the strategy for the current phase of excavation and how the two Bolivian women were working. I stood listening to the conversation, noting again how the ability to move around the site seemed to coincide with who people spoke to and about what topics. The ability to move from one's designated space of work appeared to map onto a sense of autonomy. Specifically, to stay in the same place showed that one was working hard at a designated task—a concern that implies someone else is watching and monitoring one's labor. My own ability to move was perhaps the best illustration of this. I had deliberately not taken up the offers to participate in the excavation as an archaeologist, to allow myself freedom to move around. Unlike the archaeologists or the workers, I could wander over to another part of the site, go back to the lab, or leave the site altogether to run water and chocolate errands for the archaeologists who were otherwise rooted to their excavation unit until lunch break. On this morning break, like many others, the workers sat still in circles on the ground smoking or eating candies, chatting among themselves, and passing around soda.[2] Olivia and James strolled around the now empty

2 The practice of giving the workers candies, soda, coca, and cigarettes at break times is well established and the cause of some discomfort. Some archaeologists actively object to it, although more usually because of the way it is takes place than because of the kind of things being given (e.g., sugar and cigarettes). Typically at the beginning of the break the archaeologist gets the supplies from their personal backpack, then puts a measured amount into the hands of each worker in turn. Often something like two candies, a handful of coca, a single

excavation space before coming to a halt in one corner where they stood discussing their strategy for this area. Meanwhile Camila and Trinidad remained in the same spot they had been working on all morning. Trowels in hand, they occasionally scraped the wall of the unit while talking about the immediate question of how far down this layer went and where they should stop digging.

A little while later, James literally enacted his new supervisory role as he stood with Olivia at the edge of the unit about three meters away from where Camila and Trinidad were working with their maestros. For a good ten minutes the other workers had stood waiting as the two Bolivian archaeologists discussed with their maestros an irregular black splodge in the soil at their feet. Having watched this abstractly for a while, Olivia suddenly noticed them still talking and turned to James, to tell him with some frustration that they shouldn't even be considering excavating the splodge but should be drawing it first. Without turning to the two women themselves (one of whom spoke fluent English and could presumably hear the conversation taking place a few meters away from them), Olivia continued to question, with audible annoyance, what the Bolivians were doing. When Camila stepped away for a moment to get something, James jumped down into the unit to give the maestros and Trinidad instructions from Olivia. After talking with them for less than a minute, he then returned to where Olivia had been watching. "What were they thinking of doing?" she asked with annoyance, shaking her head.

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cigarette, while the bottle of fluorescent-hued soda is passed around. When I first worked in the region, I also found this practice deeply uncomfortable. I decided to circumnavigate it at some point, by putting the bag of supplies on the ground in the middle of the circle and telling people to help themselves. After a few days I noticed that, rather than people now helping themselves to as much or as little as they saw fit, my maestro was now handing out the two candies, one cigarette, and one handful of coca instead. My action had been read as an abandonment of my duties, rather than a subversion of an infantalizing ritual. I later learned that other archaeologists had also tried and failed to get rid of this system in exactly the same way. It has been suggested to me by archaeologists who have worked many years in the region, that this kind of expectation of refreshment in return for labor, particularly in terms of coca and drink, has deep roots in Indigenous Andean understandings of labor obligations

On the flat surface of the excavation unit, this potential spatial object was distinguished by its slight difference in color from the surrounding soil. But the shape of it was indistinct, and it could potentially not have existed at all. Was it just a smear of black soil on top of the lighter soil, that would turn out to be only a couple of mms or cms deep? Where exactly were its edges, given that there was no clear line between the darker and lighter soil in some places, but more of a gradual gradation that you had to squint to see? Its shape was too random—not like a clear circle or a rectangle—meaning it could potentially be something non-human like an animal burrow, or worse could be a intersecting group of many spatial objects all crossing over one another and therefore probably impossible to separate out. The splodge had to be drawn, given numbers, and assigned a form before it could be excavated: actions that required a definite answer to the question of its extent, and ultimately whether it existed or not (i.e., whether there *was* indeed a black area of soil different from the soil around it). This had to happen before any soil was removed, to ensure that all soil sent in buckets to the screeners would be tracked with the right numbers, and the tally of buckets that provides a final measure of a spatial object's volume would be accurate. Only when its existence as a thing worth assigning a number was decided, could anyone start to use their trowel to pull the darker soil away from the interface with the lighter soil, to find out if it was deeper than a few centimeters, to see if those edges and interfaces really did exist. This was hardly an unusual moment, for any archaeological project here in the Andes or elsewhere. Spatial objects are frequently more ambiguous than the forms and system built around them.

The morning continued, and the black splodge was pondered, drawn, and carefully prodded with trowels. One of the other directors, Emily, joined Olivia and James as they stood on the edge of the unit watching Trinidad, Camila, and their maestros working a few meters away.

Emily joined in the conversation Olivia and James were having about strategy, that included heavy criticism of how Rob and the Bolivian archaeologists (still standing only a few meters away) had recorded this area last year. Sitting on the ground in front of them and filling in forms, Camila's face was blank. She made no sign that she was paying attention to the directors and her new supervisor discussing her failings a few paces away.

When talking in interviews about what makes archaeology different from other professions and sciences or how they would characterize their professional community, the archaeologists I interviewed described the informality, the friendliness, and the fact that archaeology was more relaxed and non-hierarchical than other disciplines. But this moment illustrated how information and instructions moved up and down a rigid hierarchy of positions and roles. Olivia and Emily gave an instruction to James, and he walked a meter or two away to tell Camila and Trinidad. They in turn told their maestros what to do, and later when a contra-maestro came over to ask "Señorita Camila" what he should be doing, she told him with some annoyance that he ought to go ask his maestro, not her.

Invisible and silent hierarchies

When I joined the Tiwanaku Project in 2008 my major research question had been the relationship between archaeological workers and archaeologists. I was interested in how the Aymara workers, many of whom had cyclically worked on related archaeological projects for many decades (Sammells 2009), were able to move between two apparently contradictory models of time and the past. Indigenous Aymara workers at this site are central to the day-to-day practices of excavation and interpretation. But they presumably would have fundamentally different ways of understanding the past and of interpreting the material objects encountered than archaeologists, that drew on their an indigenous historiography and epistemology (see Dillon and Abercrombie 1988, Abercrombie 1991, 1998, Salomon 2002, Lane and Herrera 2005, Silverman 2006 for discussion of Andean conception of the prehistorical past, and Sammells 2009 for this region of Bolivia specifically). How would these different ways of conceptualizing the past interact with or contradict the archaeological paradigm, during their participation on excavations? In asking this question I was inspired by the work of Clare Sammells (2009), who describes how people living in and around the archaeological site of Tiwanaku draw upon alternative conceptualizations of the past in different contexts. This question also relates to a central concern in archaeological writing over the last two decades, and one that has dominated the growing field of ethnography of archaeology. Namely, the relationship between archaeologists and indigenous communities (e.g., Hamilakis 2011, Ayala 2007, Ardren 2002, Derry and Malloy 2002; Breglia 2005, 2007; Kraemer 2008; Castañeda 2009; and Murray 2011). My intention was to build on Sammell's insightful analysis of those involve in the archaeological tourist industry, to consider the space of the archaeological excavation as a moment when two competing models of time and the past are brought together.

Shortly after arriving on site, however, I realized that the moments of contestation or conflict that I had expected simply didn't exist. Perhaps somewhat non-intuitively, the moments when people discuss *the past* during the practice of excavation are rare. History or the past are rarely invoked. The level of conversations is that of action and practice, objects and inscriptions: not that of conceptualizing or narrativising a historical past. Given this, it is hardly surprising that workers feel no tension in holding two separate conceptualizations of the past in their mind at the same time (if indeed switching between alternatives is problematic, which I am now not convinced is the case). Both archaeologists and workers repeatedly told me that the only arguments that occurred between them were to do with the prosaic issue of wages, not with the nature or content of the work being done. Furthermore, while archaeologists have been eager to record alternative narratives and interpretations of their work, and to draw the workers into discussing interpretations of the site more aligned with Aymara conceptualizations of time and history, the workers themselves have been extremely reluctant to see this as part of their job. Attempts by archaeologists to include the maestros and other workers in the interpretative process on this site and others in the region, for instance by asking them to record their interpretations in notebooks and video diaries, or to contribute local Aymara histories of the sites, have been a resounding failure.[3] Instead, tension between the *archaeologists* brewed beneath the surface and was experienced as casual slights and hidden resentment.

An unhappy sense of injustice in and of itself might be attributable to personalities and social unawareness on one particular project—hardly high stakes issues. Particularly given that it is not necessarily the case that archaeologists from different nationalities produce different knowledges. In other words, when archaeologists contest each other's written work, they do not refer back to the kind of disagreements about the micro-scale excavation data I am discussing in this chapter. But this tension relates to larger debates about the authority of scientific knowledge that is produced through bodies rather than through machines and the extent to which the specificity of those bodies must be bracketed out even as it is relied on. It also speaks to a particularly North American investment in the politics of meritocracy, and the way a commitment

3 That the relationships with local communities are *not* controversial or contentious goes against the assumed model, and I have found myself on several occasions having to defend this point when in conversations with archaeologists working in other regions, including Peru. I have also found it difficult to persuade archaeologists that this is not the major thrust of my research, given how popular a topic this has become. This says something about the common sense notion of power within the discipline, a topic I will return to at the end of this chapter.

to meritocratic and universalizing principles makes it easy to individualize inequalities that might otherwise be seen as structural.

On Andean excavations there is a clear division between the labor and expertise of indigenous workers and archaeologists. The indigenous people are treated by archaeologists with respect and their understanding of how labor relations work is built into the project's organization. For instance, the workers are hired on a rotating basis, practices like handing out refreshments at break are adhered to, and a rigid horizontal and vertical organization based on the Comunidad's own rank and zona configuration is applied to all work teams. The archaeologists, in contrast, are expected to be neutral—a culture of no-culture, to draw on Sharon Traweek's discussion of a related phenomena in physics (1992). Archaeologists are not understood to be significantly distinguishable in terms of their nationality, class, or race, although to an extent attention is paid to gender. In parallel, the materiality of objects is meant to be easy to determine by workers and archaeologists respectively. Problems arose when these two assumptions were experienced as false. The machine stalled when the black splodge could not be easily identified as either a material object or an archaeological entity; but also when the Bolivian archaeologists felt their level of expertise was not being judged according to their skills and experience, but because they were Bolivian.

The expected clash—between indigenous and archaeological knowledge and expertise—does not occur because there is an expectation of difference. Within a discipline already primed for liberal notions of multiculturalism, such difference is to be welcomed and incorporated. Difference that potentially requires work to identify, requires consideration of one's own privilege, and potentially undermines a meritocratic desire for equality and a commitment to universalist science, is much harder to see and harder to live with—particularly if the field is *meant* to be a place of relaxation and fun, not a place where one has to be engaged in the difficult work of self-critique. To understand this, we can look at moments where there was explicit tension between invocations of friendliness and the enactment of hierarchy, between knowledge advanced through relaxed, free-flowing, collegial conversation and through formally written, diligent, personal note-keeping.[4]

Speaking in ways that are heard

The tension surrounding James' position as supervisor grew over the weeks of the project. Two particular incidents illustrated how this tension was both hidden from the North American archaeologists, and complicated by a subversion of the distinction between loudness/childishness and silence. The first happened on a Friday afternoon at the end of a long week. The Bolivian archaeologists Camila, Javiera, and Trinidad were sitting on the ground by the excavation unit as they tested samples of soil for color and texture, recording the results in notebooks and forms. After a while James came over looking for a pen for the camera white-board, and asked if they had seen it. The Bolivians said they hadn't and he walked off, but then Camila looked around and

4 A few words about gender are also necessary here. The examples I have chosen to focus on in this chapter involve female North American directors and male North American archaeologists, working with female Bolivian archaeologists. Gender is of course a component of their experience, but in this example it was not the most significant axis of inequality for the Bolivian archaeologists. In other parts of the site there were different configurations of gender—for instance in the lab, the North American equivalent of James was a woman overseeing several female Bolivian archaeologists. Similar tensions were being played out in that space, and between this woman and the Bolivian archaeologists described in this chapter. In other parts of the site other male and female Bolivian archaeologists were working, whose experiences I am also not discussing here. There is insufficient space in this chapter to discuss these other configurations of gender, or to do justice to the kind of in-depth analysis of gender on this and the other sites I look at. A point of interest, which I bring up in the next chapter, is that the North American archaeologists on this and other sites were very aware of gender in their work, and actively looking for ways to counteract machoism and male bias. The Bolivian students, however, experienced gender as a far less significant form of oppression than that of North-South inequality, something that the North Americans did not see. It is this inability to see, that I am concerned with discussing in this chapter.

noticed it was behind her. I was not sure at this point if she had known it was there all along, or only just realized it—but after she found it she decided to hide it from James and tell him that none of them knew where it was. James came back again still looking for the pen, and in a perfectly straight voice someone suggested that perhaps Rob had it instead? A third time James came over, and this time Camila pretended to find the pen hidden behind a bag. James joked that he had been about to kill Rob for losing it, and with placid smiles the Bolivian women feigned innocence and lack of understanding.

Such an apparently petty and childish trick contrasted with how critical the Bolivian students often were of the Gringos' "childish" humor. About half an hour before this incident James had come over to get something, and as he left he stuck his tongue out to blow a raspberry at the women as a joke. They had watched with blank expressions as he walked away. Camila raised a single eyebrow. "Just like a child," she said in English, before shaking her head and turning back to her work. His joking antics had clearly not impressed them in the slightest. But a few minutes after the pen incident, one of the women, making little balls out of the soil she had been sampling, pretended to aim one at Rob's head as he sat with his back to her a few meters away. Looking over at me sitting nearby, she giggled, and whispered that I should aim one too.

Such petty games were literally taking place behind the backs of the North American archaeologists. Given the North Americans' belief that the Bolivians were naturally quiet and very happy working on the project, I have no doubt they would have been very surprised if they had known these kind of secret sneers were occurring. But while this is reminiscent of the "making out games" of playful trickery and parody that Douglas Foley (2010: 114-10) describes students and teachers playing in classrooms, which ultimately enabled a negotiation of authority and work norms, these games were both half hidden and half not seen because they *were* a direct challenge to the accepted frame of behavior. The normal frame was that Bolivians performed quiet seriousness, and the North Americans performed loud, playful clownishness. These momentary incidents of pettiness broke that frame, and suggested that potentially *all* performances of quiet reserve were in fact an active repression of frustration and resentment. Camila's willingness to let me see her engage in 'childish' games was an invitation to reassess the distinction between loud/childish Gringos and silent/serious Bolivians as an active performance. Thus both the behind-the-back sneers *and* the performance of apparently docile silence can be seen as acts of resistance that call to mind James Scott's (1990) concept of hidden transcripts. To what extent such performances helped or hindered Bolivian students as they tried to gain authority within the excavation, will be discussed further below. But later in the afternoon after the pen game above, another incident quite explicitly illustrated the kinds of casual, passing slights that were contributing to this undercurrent of anger. An anger only able to express itself in silence and small acts of hidden backlash.

Tuesday. 11:20. Two of the directors, Chloe and Olivia, and touring the site. Near the end of the tour they go over to Rob's area where he has a prospective burial in the corner of the 2m by 2m area. Both Trin (a Bolivian graduate student) and Rob (a North American graduate student) are now working here, and before Rob arrived it was known only as "Trin's Area" - but now Rob has joined her it is only known as his. Rob, Olivia, and Chloe stand around the burial, while Trinidad is about a meter away just in front of me. The maestros and other workers are gathered around watching and listening as well. The three North Americans discuss in English how it is probably a baby and what the strategy they ought to use to remove it, given that it is positioned in a tight corner. The strategy must take into account the concerns of the director who is an *osteoarchaeologist and who will later be looking at the bones, but she is less involved in the conversation than Rob and the other director, who both want to be able to see if they can work out the position the burial was placed in and the larger stratigraphic relationships of the area itself. The conversation is conducted in English, while everyone around watches, including Trinidad. The two directors are bilingual and have a high degree of fluency in Spanish, Rob is reasonably able to speak and follow Spanish, while Trinidad speaks very little English.*

Trinidad has been working in this area all morning before they arrived and like everyone she has been excited about the discovery of the first burial. As they discuss strategy she watches intently, but she remains outside the conversation. Occasionally she makes a noise or a movement forward as if to add something, but she doesn't. Sometimes she nods or gestures, but they do not notice her doing so at all. She is watching them with an expression of intense concentration as she tries to follow and join in the conversation. The two directors make explicit efforts to ask Rob his opinion and to understand both his perspective and what he wants from the exercise but they do not ask Trinidad anything or turn to face her at all. After a little while, James comes over and stands next to Trinidad. Olivia immediately turns around and asks him in English what he thinks. Trinidad moves her hand and leans forward as if to say something, gesturing (in Olivia's direction, not to James who is standing next to her) to where she thinks the much discussed cut should be placed. But James has already started talking and as he does so he steps forward to the group.

At this point some measurements are being taken with the Total Station in the 2x2m unit behind them, so Chloe has to step out the way and soon Rob, James and Olivia do so too. Chloe comes to stand next to Trinidad. Leaning over Trin's shoulder, Chloe asks her in Spanish what she thinks they should do and gets her to explain why. Trinidad tells her at once, but by this point the *decision has already been made by the others so Trin's point of view is superfluous, even if it agrees or disagrees. Chloe listens, but stays only a few moments next to Trinidad because the others are now all moving off to another part of the site. Olivia confirms with Rob, again in English, that he now understands and is sure what he has to do, and he says yes. As the others walk off, Rob and James carry on talking, confirming that they agree on the new plan. James then goes off to the total station, as they have realized the line of string marking the 2x2m square is a bit off, and Trinidad steps forward to confirm with Rob in Spanish what they are doing. As the others have left, Trinidad gives the maestro Nacho his instructions regarding the total station while Rob stands at the side. The conversation about the 2x2m line being off was in English though, so while Trin is instructing Nacho to take a measurement from where the cut for the burial is going to be, this is not actually what James has gone to measure. Trin is giving Nacho instructions in Spanish, and James comes over the radio telling Nacho to move quiet a large distance, which confuses him because it doesn't match the instructions Trin had just given him. Trin tells the other maestro to get a tape measure to help Nacho, but meanwhile James speaks over the radio asking Rob to confirm that Nacho is in the right place. Rob at this point comes forward and takes part in the whole operation.*

During this incident Trinidad had been excluded entirely from the conversation, even though the other people present spoke good Spanish and knew that she herself spoke little English. Her expression and body language, and the intensity with which she had been trying to follow the conversation and make gestures to include herself within it, had not been obvious to the others in the group. But it was itself unusual that she had not been asked her opinion, given she had been working in this place all morning and excavation areas are usually considered to be assigned to the responsibility of only one archaeologist. This incident would appear to contradict an interpretation of her exclusion in terms of timidness (which fit the characterization of Bolivians as shy and quiet), or unwilling/unable to make decisions. Trinidad had been excluded—and probably quite unconsciously—even when she had made tentative gestures to take part, and the conversation had automatically put her at a disadvantage because it had been conducted entirely and unnecessarily in English. While other instances of selective language use on site were to hide part of a conversation from someone present—for instance, switching to English to discuss workers while they were present, or in the case of the workers, switching to Aymara around the archaeologists—in this case the use of English was a matter of probably quite unconscious comfort. The North Americans were *simply not thinking about* the Bolivian non-English speaker, when they discussed something she was working on.

Discussing this incident with a different Bolivian archaeologist shortly afterward, she gave a rather cynical shrug and began to talk with some passion about how the Bolivian students felt themselves continually shut out of these kinds of conversations. This, she said, has led to a great deal of anger and resentment, and was something that they often talked about among themselves—not just the Bolivian archaeologists on this site, but those who worked for other gringo projects too. "Gringo's don't care about a Bolivian's opinion", she said. When she had first started working on the site, she said, she had tried to offer her opinions to the directors and talk to them about strategy and interpretation, but she felt like she was ignored or dismissed each time so now she doesn't bother. Whenever a Bolivian archaeologist put forward an idea it was ignored, she argued, so now they didn't even try. Some of them were particularly upset about the fact they had been working at this site for several years, but *still* their opinion was considered less important than an North American who had just started working there that year (i.e., James being promoted to supervisor in his first year). They were still dismissed when they tried to express themselves, or ignored from conversations completely. What came first, I asked, that the Bolivian students were too timid to put forward their ideas, or that the directors never asked for them? She laughed a little bitterly and said she didn't know. But then asked me—how could they put forward ideas that disagreed with the director? How could they persuade her? What about Amanda, I asked, the Bolivian archaeologist working in another part of the site who the directors often listened to and appeared to trust to work independently? She shook her head and told me that Amanda was only listened to because she was the only archaeologist working over in that area. If James walked by, Olivia would ask him what he thought first and Amanda too would stand there silent like the rest of them did.

The phrase to "stand there silent" seems to express much of what is happening in these interactions. This sense of exclusion is not just a matter of being able to speak a foreign language. Several of the Bolivian archaeologists spoke good or fluent English, and still "stood silent". Likewise most of the North Americans spoke good or fluent Spanish, but in both formal and informal settings chose to speak almost entirely in English unless they were directly addressing a Bolivian for a particular reason, for instance to ask them to pass something at the dinner table, or to communicate formal instructions about the day's plans.

Notably, this is in contrast with another excavation in Peru I visited, where the North American co-director insisted that his team of both Peruvian and North American archaeologists spoke Spanish in both formal and informal settings. This project was unusually integrated for this region, and informally the North American undergraduate students commented that it was difficult for those who did not have good language skills, but they had made more friends than they expected. This director briefly came to visit the Tiwanaku project, being a good friend as well as a colleague of the directors. Arriving late at night with a long tale of a terrible journey, as soon as he noticed that there were Bolivian students in the room as well as Gringos he switched into Spanish, and continued despite others slipping back into English. For the first time that season the evening conversation included everyone in the project, both in terms of technical discussion of the sites in the region as well as the usual practice of telling archaeological anecdotes of mishaps and adventures. Or at least, everyone followed the conversation and felt part of it, even if they didn't participate. The Bolivian students stayed in the communal dining room much later than they usually did, and talked later about how friendly and approachable this director was in comparison to other Gringos.

Talking with North American archaeologists about these experiences, and particularly the use of English over Spanish, the response was often that field work is exhausting. At the end of the day, you don't want to make any effort. Trying to talk in Spanish when you are more comfortable in English, particularly with people you don't really have much in common with, is really hard! And field work is *meant to be fun*, they emphasized. It's meant to be a time for joking around with friends, relaxing, enjoying being casual and laid-back. As one of the North Americans put it, the Bolivians are probably more comfortable socializing on their own, and we just feel more comfortable with each other too. The director from Peru's effort to cross the language and cultural divide, was an effort few consider worthwhile or necessary to make. This observation occurred only when I drew attention to it during an interview, however. The social isolation of the group was rarely seen as a problem otherwise, although my analysis suggests that this commonsense conception that Bolivians are quiet and formal and therefore happier not to socialize with loud and playful North Americans, directly affects the way they are also able to perform expertise on site, in such a manner that North Americans will recognize it.

The machinery of the excavation is built on the understanding that all such cultural concerns can be bracketed out. An archaeologist is an archaeologist, and their ability to act as one within the parameters of the excavation is independent of their otherwise 'cultural' nature: their race, gender, nationality etc. All archaeologists will be listened to, and have their assertions judged, on the basis of the correctness of their truth-statements, not their individual identity.[5] Archaeologists were automatically assumed to have the ability to excavate and interpret archaeological entities, even when they came from a different educational backgrounds and disciplinary cultures (see Chapters 5-7), or had different language skills. Or if they were socialized into different understandings of how one acts around professors or employers, according to intersecting class, gender, race, and nationality identities. In practice, the inability of lower-middle class Bolivians to act like North Americans does indeed mark them as non- or less expert.

It might be tempting to dismiss the incidents described above as examples of individual pettiness or larger cultural differences between North and South Americans. It is also important to emphasis that the interpretation of such events is not necessarily clear cut. When discussing these incidents with other Bolivian archaeologists—both those involved in this project and those who had never met the principle actors—there was agreement but also disagreement with the interpretation of these particular interactions by these students at this site, although they generally tended to believe that the scenario was plausible, based on their experiences elsewhere. But my overarching intention in describing these incidents is to illustrate the *messiness* of the social context out of which archaeological knowledge is being produced, precisely because this

5 The exception to this was gender. On this site the directors paid attention to gender differences, and made decisions that took into account the historical under-recognition of women in archaeology.

messiness is nearly always bracketed out of consideration as part of the knowledge production process but in fact constitutes a significant proportion of *practice in the field.* On the one hand, the informality and non-hierarchical nature of archaeological communities is held up as a central ethic, but this ethic does not translate into critical attention to whether it perpetuates structural inequalities. To ignore structural inequalities such as gender or race that are always already in existence within wider society, to pretend that they can be ignored by not addressing them, is itself a form of structural violence. This messiness also contradicts the assertion built into the machine, that all archaeologists are equally standardizable tools through which standardizable and comparable knowledge objects can be produced. To illustrate this connection between expertise and knowledge products on Andean excavations, we need to look in closer detail at how each are made.

Making material relationships through talk

The ability to speak Spanish or English is important, but more significant is what speaking one language or the other implies. Trinidad was excluded from the conversation because she couldn't speak English, but the fact that it would have been easy for the others to speak in Spanish suggests that this was not a simple matter of language choice, but a matter of not remembering or considering that she *should* be included to begin with. After all, plenty of incidents occurred like the one described above, where the directors stood in earshot of a fluent English speaking Bolivian student and talked critically about his or her work and questioned what they were doing, knowing that they would be able to hear. The question of language fluency is significant, but the director from Peru was doing more than speaking in Spanish—he was also looking at, addressing questions to, and responding to comments made by Bolivian students in an informal setting, and about topics over than basic practicalities.

The ability to speak as opposed to standing silent requires more, therefore, than a simple command of Spanish or English. It requires an ability to speak about certain topics in certain ways. On the one hand this is a matter of socialization and perceived cultural difference ("Gringos are so loud, Bolivians are all shy") that cut across interactions between North and South Americans, but are manifest in particular ways in the kinds of close living and working conditions that field excavations require. On the other hand, there is the matter of hierarchies based on perceived expertise within archaeological practice, from which we can extrapolate beyond the case of the Andeanist community.

Moments such as those I have been describing draw attention to differences at this site in the kind of interpretations that are being made, in relation to *particular kinds of people's* engagements with concepts and objects. In other words, there was a hierarchical relationship between particular kinds of people, and the sort of evidence (objects, material relationships, textual and non-textual discussions, references to other sites and experiences) that they would be able to marshal within a conversation. On this site this could be roughly organized as a division between directors, senior archaeologists, junior archaeologists, maestros, and workers. The division between junior and senior archaeologists is an etic category here, not one that was articulated by my informants—the unspokenness of this division is precisely the larger point. The degree to which Bolivian archaeologists were able to position themselves (and be recognized as) senior archaeologists was the crucial source of tension, in concert with the fact that it was an inarticulable and hierarchical division.

To illustrate, let us look at an example of a chain of conversations and actions surrounding an interaction with a new spatial object. A maestro sees a change in the soil and calls over an archaeologist. The maestro possible interprets it in terms of what it is *archaeologically*—an ash layer, a wall, an adobe brick—but he more usually refers to it in terms of the *physical properties* of the change in soil in contrast to the soil before or around this new thing. Here it is darker, there is more ash, this feels harder than what came before. The maestro tells the archaeologist what he sees and feels, and the archaeologist is then required to confirm that this is indeed a soil change. At this moment the archaeologist interprets the same physical thing in terms of a 'feature': an entity that has archaeological significance through its material relationships to other things, and something that will be assigned its own number. The archaeologist conceptualizes what is now an *archaeological* entity in terms of its relationship to other archaeological entities around it (for instance, it is inside or outside the wall, it is older or younger than the feature next to it), and will ultimately record these relationships in written forms and diagrams. The forms and diagrams will assign different archaeological entities[6] locus numbers, but also position these numbers in relation to other numbers. The positioning of these numbers marks their relationship to each other—i.e., the forms ask the archaeologist to specify whether this locus is above or below, similar to or different from, other loci. Similar sets of numbers group artifact objects: so for instance all the ceramics found in the locus will be bagged together and each ceramic will then carry the individual bag number, and the locus number, thus assigning a relationship between all those ceramics and each other, and the ceramics and the soil of the locus.

Thus although archaeology deals with material objects like soils and ceramics, these are

6 Or excavation events, as on this site there is some confusion over whether the forms record entities or events—see previous chapter. For the purposes of this discussion, I am glossing over this distinction.

not the object of enquiry and these are also not *archaeological* things: the epistemic object for archaeology is the *relationship between material things*. Archaeological knowledge comes about through the recognition, collection, recording, and organization of *material relationships,* not material objects. The process of excavation is thus not a transformation of the material world in an ontological sense—excavation does not change the nature of an object, which exists in a state independent of the act of observing or interacting with it. But the object becomes *archaeological* through the way in which it is *perceived and understood as archaeological*, by archaeologists.

The maestro talks in terms of a perception of changes in the physical entity—e.g., the soil here is slightly redder, and this is where the division can be discerned. But while the ability to see these divisions may be recognized as skillful (there is some acknowledgment in practice, that not everyone has the ability to see and distinguish these differences), he is not perceived as seeing the thing as an *archaeological* entity (c.f. Galison 1997). Both the archaeologist and the maestro look at the same thing and see it differently, while knowing and expecting that this is the case. The archaeologist is the one who perceives the red soil as an *archaeological feature*. Moreover, the single material relationship (this soil is a different color to that soil) is not particularly significant until it is tied to other material relationships (many instances of reddish soil in particular shapes and depths, that are found with assemblages of ceramics and bone, and cut through other features). This could perhaps be described as assembling 'bundles' of relationships, each of which is small and relatively insignificant on its own, but meaningful and archaeological useful when gathered together. The bundle of relationships that includes the reddish soil in relation to the dark soil, the ceramics and bone fragments in relation to the reddish soil: the archaeologist can now refer to this as, for example, an 'early intermediate period trash pit'. The act of tying all these bundles of material relationships together is what makes the work and skill of the archaeologist different from the work and skill of the maestro. It requires knowing about and making connections to other instances of similar relationships elsewhere.

The distinctions between archaeologists come, in part, through the ability to access and draw into a bundle relationships that are more remote. The next level of connecting material relationships occurs when the director comes over, or—less frequently—when something dramatic turns up like an articulated llama skeleton ('an offering') or a clump of adobe and stone ('a perimeter wall'). For more significant or dramatic features like these, the archaeologist themselves may begin to talk in terms of this level of connection but normally it only happens when the director comes over. These kinds of conversations only occur between the director and more senior archaeologists. These conversations draw on different kinds of data—published papers they both have read, experiences of other excavations in the region, the director's previous excavations on the same site or the archaeologist's remembrance of what they were doing here the year before. But this is specifically a hierarchical arrangement. When the maestros have a question they invariably go first to the archaeologist they are working with more directly and never straight to the directors, and when workers or contra-maestro have a question or concern they turn only to the maestros.

The following roughly characterizes the kind of conversations that occur, and the kinds of evidence that are being drawn upon.

Table 1: Forms of evidence and types of interpretation by role

Person	Evidence drawn on	Type of interpretation
Maestros	Changes in soil texture, compaction and color	Changes in soil. Some features recognized.
Junior archaeologists	Locus numbers, features, relationships between features and areas, their own excavation experience in the same unit in previous years.	Relationships within the unit, information that will go on forms and is directly related to recording such as the separation of bag numbers and how to fill in the forms.
Senior archaeologists	As for junior archaeologists, but during conversations with directors will also draw on published works read in classes, other published works or previous excavations.	As for junior archaeologists, but will also talk with the directors about bigger questions and the unit as it relates to other units or to the survey.
Directors	The unit as immediately seen but more directly as described by senior archaeologists; published papers by colleagues in the field; their own previous excavations in the area; colleagues' excavations in the area; the research proposal for the project, future plans.	Level of the site and of the region; of the general research questions of the project; interpretations in terms of the past and past social organization/structure; the report to be written, and what should be done in terms of the future research directions/questions.

The maestros interact with the most basic of facts: uncovering/recognizing a material entity's existence. The junior archaeologists are able to make connections between these material entities and others, thus making them simultaneously into archaeological entities and more significant knowledge objects. The senior archaeologists and directors tie these bundles of relationships to others that might not be apparent on this particular excavation, thus making more and more significant statements that stretch beyond the single site into the larger region.

This cannot be characterized as a ‘hermeneutic circle’ (c.f Shanks and Tilley 1992: 103-111) with constant circular movement between data and interpretation, but more as a chain of command. Instructions and directions are sent down the hierarchy, while data and descriptions are sent up. Records are usually seen as a way of fixing information for the future, but they also serve to pass information *up* during the excavation. In practice however, interpretations by the directors are being made acted on in the moment, long before written records are examined back home in their university office. Directors’ information comes not from the paper work or even the bags of artefacts, except in an informal, occasional sense. Instead they learn what is happening from the *conversations* they have when, periodically through the day, they leave their field office and walk out to tour the site.

Thus before material relationships are fixed through forms, they are created through conversation. Engaging in conversation relies as much on the ability to move, as the ability to talk. As I described earlier, the ability to move around the site during the day relates to a person’s position in the organizational hierarchy, and echoes concerns about relative autonomy/monitoring. Directors walk everywhere. Senior archaeologists (like James) can walk with the directors around any of the excavation units. Junior archaeologists (like Trinidad and Camila) stay close to their unit or the excavator’s bag-processing area, even during break-times. Maestros and other workers stay in the unit or space where they are working.

The directors make a point of walking through the entire excavation every day to visit all the archaeologists, check in with them, and talk about what they are doing. The directors ask to see new features, and through conversations with the senior archaeologists they make interpretations and plan what to do next. As we saw from the exchange the director Olivia had with James, while watching Camila and Trinidad excavating the black splodge, the director talks *with* senior archaeologists, but *about* junior archaeologists. As the example of Trinidad being unconsciously excluded from one of these conversations demonstrated, the distinction between junior and senior is not a conscious or acknowledged one, but nevertheless exists.

Informal conversations, about what the archaeologists think they are doing and about method/strategy, also occurred away from the site of the excavation however, and specifically at meal times. Informal 'shop talk' conversations at meal times are the space where connections, strategies, and decisions are discussed and decided. In this sense bilingualism was very important, as discussed above, but not the only factor. At meal times the group split neatly down the middle of a long table, with myself and a North American archaeologist called Sarah almost invariably being the ones in the middle of the language divide.[7]

On one occasion Olivia and James happened to be sitting next to each other at lunch while Rob was sitting some way away, having arrived too late to sit in his usual place next to them. Hearing Olivia ask James what was happening in an area just opened, Rob pitched in from across the other side of the room. Three Bolivian archaeologists were sitting between Rob and Olivia, two of whom spoke no English. They had also been working in that area that morning, but none of them were included in the conversation. Rob seemed frustrated at being stuck at the 'wrong end' so raised his voice and leaned over the table, past the three Bolivians eating their lunch, to make sure he could still take part in the conversation at the other end.

The interpretations that directors make on or off site override those of junior archaeologists, even though the archaeologist is the one who has the most direct experience of the object including physical interaction with it (rather than looking at it, or talking about it). For the

7 Sarah told me at some point that she made a point of sitting in the middle and socializing with the Bolivians as often as the other Gringos, but she was unusual in doing so.

junior archaeologists excluded from these conversations, resentment builds. Their sense of *being there,* of having spent the time carefully and painstakingly coaxing a feature from the ground, gives them a sense that they understand it better. Their experiential knowledge of a specific object, however, is less privileged than the director's ability to sum up the object on sight or while hearing it described during a conversation.

It is here, then, that tension on an excavation project builds. The junior archaeologists on this particular site were Bolivian and they perceived that their exclusion from conversations was due to their *Bolivianness* and was therefore unfair. This may or may not have actually been the case—for instance another Bolivian archaeologist, Amanda, explicitly described the Tiwanaku project as one where her voice was heard, when discussing her experiences working for foreigners.

> Amanda: I think they see us yeah as, um.... um... how do you say? er... the opposite to evolutionary?
>
> Mary: Backwards?
>
> Amanda: Unevolutionary or...
>
> Mary: Backwards? Primitive?!
>
> Amanda: Yeah yeah. Yeah! they saw us-- er, if you will-- so,
>
> Mary: The [European directors you worked for] or..?
>
> Amanda: Many! Many people yeah, because even if you have a degree here, they don't consider it a good thing. But if you get a degree outside, or-- they listen to you. And I saw it-- many people tell me that in a rude way. They see me as ... a construction worker, or something like that. They don't see me as a...
>
> Mary: A professional?
>
> Amanda: ...or a person, or a part...

> Mary: A part of the project, yeah...
>
> Amanda: ...or something like that. They only are, ‘Ok, here are your tools’, and--
>
> Mary: ‘Go off and do it’!
>
> Amanda: And they check me! All the time! Every five minutes. Erm...
>
> Mary: So, so they don't respect your skills?
>
> Amanda: Yes. Exactly. Well! It’s ok to check. But I feel in this project more respect.
>
> Mary: In this one? [Meaning on the Tiwanaku project]
>
> Amanda: Yeah-- I don't know if it’s because I am mature now. Um, but... [Olivia] always asks you for your opinion.

Thus Amanda saw this particular project as a place in which she was judged more fairly, according to her skills and experience. But this was still understood in relation to an otherwise familiar pattern of being treated by foreign directors as less skilled, as just a ‘construction worker’, and someone whose opinion was less valid because she was Bolivian. This is significant because it points to a tension that goes beyond this particular case study and indeed beyond the details of whether or not—in these particular examples—archaeologists were being evaluated according to their nationality. (Moving beyond the impulse to arbitrate these particular examples is additionally important because these are, by necessity, sketches drawn to illustrate a broader ethnographic point, rather than fully fleshed out case studies. Notably absent are fuller details or discussion of the relationality between nationality and factors like class and gender, which I have not had space to discuss here but were undoubtedly also involved in these specific examples.) The important point in this discussion is that a situation wherein a Bolivian archaeologist’s claims to expertise would go unrecognized was a) considered to be plausible and b) was resented.

Tensions arose because of the misalignment between the logic of the 'well oiled machine' and the reality of lived experience. The excavation project as a complex organization of labor, methodological strategies, and epistemic objects rests upon an understanding that material objects are inherently knowable and all archaeologists are equally able to infer epistemic objects from these material things; a supposition supported by the ethic of informality and the principle of meritocracy that are foundational to the disciplinary culture. Disputes over interpretation thus *ought to be* rationally and amicably arbitrated by drawing attention to visible, material facts, laid out on the ground and discussed during informal conversations among people equally able to interpret what they see. In practice the boundaries and material properties of objects—even their existence—is a matter of debate, and not everyone has the ability to speak in ways that will be heard or respected.

Watching the kinds of interactions described above, I noticed that when the Bolivian archaeologists felt excluded from conversations taking place in English among the North American archaeologists and directors, they would turn back to their notebooks and forms. There was a sense that although they were excluded from the informal conversational arena where decisions were being made, they could turn back to the act of formal record keeping. This was a process carried out silently, but one that marked their voice in the long-term and more permanent conversation—if indeed their notes were taken into consideration at a later date, given that they might contradict the narrative that had been established during the excavation and it was this discursive interpretation that would influence how the day's work proceeded.

The forms, then, create a record for a moment in time other than the excavation itself. Writing forms and taking photographs is part of the practice—a thing to be done—rather than a part of the interpretative process. This relates back to Rob's comment in the previous chapter, that he waits until as late as possible to write his locus forms because he wants to be sure of himself before he commits to a permanent record. The act of writing comes after interpretation has already taken place in conversations around the site and the dinner table. Technologies like forms and databases keep track of data, but the data itself is crafted first through conversation. Finding themselves excluded from conversations, Bolivian archaeologists worked silently on forms and ignored the loud, 'childish' games of the North Americans. This retreat into a formal professionalism that they believed ought to be recognized, was a form of silent resistance. They were aware that their inability to act in a certain manner, a careful balance of expertise that is performed *through* informality, was holding them back. But they fell back onto the strategy of hard work and formal professionalism as almost a moral statement of how their hard work *ought* to be formally recognized, that the carefully maintained records *ought* to be more important than conversations, and as a form of detachment from a situation of powerlessness.

Blackboxing the body: the problem of embodied expertise

The differences between the Bolivian and North American archaeologists' experiences on site illustrate the extent to which practices on site simultaneously create experts out of people and knowledge out of an otherwise unaltered material world. The material world continues to exist separately to the archaeologist, but the way it is perceived by an archaeologist during the process of fieldwork is generative of archaeological knowledge. Archaeologists arrive at judgments about the nature and interpretation of archaeological objects through conversations with each other, but not all archaeologists are able to participate in ways that will be heard. This situation can illuminate a number of points within anthropological discussion of expertise and tacit knowledge, particularly when we consider the comparison of Andean example and British archaeology from the previous chapter.

As a form of scientific practice, archaeology has a somewhat uneasy commitment to mechanical objectivity (see Daston and Galison 2007). As Gavin Lucas has argued (2001: 2), although the explicit positivism of 1960s and 70s processualism was challenged by the constructivist turn of postprocessualism in the 1980s, even in sub-disciplinary communities that embraced the postprocessual in terms of theory and writing, actual excavation methods remained unaffected. Processualism is still the modus operandi in terms of method in the Andes. But although processualist archaeology explicitly attempted to model itself on the physical and experimental sciences, it still had to grapple with the problem that archaeology is not an experimental science, and has no means of standardizing its practices or objects of knowledge. Archaeology's problem, if one believes as many processualists still do that scientific truth can only come from mechanical objectivity, is that there is no way of repeating, testing, or standardizing archaeological objects or facts. Archaeology has no microscope, air-pump, or telescope to mediate the scientist's vision, nor the ability to replicate experiments so that multiple witnesses can attest to the security of matters-of-fact (Shapin and Schaffer 1985, Daston and Galison 2007).

The sub-disciplinary/national differences in methodology and epistemology that I discussed in the previous chapter are again relevant here. In British archaeology, for instance, the interpretive unit is the single professional archaeologist—all excavation is done by professional archaeologists, with no division of labor between a 'worker' who handles soil and an 'archaeologist' who handles paperwork. The single archaeologist has the authority to fill in his or her own form and thus make his or her own interpretations of the material and archaeological object, guided by immediate embodied experience. The expectation is that this professional archaeologist will draw on a combination of expert trained-judgment and an authority that derives from the tactile experience of handling the physical entity over a period of time (see Edgeworth 2003: 47). In Andean archaeology authority is unevenly dispersed among a much larger number of people who are all engaged in hierarchically distinguished conversations and practices. The archaeologist's primary role is to manage his or her workers, to record their actions in paperwork, and to engage in interpretative conversations with other archaeologists. But the ability to have these conversations—the ability to speak correctly, to be appropriately informal while also assertive, to call on shared references, theories, and experiences that are meaningful to the director—depends to a large extent on whether one has been socialized into the same disciplinary culture. I explore this in more detail in the second half of this dissertation by looking at how students are trained and socialized into becoming archaeologists in Bolivian, Chilean, and North American universities.

The point here is that in Andean excavations the work of the archaeologist involves taking part in conversations through which authority is unevenly dispersed, but in ways that are subtle, hard to define, and difficult to explicate except through the comments about how "Bolivians are so quiet" and "Gringos are so loud". Moreover, while the extent to which there is a difference between the expert's and non-expert's eye (Jasanoff 1998) is an important factor in defining who counts as an expert, it is only a subsection of a larger problem: namely, the claim that (a director's) vision is indeed the most salient sense through which archaeology's objects of knowledge are known, rather than for instance an excavator's sense of touch. The junior Bolivian archaeologists were annoyed that their physical experience of a specific spatial object was not enough for them to be taken seriously as equal colleagues and experts when that object was being discussed. They believed that their engagement with a particular spatial object, for instance the black splodge or the baby burial, meant they were better able to understand it. In contrast, the directors and senior archaeologists were able to have conversations about the object based on looking at it and hearing it described, but not as a result of knowing the object through physical experience. Despite Olivia's comment in the interview that one needs to know a unit like your child in order to understand it (see previous chapter), her level of trust in the interpretations of the archaeologists working for her did not come only from their intimate knowledge of the object. The senior archaeologists' ability to connect any particular part of the site to another (to see a 'bigger picture') was more salient and persuasive. Archaeological skill lies in the ability to tie together larger bundles of relationships, to make connections between things seen in the moment and those remembered or seen elsewhere. This talk draws the particular object under discussion into relationships with others that might lie elsewhere. So there are two understandings of the extent to which any individual physical thing has a dependable existence, that implies whether knowing it is skilled or not. The organization of the excavation-as-machine implies that objects are *so* inherently knowable, they *can* be known only through a visual inspection, and the director sees the ability to 'see the bigger picture'—to make broader and further reaching connections—as a sign of archaeological expertise. The excavator, however, insists that objects are complex and non-inherent, that knowing them requires physical and tactile engagement that ought to be taken into consideration first, before larger connections are made.

The machine's organization insists that material and archaeological objects can be measured, described, or drawn by anyone trained in the fundamentals of using a tape measure and writing numbers on a form, and interpreted as archaeological through reference to a basic knowledge of archaeological features. This involves a commitment to mechanical objectivity that has its origins in the positivist turn within archaeology in the 1960s, from which modern excavation techniques in the region date. But the fact remains that in practice this is not the case—differences in the color, texture, and composition of soil *are* difficult to distinguish and drawing out the multiple complex relationships between the material traces of soils, ceramic, stones, charcoal, organic material, human and animal bones is far from easy. For example, as we saw with the black splodge it is not always possible just to look at a flat surface and know whether it is going to be a three dimensional feature. Figuring out the materiality of the object *does* require physically interacting with it: pulling back one kind of earth from another, rubbing it between one's fingers, getting a sense of its heaviness, compaction, and texture as much as its color and the type of grains one can see. The commitment to mechanical objectivity results in blackboxing the tactile experience and tacit skills the excavator-archaeologist and maestro both draw on. Discursive skills and academic experience are valued over tacit skills and tactile experience during the course of the excavation. Later *both* end up being blackboxed out of the narrative of how archaeological knowledge is created when the field is left behind. During the excavation, the significance of discursive/academic over tacit/tactile is also an implied, rather than stated, distinction.

Writing out tacit skills

Tacit, manual skills are often considered to be less important than mental, or visual skills. Discussions of the potentially higher significance of tacit expertise emerged in the 1980s and 90s with the increasing sophistication of computers and the possibility of artificial intelligence that could replicate/replace human skills. It was realized that computers can be taught rules but can't think creatively and this opened up a debate around defining hierarchies of skills and knowledge as intrinsically more human and less mechanical, based on modeling how novices learn: a novice follows a set of rules exactly and explicitly, while an expert creatively and unconsciously develops solutions to novel situations (Dreyfus and Dreyfus 1986). In critiquing a rule-centric approach to the question of expertise, H. M. Collins argued that contemporary western societies value the kinds of knowledge acquired through formal education by a limited number of people, such as learning abstract facts, much higher than manual and social skills mastered by more people, such as cultural knowledge or the ability to move one's body (Collins 1990: 109). Manual and social skills are seen as less expert despite being harder to communicate or acquire beyond childhood, because they are interpreted as merely 'common-sense'.

This was a concept previously explored by Michael Polyani with his work on tacit skills. A tacit skill is an ability to do something that cannot be verbalized, such as riding a bike. A novice learns from a master by watching and copying without either being able to verbalize the rules they are both following (Polyani 1958: 53). Looking to the realm of scientific practice, Collins demonstrated that scientists working in labs learn the same way. Although the scientific method stipulates that experiments conducted under the same conditions using the same materials should be independent of the observer or practitioner, it is often the case that an experiment done in one lab cannot be replicated unless either the original or the new experimenter goes in person to the other lab to learn or teach how to conduct the experiment through face-to-face interaction (Collins 2001). This remains controversial, however, for reasons Delamont and Atkinson (2001) explore in their study of graduate students learning lab techniques. The presentation of scientific practice in undergraduate training mirrors the idealized image in science texts—that experiments are replicable given the correct materials and procedure and not dependent on the skill (or luck) of the experimenter. Graduate students discover abruptly that experiments are difficult and often fail when they start actually working in a lab. But after tacitly learning this they are socialized to exclude this learning process and failure from their writing, thus leaving the next generation of students to encounter the same shock (Delamont and Atkinson 2001: 88).

The idea that manual skills and tacit expertise are less valuable than abstract, mental knowledge, is pervasive. A division is made between technicians who do manual work and scientists who do mental work. Technicians are mostly invisible in narratives of science, at times becoming equated with the infrastructure and technical equipment of the lab in contrast to the agentive, strategizing scientists. Shapin and Schaffer (1985) draw attention to how Boyle left the manufacture and management of his air-pump to his technicians, particularly Robert Hooke. Technicians appear in Traweek's (1992) ethnography to support her discussion of the gendered division of labor within high-energy physics—technicians are more likely to be women, thus illustrating their lower status. In the laboratories studied by Latour and Woolgar (1979) a 'failed' scientist becomes a technician, while a physicist considers it an insult to be called an engineer (Gusterson 1998: 49). Technicians and engineers from such diverse fields as music recording (Schmidt Horning 2004), oceanography (Bernard and Killworth 1973), and emergency medical technicians in ambulances (Nelson 1997) feel that they are undervalued or have an ambiguous status because their skills involve the manipulation of physical things rather than abstract concepts (Barley and Bechky 1994).

> Physicists are the elite within the elite, the laboratory brahmins who rank the highest because their work, being the most abstract, is thought to be most difficult and because, unlike the lower-ranking engineers, they are more preoccupied with thinking about things than making them. (Gusterson 1998: 27)

One of the few exceptions of high-status professions that are based around embodied manual skills are surgeons, although even here there is complicated by the inclusion of technology (Hirschauer 1991, Pollock 1996: 349). In archaeology, embodied, tacit skills and an ability to physically and visually engage with material objects is necessary to bring-into-being archaeological knowledge, but without the mediation of technology. Tacit skills thus ought to be at the heart of the discipline and highly valued. But instead the excavation machine in Andean archaeology is structured around the idea that such skills are insignificant and non-archaeological. Moreover, the mismatch between the machinery of the excavation and the reality of practice is something continually experienced, but always ignored. Even the directors acknowledged, when asked, that excavating is difficult and requires skill, and objects are not unproblematically, inherently knowable. The most common answer to my question of why methods are never discussed was a cautious joke that everyone is 'just too scared' to engage in such conversations with their colleagues 'because they all secretly believe themselves to be doing it wrong'. Graduate students, going through the process of analysis and writing for the first time, struggled to overcome the concern that their facts were not actually as secure as they ought to be. Professionalization can thus be interpreted as a process of learning to bracket out contingency and write a dissertation *despite* personal fears about the insecurity of one's data (c.f. Delamont and Atkinson 2001). In this way, mechanical objectivity is held up as a model in archaeology despite constant experiences of its failure.

It is interesting that Frank Hole, writing in the same 1973 edited volume on the future of archaeology that included Kent Flannery's famous "Archaeology with a Capital S" paper (where S stands for "Science"), argued that "perceptive digging is basically a sensual experience that integrates visual, auditory, tactile, olfactory and even gustatory information." (Hole 1973: 306).

His description of teaching students to excavate fits exactly the description of the novice and master described by Polyani:

> I have often watched students dig through strata, walls, and other features without any apparent recognition of the fact. Floors of hard-packed earth, especially when they are marred by burrows or broken from use are notoriously hard for students to comprehend. Yet a supervisor will take a trowel and scrape down to a surface he confidently asserts is the floor while the student stands by in absolute bewilderment. The supervisor is likely to tell the student that the floor is obvious. Often he will say he can see or feel it but the typical student remains unconvinced and unable to find the floor by himself. The average supervisor will not be able to convey further information and either tells the student to scrape the whole area down to the same level or does the job himself. (Hole 1973: 306)

This explicit invocation of the embodied skills necessary to excavate is unusual, however: Flannery's paper is a classic, Hole's has been cited only once. The model of archaeological knowledge and expertise carried forward from the great debates about archaeological theory and method in the 60s and 70s, was one that required these embodied skills to be downplayed in the name of scientific objectivity: one cannot systematize sensory experience. But in practice mechanical objectivity is a difficult standard to live up to—and as Daston and Galison document, in other scientific disciplines trained judgment has become a more common epistemic model. "The trained expert (doctor, physicist, astronomer) grounds his or her knowledge in guided experience, not special access to reality." (2007: 359). Why has archaeology not embraced trained judgment, in such a way that embodied skills and experience would be more highly valued?

In part this invokes a problem of trust and authority that goes beyond the discipline itself. Through the example of high-energy physicists at CERN, Karin Knorr Cetina (1999: 133-35) drew attention to how expertise is a matter of trust. Although knowledge is produced through highly complex machines, an expert is someone who has sufficient experience to know when and when not to trust a machine. He or she is also, however, someone *other* scientists trust to be able to make this judgment. Theodore Porter (1995) argues that mechanical objectivity becomes a necessary virtue when a discipline is new or weak, because of a lack of trust. Objectivity is a means of defending professional judgment from outside interference or a strategy used to resolve the problem of long-distance interaction by creating a shared discourse. Rigid rules leave little room for personal judgment and therefore enable others who are at a distance to trust their colleagues' results. Porter and Jasanoff (2005) have both argued that, in the US, there is a marked preference for quantitative analysis that comes out of the US's particular tradition of anti-elitism and anti-intellectualism, and the dominance of litigation. A lack of trust in scientists and academics leads to a desire for numbers that "bear no mark of the knower" (Daston and Galison 2007). This is particularly the case for sciences and professions that are called upon to justify themselves to broader national publics or to funders. Porter's analysis of why the high-energy physics community studied by Traweek (1992) does *not* have to rely on such rigid adherence to formal objectivity, for instance, illustrates how the need to appear to be mechanically objective is linked to a lack of authority, an inability to inspire trust.

> Their work has, until very recently, been so prestigious that they have had little responsibility except to each other. They have suffered a minimum of intervention by powerful outside interests. The physicists have wanted nothing from the government but money, and the government has, since the war, been content with the physicists' own marks of esteem, such as Nobel prizes. So they have been free to cultivate their own style, language, and traditions. (Porter 1999: 224)

Taken against this background, downplaying the centrality of embodied expertise to the creation of archaeological knowledge can be seen as a sign of disciplinary insecurity—although in practice archaeological statements of fact are rarely challenged by anyone outside the discipline, much less by policy makers or funding organizations. The lower value of manual labor and tacit skills in general pushes towards a downplaying of archaeologists' embodied skills; while the perception that archaeologists lack other forms of social authority pushes towards an over-emphasis on objectivity.

However, in this chapter I have been discussing more than the manual skills involved in excavation and the sensory engagements that Frank Hole alludes to. The divisions of labor between workers, maestros, Bolivian and North American archaeologists also brought into play discursive skills and varying abilities to perform expertise. Summerson Carr (2010: 18) argues that expertise is not something one has, but something that is done: expertise is always a performance rather than a cache of skills or knowledge. This performance is a matter of learning how to communicate a familiarity with people and things, which certainly appears to be the case here. Apprentices must learn *how* to say, as well as *what* to say. Not all archaeologists are able to act in such a way that they are recognized as expert. It is difficult to say why exactly Bolivian and North American archaeologists were treated differently on this and other projects, although different education and disciplinary cultures certainly play a part. Equally it is possible that the recognition of confidence and 'assertiveness' is related to differences of class and gender despite the focus I have placed on differences in nationality manifest through implied stereotypes of silence and loudness, shyness and friendliness. The ethic of non-hierarchy and informalness, particularly prevalent among the North American archaeologists who place a high value on the field-experience being one of relaxed, friendly comradeship, however, precludes discussion of any inequality. The over-riding premise is that everyone begins as an equal, as a friend—that because there is no *explicit* hierarchy and everyone is 'slumming it' together, there is indeed no hierarchy. Moreover, there is an extension of a very specific, US concept of meritocracy, particularly within education and academia (Durrenberger 2012).

Meritocracy is a slippery concept, particularly when discussing hidden, masked, or tacit expertise. It implies that the best will naturally get to the top through individual talent and hard work, but an appeal to meritocracy can mask deeper structural inequalities. Karen Ho, for example, describes the kind of tautological arguments used to justify privilege in terms of merit among investment bankers (2005, 2009). Novice bankers are repeatedly told that they deserve their social positions because they are smart and are offered as proof of their smartness their Ivy League status and later their jobs as investment bankers. This is a circular argument: their social position come from being smart, they are smart because of their social position. Meritocracy is an ideal that ignores the larger structural, socio-economic, and cultural conditions that, for instance, enabled young upper-class white Americans to attend Ivy League universities in the first place. Armstrong and Hamilton's (2013) insightful and thorough critique of 'inherited meritocracy' among a cohort of female undergraduates at a state university makes the same point: appeals to meritocratic principles serve, more often than not, to bolster inequality and stifle critique.

Within expert communities the less formalized forms of authority are, the harder they are to challenge (Tyler and Tyler 1986: 238). Authority thus lies in the ability to define the world and other people's place in it unchallenged, rather than the necessity of imposing a view of the world on others by force. A meritocracy built on formal rules would be easy to challenge, but one built on tacit social skills is harder to pin down. Armstrong and Hamilton's discussion of the consequences of college party-culture and the example described here of archaeologists' joking relationships point towards the hegemonic power of an ethic of informality when it comes to recognizing embodied social expertise. Actions that are small or informal (such as having a conversation over dinner about the wall dug that morning, or chatting enthusiastically while walking to site about a paper you both read) become instances of tacit, 'common-sense', social or cultural skills. As Collins argues (2001) these are the hardest to learn or identify, and thus authority grounded in tacit cultural expertise is the hardest to either challenge or learn. I would add that not only might it be unchallenged, but simply unnoticed: as, for instance, when the North American archaeologists were entirely unaware that they were overlooking Bolivian archaeologists' expertise. Informal authority might not even be recognized as such by those who possess it at the expense of others: but this is exactly the point about the nature of privilege that feminist standpoint theory, for example, draws attention to. Those privileged by a dominant culture are least able to see its effects.

Unable to perform their expertise in ways that would be recognized, the Bolivian students fell back on writing their forms and diaries, following each instruction they are given with increasing formality and scrupulousness. Unconsciously or consciously excluded from the informal conversations taking place on the edge of the site or around the dinner table when connections are being made, they remained silent. In effect they retreated into a kind of stubborn formalism, embodying an argument for meritocracy that does not exist—as if to say that if they work hard and follow the rules, their lack of recognition is the fault of unfairness and discrimination rather than a failure to correctly perform social expertise. Moreover, the performance of *Bolivian seriousness* in comparison to *Gringo childishness* described at the beginning of this chapter—a frame broken by the incidents with the pen and the pellets of soil aimed at the supervisor's head—now make sense both as rejections of non-standardized methodological strategies, and a way of calling-out the belief that Andean archaeology has an informal, friendly, and meritocratic culture. But this echoes the strategies of the minority stock brokers Karen Ho described, who, finding themselves unable to draw on privileged social networks to get ahead, determined on a strategy of working extra hard despite knowing that this could backfire (2009: 107-112).

Postcolonial archaeology and multivocality

The postprocessual turn in archaeology during the 1980s and 90s involved a critique of ‘objective science’. This argument, however, was entangled with a concurrent discussion of the possibility of multiple interpretations and specifically the recognition of non-archaeological or ‘alternative’ interpretations of the past.[8] Such engagements have become expected of archaeologists: today involving local and indigenous communities in archaeological practice and to making gestures towards the consideration of indigenous knowledge of the past is a matter of ethical and professional best-practice.

But what challenge does such engagement pose? Particularly in comparison to other alternative voices that could be included but are not. In the Tiwanaku project there is a rich and productive collaboration with local community members. The structure of the excavation machine is dictated to a large extent by labor arrangements that the Comunidad requires. Throughout the Andean region archaeologists include religious rituals that are requested by local

8 While the issues involved were complicated and various, the debate focused on the recognition and inclusion within ‘mainstream’ archaeology of indigenous voices, connecting the need to do so to past and current abuses suffered by these groups. This point is important for understanding why, for example, the claims of neo-pagan druids in England to be the indigenous descendants of prehistoric skeletons found at Stonehenge are treated with derision by the same archaeologists who believe strongly in NAGPRA. This argument was the subject of a heated debate on the World Archaeological Congress mailing list in 2008, following the posting of an email mocking a newspaper article. (http://news.bbc.co.uk/2/hi/uk_news/england/wiltshire/7854134.stm) Neo-pagan druids have not suffered the same history of persecution, repression and alienation from their land, culture and history that the indigenous populations of the Americas have endured. With few exceptions, notably the longstanding engagement with Mother Goddesses at the archaeological site of Çatalhöyük, the alternative voices that are sought in the name of inclusivity are those of local indigenous groups who are believed to be connected to the archaeological site by self-recognized descent.

communities at the beginning and end of excavation. The Andeanist archaeologists I worked with were very interested in local concepts of history and the past, and if anything would rather know more, not less, about them. Engagement with local communities involves being respectful of and interested in local spiritual and cultural life, and being responsible employers.

On the whole, however, this poses little to no challenge to the archaeological process. There is neither a challenge to the narratives that archaeologists produce, nor a challenge to their practices of excavation. On the North American Field School (NAFS) in the Chilean Tarapacá desert that I discuss in later chapters, objections were initially raised to the excavation of mummies. But these problems were resolved after an intense period of conversation and engagement. After spending several weeks talking with members of the community who were unhappy about the excavation of their ancestors, and negotiating that appropriate ceremonies would be conducted by both the priest and the local shaman, excavation resumed amid an atmosphere of increased trust and collaboration between the archaeologists and the local community. That there were objections to the excavation in the first place was a highly unusual situation for this part of the world (Lozada, Boytner and Kakoulli 2007), but the problems were resolved through the kind of open engagement and willingness to take seriously the concerns of non-archaeologists that has become expected of archaeologists working in settler-colonial contexts in the past few decades. Against an older model of the arrogant western scientist who believed in their own infallible truths, there is a commitment to engaging with local indigenous communities, taking seriously their alternative perceptions of archaeology, and including communities in decision making processes as stake-holders (Derry and Malloy 2003).

But the inclusion of alternative voices does not necessarily challenge archaeological authority. Perhaps surprisingly, conflict is rarely over the things that archaeologists themselves consider to be their final product: namely, narratives about the past. Nor is there conflict over the details of excavation practices I have been discussing here. Once permission to excavate has been granted including indigenous voices does *not*, in practice, threaten the relationship between data and interpretation or interrupt the process of creating archaeological knowledge. The stakes involved in acknowledging the non-objective nature of archaeological data are such that one must engage with alternative voices, but those voices are likely to challenge only the least archaeological aspects of one's work. The practical matters ("is this man going to let me excavate in this field or not?") are frustrating, but they do not get to the heart of the archaeologist's epistemic authority.

I noticed during the course of my research that the suggestion that a foreign archaeologist may have offended their local indigenous community could seriously damage that archaeologist's reputation. But the notion that a European or North American archaeologist working abroad would have a troubled relationship with their local colleagues seemed to be taken as almost a matter of course. As I discussed in the example from Bolivia, the moments on site where there is tension and conflict are not the encounters that occur between archaeologist and the material world, nor between archaeologists and indigenous workers, but between different archaeologists and usually archaeologists who have been trained in different disciplinary cultures. Much higher stakes are involved in allowing one's colleagues to challenge moment-by-moment acts of interpretation on site (such as: "this is not a floor." "That should not be dug this way."), or one's narratives ("This period ended in 300 BC not 500 BC." "This was the most important site in this region, not that one"). To include the voices of shamans, or indeed Çatalhöyük's mother-goddesses, is, in effect, more acceptable than including the voices of those one considers to be culture-historians, or nationalists, or less specifically just bad archaeologists.

Understanding this distinction does not necessarily mean that archaeologists ought to include all interpretations, including those they considers to be incorrect. But it does raise the question of what the notion of multivocality means in terms of actual methodology. Gavin Lucas (2001: 2) noted that the postprocessual critique, while changing much in terms of the way archaeologists talk and think about archaeology, had almost no impact on field methods. I suggest that this is in part because its concern with opening up the interpretative process did not in fact challenge the creation of archaeological facts at the most basic level of day-to-day practice. This level of knowledge making remains unchanged. The stakes involved in including alternative voices at certain stages of the archaeological process are resolvable, while the stakes involves in including alternative archaeologist's voices are much higher.

Chapter Four

Globalizing and localizing The Field

Introduction: encountering the field

To the great excitement of both the student archaeologists and the director Matías, the last week of the Proyecto Arqueológico del Norte, Uno (PANU) coincided with Pueblo's celebration of the festival of San Francisco. Matías had been working in this region of Chile throughout his career, and had based himself in the town I am calling Pueblo for 15 years. Although most of the students on his project had extensive experience working in the north of Chile, they had not previously had a chance to see this fiesta and were looking forward to it as the highlight of the trip.

Pueblo is situated not far from the border with Bolivia, and on this two week field season we were working on three sites that lay within an hours drive of the town. I was told that there is a lot of cross migration between Bolivia and Chile, and the population of Pueblo are Atacameños, or possibly Aymara from Bolivia. As we worked on our excavation site during the day, the students would occasionally look up to the mountains that rose high along the horizon and ponder the fact that Bolivia lay on the other side. So close they could see it, and yet none of them had ever visited the country themselves. The chance to see the fiesta, and particularly the traditional dancers that would be its highlight, was a chance to get close to an indigenous Andean/Atacameño culture (something nebulously part-Chilean and part-Bolivian) they had read about in their classes but never experienced.

The day the fiesta started, the group of five archaeologists I had been working with was forced to leave site early. Out in the desert the wind was vicious and incessant. On good days it was just tiring. On bad days like this, the wind was so intense is was difficult to see, impossible to hold onto tools or paper long enough to work, and each step felt like wading through water. A few times a small archaeology student was literally blown off her feet. After some nervous discussion out of sight of the others, followed by a round of hesitant conversations among the team, the senior archaeologist in our group, Luciano, eventually called Matías and got his approval to leave site. We went to the city to run errands—selecting groceries for the project and beers for ourselves under the watchful eye of the security guards who stared long and hard at our muddy clothes—all the time trying to shake off the feeling that we were playing hooky. More than a few times the other students and I reassured Luciano that leaving site had been the best call. Eventually, as we headed home to Pueblo, the Chilean archaeologists began to see the bright side of the dispiriting weather: at least we would get a chance to watch more of the dancing.

After a few quiet beers in our hotel we headed out onto the streets of Pueblo. Luciano was one of four archaeologists with a licenciatura title in the larger fifteen person project.[1] As such he was nominally (and quite reluctantly) in charge of the small group that consisted of him, three younger female licenciatura students, and me the visiting ethnographer. As we stood in the street outside our hotel door, watching the colorful dancers pass by and listening to the loud music that would play day and night for the next few days, the students excitedly took photographs.

1 See chapters 5-6: a licenciatura is the first degree earned at university, but it not directly comparable to a US Bachelors degree. Pertinent for this discussion, someone is not legally allowed to call themselves an archaeologist, or direct/run projects, until they have earned the degree awarded *after* this one, a titulo professional. Luciano was in the process of getting his titulo, and therefore was more senior than the others.

Pueblo is quite small, or at least seems so from its center. Around the tidy plaza there is a church, a school, a hospital, and other administrative buildings. Only a few of the white-plastered houses or buildings have a second floor. There are two main streets running parallel to either side of the plaza, one of which is lined with a few shops that sell a selection of basic items like butter, laundry powder, candies, and pens. A few newish looking stalls selling souvenirs for tourists clustered around a paved area that was set off from the main street, near to the church that was listed as a national monument and thus served as the town's single tourist attraction. Pueblo thus exemplifies the image of a quiet, rural town—in marked contrast to the cosmopolitan and urban Santiago we had left behind. PANU ate each evening at Pueblo's only restaurant, but there was also a social hall on the same street. We occasionally noticed townspeople using the hall, but never saw other guests in the restaurant. On this night of the fiesta the social hall had been converted into a disco, complete with lighting effects that came from turning the hanging light bulbs on and off. Six of the team members, including myself, were staying in a hotel just down from the restaurant, while the rest of the team were in another hotel located some distance away, on the far side of Pueblo.

Outside *our* hotel was a grotto to Our Lady of Lourdes: the focus of most of the dances over the weekend. As we came out into the evening with our beers and cameras, therefore, we had an excellent view of the celebrations. The students were delighted. As the dancers swirled and stomped past in lines—high heeled boots stepping in coordination, purple sequin-covered mini-skirts billowing, and occasionally clutching their matching, feather strewn bowler hats to their head—the archaeologists watched with curiosity and excitement. The next troupe featured women dressed as 'cholitas' (Weismantel 2001) in long, layered skirts and colorful shawls and men in matching uniforms playing brass band instruments. To my eyes the dancers were a small-town version of many almost identical troupes I had seen in Bolivia and reminiscent of those I had seen a few years earlier at the famous La Tirana festival, a little further north but still in Chile.

My conversations with young Chilean archaeologists frequently involved them asking me curiously and sometimes wistfully about my experiences with indigenous people in Bolivia and Peru. These students took ethnographic and archaeological classes on the indigenous people of Chile and they might have campaigned or marched in the streets on their behalf; they quite probably wore 'ethnic' clothes or jewelry of the same type sold to tourists, but they had rarely actually met or interacted with an indigenous person. Given the history of repression, harassment, and forced 'Chilenization' of Chile's indigenous populations throughout the twentieth century, this is not surprising (see Jofré 2007, Van Kessel 1992, Miranda 1996). What I am interested in drawing attention to here, however, is how the experience of going into the field, for these archaeologists, was made pleasurable and exciting in part because it promised an encounter with the indigenous culture of people they had professional and political sympathy for (in the sense that they were highly critical of the way indigenous people are treated by the Chilean State), but had never met. Fieldwork was thus a chance to encounter something familiar but unknown; something part of their national and professional identity but without the expectation of easy identification, given their ethnic/racial difference as middle-class Santiagueños for the most part of majority European descent.

While taking a walk through Pueblo alone a little while later, I ran into my friend Martina, a member of the other team that was now apparently back for the day. She was walking in the opposite direction and talking on the phone, but she seemed distressed. She stopped when she saw me and ended the phone call, as I asked what had happened. Agitated, she said she'd had a bad day on site and was getting “very stressed” being around everyone all the time, and having no “personal space”. Even just now, she complained, she’d asked them to let her get out the car in the center of town, rather than riding back to the hotel, so that she could use the walk back as an excuse to be alone. But even then they had not wanted to drop her, and kept asking her “why?” and “what was she going to do?”. She complained that the only moments she had by herself these days were in the bathroom, so that she was taking much longer than necessary each time she went, just to get some space alone.

I tried to comfort her and reassure her that is was OK to need time by herself. It’s the little petty things that start to annoy you, I said, when you spend too much time living tightly together on a project. At this she launched into a recounting of the various mishaps and misunderstandings on site that day, interspersed with her speculation that some of the blame for the interpersonal drama among *certain* members of the group lay in a possible doomed romantic attachment: a subject which had been a hot topic of gossip for the past week. We walked up and down the quiet road for a while until she felt a bit calmer. Promising to talk later after dinner, I headed back to my own hotel alone. The feeling of claustrophobia and frustration Martina had described was utterly familiar. I could hardly think of a single project I had been on since 1998 that hadn’t involved someone, at some point, making the same complaints. In an interview back in Santiago a few months later, I discussed with one project director whether the ability to handle this kind of communal living is a skill that archaeology students needed to have or acquire, comparable to the kinds of excavation skills, writing skills, and analytical skills we had been discussing.

> Yes yes very much. Very much. That’s why I've chosen these people [to work on my projects] according to those skills. With people that we can get along well. And they will not create trouble. And they are simple and they can adjust and

> accommodate to certain circumstances. And they are not so... because some are more difficult, sometimes people are more simple. I... so it's better to... so it's easier with simple people.

Later that evening, as the whole group sat down to a boisterous and cheerful meal, the noise of the fiesta outside proved too much of a temptation to keep us in our seats. The evening meal was usually something lingered over, a place for the different teams to gossip and chat, but also to compare notes on the day's work. Tonight, however, the cook— an elderly lady who delighted in the fact she had known Matías for 15 years and still had photos of him as a student himself—kept dashing across the room to peep out the door at the dancers going by. Eventually there was a collective decision to abandon the table and head out onto the streets. Finding myself standing next to Matías, I asked him how he liked the dancing. Very much he replied, then took the opportunity to make one of his frequent semi-jokes about how, as an anthropologist, I ought to know more about the indigenous dancers than he did. Matías seemed always to slightly disapprove of my apparent lack of professional expertise when it came to indigenous groups of Chile, something I interpreted as an insinuation that the proper focus of anthropology was the study of indigenous peoples.

Much later that night, as the group of us staying in the same hotel walked home, we passed the social center. The parades were over, but the party was just getting going. There was a live band playing a kind of huayno-cumbia music[2] and the entire small room was packed with people dancing and drinking. The building itself was simple—open on one side to the street, with a basic roof and a concrete floor. The archaeologists and I stopped in the street to watch. People

2 Modern cumbia is a type of synthesized pop-music that is very popular in South America, but like Reggaton it is considered to be low-class, particularly in Chile. Huayno-cumbia is a more Andean version of cumbia, popular in Bolivia and Chile. See Romero (2002). To hear an example: http://www.youtube.com/watch?v=JGsnKdBcNHk.

were dancing hard: drunk old men clutching each other's shoulders, teenagers in tight jeans and with sweat smearing their heavy make-up, middle aged women with rolls of fat and large breasts squeezed into tight t-shirts. The scene was rowdy, drunken, and loud.

We stood outside, looking in. About half our group bobbed up and down to the music, turning to look at each other to see if there was a collective desire to go inside. The other half looked decidedly uncomfortable and kept glancing down the street in the direction of the hotel. I wanted to go in and join the party or at least have a beer, and tried to work out if this was the feeling the others shared as we stood indecisively in the street. Even those dancing along seemed unsure of themselves, tentative and not quite daring to step closer, so I didn't repeat my suggestion too often. Luciano said he wanted a beer too but he seemed as indecisive as he had been on site when trying to decide whether we could leave. I tried to prod us towards a decision: "Well we can either go in, or keep going back to the hotel". The others smiled nervously but didn't reply. After about ten minutes of uncomfortable indecision hovering on the edges of the street we gradually drifted back towards the hotel and away from the boisterous party.

Understanding the reluctance and discomfort the group felt when it stumbled across the party in the town hall, requires situating it in comparison to the excitement at the traditional dancers. The people involved in each festivity were quite probably one and the same. Encountering them through folk dances and costumes was exciting, and an excitement the students had been looking forward to. Encountering them unexpectedly in the familiar clothes, movements, and drinking habits of non-indigenously marked Chileans—and more to the point, of lower-class Chileans—was unnerving. The second image undermined the first, in the process collapsing the contrast between the world one encounters in the field and that left behind at home in Santiago. When put next to Martina's frustrations, the moment outside the social hall also highlights the tensions of archaeological fieldwork as an intense experience of working and living in close and continuous contact with a small group of other people. The desire to maintain an even-keeled group dynamic by not striking out alone or taking charge was a concern for both male and female Chilean archaeologists on this project.[3] Nobody wanted to be the one who took charge of decision making, told others what to do, or appeared to shake up the calm collective process. Like in the North American/Bolivian excavations described before, the group dynamics of fieldwork foster an ideal of communal harmony within which everyone is more or less equal, and that this is a relaxed, democratic space without formal or marked divisions or hierarchy. This came into clear contrast for me as I realized that my vocalized attempts to prod people towards decisions were being interpreted as rudeness by my Chilean companions, both when I expressed exasperation with the long deliberations about whether the wind was bad enough that we could leave site and in the street outside the social hall.

What does it mean to be 'in the field' as an archaeologist? For Santiagueño archaeologists working in the north of Chile, the field involves immersion in a space marked as indigenous and rural, as collective and non-confrontational. It is a space that is understood through its contrast to normal day-to-day life back in metropolitan Santiago and the classrooms and offices of the Universidad de Chile. However, this construction can break down: when indigenous people turn out to also be lower-class Chileans, when attention is paid to one person's seniority and they are required to take on decision-making responsibilities, when living and working with one's friends and colleagues 24-7 becomes irritating rather than fun.

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3 Although I am reluctant at this stage to go so far as to link this directly to a specifically Chilean performance of gender, this possibility is one for future consideration. I am specifically inspired to do so by similarities I see between the gendered performance of young male Chilean archaeologists I encountered, and the portrayal of men of similar age and social background in the Chilean movie *Crystal Fairy* (2013) directed by Sebastián Silva.

When contrasting this Chilean excavation with the North American/Bolivian excavations described in the previous two chapters, we can see that experiences of 'the field' are specific to the place from which one starts and returns to, but take on a similar form of transition or contrast between one place to another. For Chileans 'the field' is not associated with foreign travel, as it was for the North Americans going to South America. But it does draw on an expectation of Otherness that is structurally similar to that employed by North Americans working abroad. The Chilean field as a place of indigeneity and ruralness, of communal work and socializing, where hierarchy is minimized and one is surrounded by friendly, easy going colleagues, a place where gossip is rife and love-affairs may break out or be repressed: a place that is enjoyed because it is a contrast to one's 'normal life' back home in the university in Santiago. Moreover, in moving from urban Santiago to the rural and indigenous North, these archaeologists constituted themselves as national/Santiagueño subjects with particular upper-middle class and politically leftist identities. But encountering a space that is both similar (still in Chile) and different (framed as more rural/indigenous than Santiago) from one's normal life generates a heady combination of pleasure and unease. Pleasure when the field proves to be as much of an escape from normal life as expected; unease when a shift in perspective brings into focus their own class position, and disrupts the expectation of what the field ought to be like. In what follows, I intend to elaborate on this structural opposition rather than the specific details of indigeniety, communalism, and urban/rurality that are specific to this Chilean example, in order to relate this opposition to the ways in which 'the field' is socially and epistemically constructed in archaeology more broadly.

Theorizing field sciences

The fact that archaeology's places of knowledge-making are geographically and temporally dispersed—in other words, that field work is something done in a variety of locations for limited periods of time—is significant for understanding the broader epistemic culture of archaeology in comparison to other sciences, and the variation between archaeological communities in different countries/sub-disciplines. To what extent is 'the field' in archaeology analogous to 'the lab' in other sciences? And in what ways is the Chilean experience of the field described above similar or different to that of the North American and Bolivian archaeologists described in the previous two chapters? Laboratory studies have suggested that the bounded space of the lab serves to purify and control nature, and that labs are central to the knowledge-making practices of sciences (Knorr Cetina 2007: 366). Laboratories transform objects into partial or purified forms and can 'bring them home' to manipulate them on their own terms, for instance by studying events outside of their usual cycles of occurrence—all of which is "epistemically advantageous for the pursuit of science" (Knorr Cetina 1999: 26-9). In archaeology this is not possible. The epistemic objects of archaeology are encountered in a space that is impermanent, elsewhere, and always already something else, rather than in a controlled and controllable lab. In some field sciences this problem can be resolved by taking a lab into the field: in the case of the ships and probes used by oceanographers, a kind of lab is literally immersed into the field (Helmreich 2009). In others like archaeology and primatology (Haraway 1989) this is not possible. Records and certain objects like ceramic sherds and bones may be taken back to a temporary field-lab/artifact processing station or even 'home' (the archaeology space away from the field, in the cases discussed here an anthropology department in a university in Santiago). But the epistemic objects themselves are created in the field first.

So how do sciences that cannot bound their objects of inquiry within the lifeworld of a laboratory maintain control over them? Moreover, when any notion of reproducing experiments or observations in another laboratory is impossible because each field site is a unique space (and one that is destroyed in the process of being studied), how is the concept of the reproduceability or commensurability of knowledge grappled with, such that the knowledge-products retain their integrity when they leave the field (Howlett and Morgan, 2011: xv)? I will argue in this chapter that 'The Field' is constructed as a specific social and epistemic space—a lifeworld to build on Knorr Cetina's use of the term—through particular practices and meanings. A lifeworld that is constructed through contrast with the university as 'home' when both are understood to be locations of archaeological practice; and at the same time a space that is 'local' and therefore particular—but a generic, exchangeable, comparable form of localism which is defined more through its opposition to the 'home' as that which is *globalizable* (i.e. able to be imaged as 'global'), than through its own specificity. The generic Field is then able to become a space that is commensurable with other fields elsewhere, making the knowledge that comes from each also controllable and commensurable.

In making this argument I will also, however, address another particular challenge archaeology poses as a field science. Namely that its objects of study and its fields are also bound up with the politics of location. The field is always also a location that exists within national or regional territories, and as a result access is always threatened by external factors like governmental regulation, the freedom to cross national borders, the outbreak of war, or prior claims to landownership and stewardship. This also, however, makes archaeology particularly prone to nationally specific differences in epistemic culture. In other words, I end with the possibility that because the politics of location is so central to the construction of The Field as an epistemic space, archaeology is likely to be more prone to fragmentation into different nationally located epistemic cultures than, for instance, other sciences where a single epistemic culture is shared internationally and international collaborations are less problematic.

The landscape as lab and the heroic explorer

Before looking in detail at archaeology, we can consider how the same problem has been discussed in other field sciences. The difficulty in maintaining authority as "real science" (Kuklick and Kohler 1996: 1), with explicit reference to the shortcomings of the field in comparison to the lab, is a constant theme of concern in historical accounts of field sciences. The propensity of knowledge in the field to be built through experience (Mitman 1996), to rely on volunteers or non-professionals to gather data (Kohler 2002: 71), or to have a suspiciously popular appeal that distracts from an image of sober, elite vocation (Tucker 1996, Hevly 1996, but see also Kaiser 2004 for physics), have also all been proposed as reasons why the knowledge-claims of field disciplines are considered to have a more fragile legitimacy than laboratory sciences. In his brief discussion of Brazilian soil sciences, Latour (1999) argues that in field sciences the laboratory is the landscape and as such the problems of authority and transferability of knowledge that are central to all sciences are magnified, but ultimately Latour sees field sciences as not fundamentally different from lab sciences. Hevly however, in reference to Alpanist-scientists studying glacier motion, argues that the field will always appear too uncontrollable, too difficult to bound, and too localized when compared to the lab, suggesting that Latour's analogy is not as easy to make as it seems: the landscape-lab by definition can't be reproduced in another location (Hevly 1996: 71, Haraway 1989: 249).

Looking at field and laboratory biology, Kohler (2002: 9-11) argues that in the early days of the discipline the exclusivity of labs inspired trust, but field biology also acquired its own form of legitimacy by drawing on the image of the heroic explorer. As this became a less viable and successful source of scientific legitimation, field sciences tried to appear more lab-like through adopting various practices of experimentation and control. A similar process occurred in Arctic and Antarctica sciences (Bloom 1993; Powell 2007, 2008; Sörlin 2011): during the late nineteenth and early twentieth centuries, the authority of scientific exploration at the Poles was inextricably tied to the (nationally specific) performance of heroic masculinity by Norwegian, Canadian, US, and British scientists and explorers. This masculinity was explicitly raced as well as nationalized. In the case of Peary's expedition to the North Pole, for example, Matthew Henson, an African American, and the Inuit members of the team were conceptualized more as tools and explicitly referred to as 'technology' or 'cogs,' rather than being seen as assistants, technicians, workers, or scientists (Bloom 1993: 5-6, 98). In contrast, Peary's white, US, male body epitomized the ultimate *scientific* device. "There was an interest in showing that a male American body as a scientific device could dominate the most severe and inhospitable physical environment of the globe." (Bloom 1993: 116). In the nineteenth and the first half of the twentieth centuries the image of the field thus invoked conceptualizations of idealized masculinity, particularly in the US. With the onset of industrialization and urbanization, the US found itself in the grips of a 'crisis of masculinity', leading to the idealization of the 'perfect' white male bodies of figures such as Tarzan, Houdini, and the body builder Eugen Sandow (Kasson 2001), and the impulse to toughen up weakened, urbanized men with pursuits like the Scouts that allowed them to 'Play Indian' (Deloria 1998). But by the 1950s and 60s there was an increasing understanding that, to be a ‘real’ science, one could not rely on the trained judgment embodied by a gentleman scientist—one had to model experimental laboratory sciences. The difficulties of modeling the control and replicability of a lab, given the wildly unpredictable environments at the Poles, became a source of constant frustration and concern, as Bloom, Powell, and Sörlin all document.

A similar turn to positivism occurred in archaeology in the 1960s for the same reasons, and one could argue that the history of archaeology over the twentieth century was one of gradually becoming less associated with the romantic masculine heroism of figures like Heinrich Schliemann and Howard Carter, and more with the idea of archaeology as ‘science’ (Lucas 2001: 7). The popular image of archaeology still involves this romantic ideal (Holtorf 2006), but within the discipline itself the reliance on trained judgment is more problematic, as the previous chapters discussed. Over the twentieth century several field sciences went through a similar process of moving from the ideal of the heroic, white, male explorer to that of the landscape-as-lab, but with neither being entirely successful models for establishing authority. Looking at the present, neither model fully explains the significance of the field to the archaeologists working in Chile and Bolivia today, nor how its knowledge-statements become authoritative and trustworthy in the absence of laboratory conditions. This suggests that we need a different model to understand the field in archaeology and the epistemic advantages it provides.

National cultures of the field

In this chapter I want to tack between two positions, however: both discussing ‘archaeology’ and drawing attention to differences between nationally based archaeological communities. In doing so I will make what is essentially a structural argument: that the lifeworld of the field is constructed as a specific kind of place not through its isolation, boundedness, or ability to control nature (as in the case of a lab), but through its opposition to the non-field. This basic structural opposition might be performed or experienced differently in different places, thus leading to different epistemic cultures in different countries. But the structure of opposition between the field and non-field remains the salient feature.

The importance of gender can briefly serve as an example to illustrate what I mean by different experiences of archaeology and different understandings of the culture of the field. The North American archaeologists I studied frequently discussed their understandings and experiences of the field in relation to a concern for gender inequality and a critique of machoism. Their understanding of gender in the field was nuanced and complex, and a topic that was often brought up casually in conversation. For instance, they discussed how they were positioned as non-female by indigenous men when they were dressed in their own clothes but instantly become both female and highly comical when they 'dressed up' in the clothes of indigenous women. They also discussed what they perceived as sexism directed to them by mestizo, upper-class male archaeologists from La Paz; and how they hoped that the Tiwanaku Project, and another in the region also directed by a prominent female archaeologist, would serve as models for both male and female graduate students while countering the "kind of idea of the larger than life male" trope that they believed had become prevalent in Andean archaeology.

In contrast, during my research with Chilean archaeologists there were a notable number of situations where I brought up the subject of gender in interviews and was told by women, with much forthrightness, that this was not a significant issue in Chilean archaeology. On the one hand there was an acknowledgment that Chilean society is macho and homophobic and this undoubtedly affects the day-to-day professional life of any Chilean professional. On the other hand there was a common insistence that archaeology was a non-sexist discipline. Two of the oldest and most respected archaeologists in the UdeChile department are women and their role in directing the trajectory of the disciplinary community up to the present day is undeniable—one noted in an interview that she had personally taught or mentored nearly all the current faculty when they were themselves students. The female and male archaeologists I spoke with in Chile considered gender to be an interesting topic when I asked them about it, but not necessarily a significant one; and they emphasized the number of women in prominent roles in the Chilean archaeological community. One middle-aged woman added for emphasis: "If you ask all the women they will say then same. Did you ask [Catalina]? She has been the president of the Society of Archaeologists." Instead they echoed the critique of the Argentinian archaeologist Gustavo Politis (2001) and argued that the more pressing concern was the inequality perpetuated by the hegemonic influence of US and Anglo-European archaeologists, itself a reflection of political and economic inequalities between the North and South.

My point here is not to claim that Chilean or North American archaeology is or is not male-dominated; but to show that something that was central to *all* discussions of the culture of archaeology for North American archaeologists was not considered by Chileans to be particularly significant. US theorists of archaeological practice, for instance Joan Gero (1996), have seen the field primarily in terms of gender, and British archaeologists focus far more on the question of class (see Chapter 2). But South American archaeologists potentially understand it most immediately through the lens of North-South inequality. These different experiences of class, gender, and colonialism are bound up in their understanding of their disciplinary culture, including their experiences of the field. But if we attempted to understand experiences of archaeology and specifically the culture of the field from the perspective of only one of these, for instance North-South inequality or counteracting machoism—*even though these were often the ways the field was discussed by these particular people*—we would be universalizing what is in fact a specific experience of the field from one place, and would also miss what these experiences *do,* at a structural level, in terms of creating the lifeworld of the field.

Thus we need to understand both the specific ways in which the field is experienced (for instance, in the Chilean case described earlier in terms of indigeneity and non-hierarchical group-affirming interactions that downplay confrontation), but then look to see how the structure of these experiences of the field are central to constituting the field itself, as a specific kind of (temporary) epistemic space. My intention, then, is to focus on how the field is structurally constituted through practice, and how this contrast is experienced as shifts in performances of gender, class, and colonialism, but also through invocations of civilization/urbanity, freedom and escape, ruggedness and manual labor, travel, and indigeneity/nationalism, all of which are brought into focus through contrast with non-field archaeological practice back home.

Performing the field as a local space

The field cannot be defined by its geographic location nor its material properties alone: it is not a fixed place. Although the excavations I have discussed here all took place in rural locations, archaeological excavations or surveys can occur anywhere humans have lived and left material evidence of their lives, which means that the field might just as easily be an urban space or even a university campus. Field sites can also simultaneously be something else (e.g., a corn-field, a tourist destination, a place or worship or a national monument) without ceasing to be a site of scientific investigation, demonstrating that there is nothing intrinsic to the place itself that makes it an archaeological field except that the material within it has become the subject of archaeological investigation.[4] The field is instead defined by the practices of knowledge production that occur there: by the way people act (as archaeologists) when they are in the field, in contrast to how they act (still as archaeologists) when they are within other professional spaces such as universities or conferences.

The lifeworld of The Field

In exploring the social experience of the field and particularly the ethic of informality that I began to describe in the previous chapter, I take seriously jokes, stereotypes, and aspects of the discipline's sociality that might otherwise be considered trivial, such as the idea that archaeologists are down-to-earth, roguish, and prone to wild behavior and parties. Archaeologists in the field are stereotyped as unkempt and jovial, working hard but also partying hard. Field trips are characterized as places of heavy drinking, late-night partying, secret and not-so-secret romantic affairs, and practical jokes. In my fieldwork among Chilean, Bolivian, and North American archaeologist I certainly came across plenty of field teams that lived up to this reputation and I also encountered such tropes in my own archaeological field work in the US, Europe, South America, and the UK. The manner in which a particular group of archaeologists performs this ethic of informality might vary: for example, consuming hard drugs was a common part of being in the field in one country but not others, and earlier I discussed contrasting

4 Consider, as an extreme example, experiments in the archaeology of the contemporary which began with Lucas and Buchli's (2001) excavation of a council house in London that had been vacated by its former occupants only days prior. The space did not cease to be an apartment or a home, while it was also an archaeological field site.

performances of silence and loudness among Bolivian and North American archaeologists. But we can see these as structurally similar performances in that they all draw on a contrast between the carnivaleseque liberation, comradeship, and physical and social isolation of the field in contrast to the more formal and ordered activity of professional life at home.

One way to illustrate this image of the way one acts/who one becomes in the field, is through digressions from it. The Tiwanaku Project was to a certain extent characterized as the non-normal project because the participants did not drink or party much and there were no sexual or romantic intrigues going on. The relative 'calmness' of the project was highlighted in comparison to another project in the region, which had a reputation for rowdy nightly parties that lasted until the early morning and frequent romantic 'dramas' among the personnel. Tales of the annual parties held be a group of projects in this region became legendary, including the field season where several people ended up visiting hospital for broken bones sustained while engaging in drunken chicken fights.[5] Talking with a Bolivian archaeologist about her decision to work on the Tiwanaku Project, she was concerned that working on the contrasting project would be a better career move (the director was held in high regard as an academic), but she knew she would be uncomfortable with the drinking culture and unable to work effectively with only a couple of hours sleep each night. Similarly Matías, the director of the Chilean PANU project discussed above, was known for having what were either characterized as 'boring' or 'calm'

5 Several years later a Bolivian archaeologist explained that when, after one of these parties, a North American grad student had go to hospital with a head injury, it had almost caused a problem with the police because fights legally have to be reported and a young foreign woman with serious cuts to her head attracted attention. The archaeologist telling me the story explained that the Bolivians accompanying the injured foreigner thought quickly and told the officer "They doing crazy Gringo games. What can you expect?" This was apparently enough of an explanation to avoid a police investigation. But lest this be taken as an indication that the person telling me this story agreed with the police officer that only Gringos engaged in such behavior, the same conversation included an anecdote about a Bolivian project that had recently been shut down because the project members had, according to this informant, spent all their research money on beer and each morning the archaeologists were too hungover to excavate.

projects, depending on one's perspective. He was well aware that his policy of actively discouraging drinking, parties, and other 'wild' activities discouraged some people from working with him and was seen as odd. Meanwhile, students on an archaeological field-school I was involved with in Chicago were teased that they were not getting the 'real field experience' because they all went back to their own homes at night and thus never socialized with each other. And during the TVAP field school, which had only two male undergraduates for nearly twenty women, there were constant jokes about how the lack of 'field romances' meant it wasn't a *real* field school experience. The image of how 'wild' one's experience in the field *ought* to be, is only reinforced by the disappointment or awareness that this *particular* experience is not transgressive enough. Whether the ideal field experience was wild or calm, however, it was always seen as a break or separation from normal behavior back home. The field might be fun because it is a place to sit quietly outside gazing up at the milky-way, miles away from the nearest cell phone or computer; or because it involves getting drunk each night and dancing with someone who isn't your girlfriend: but both become pleasurable in part because they are transgressions from the restraints of 'normal life' back home.

The stereotyped image of the 'wildness' of fieldwork can thus be understood in terms of carnivalesque inversion. Fieldwork is a temporary break from normal rules of behavior, but the very awareness of this opposition draws attention to the reincorporation of the same norms when the field is left behind. As such, the act of breaking a norm will always be in reference to it. Drinking heavily, taking drugs, or staying up all night to dance on the artifact lab's table—these actions only make sense as enjoyable transgressive acts in the field if they are not part of the normal social world back home. Likewise camping outside, washing in a river or bucket, sharing a house with one's adviser, or engaging in manual labor were all described as being pleasures of fieldwork by different people. But these are enjoyable only to the extent that they are contrasts to one's usual living and working environment in an (urban) university. While a Chilean student, for instance, *loved* working near the small rural town of San Pedro de Atacama, she still thought it odd that another archaeologist had moved there permanently. The ruralness, indigeneity, and picturesque calm of the town were delightful during fieldwork, but she imaged that living there all year round would be horribly boring. The experience of the field is pleasurable and meaningful through its contrast to life away from the field. But this also underlines why experiences of the field will be different in different archaeological communities, because there are significant variations in the structure of professional life (in terms of archaeology as a form of employment, for instance) for Chilean, Bolivian, and North American archaeologists.

To illustrate the way contrast between the field and non-field is marked and commented upon, consider clothing. North American archaeologists dress very differently in the field and at conferences, and the contrast between these two is something I heard frequent jokes and discussions about.[6] Reuniting at annual meetings like the *Society for American Archaeology* or the *American Anthropological Association*, friends and colleagues who last saw each other in Bolivia or Peru exchange joking compliments on how well they 'scrub up'. In the field men and women wear very similar androgynous and practical clothing. Hiking boots, scruffy pants covered with pockets, thick shapeless sweaters and down jackets; layers and layers of thermal

6 Conferences too are places that can have a carnival-feel, particularly the famously raucous Andeanist hotel-room parties held at the AAA and SAA, the location of which is disseminated by word of mouth. But here the balance is a delicate one: a conference is both a space where the contrast with the field is constantly made by reference to appearance, but where a small part of the field is also recreated through the field-like parties. A conference thus combines heightened aspects of both the home and the field. The clothing worn in university spaces would perhaps be a more appropriate comparison, but I have chosen to focus on conference attire because of the *frequency* of jokes and commentary on people's clothing, male and female, that occurs at conferences. For a discussion of the contrasting significance of clothing and the field/conference distinction among Chileans, see Chapter 7.

under-ware; t-shirts, gloves, and scarves; and woolen or felt hats rammed permanently over dirty hair. At conferences men wear suits in muted blue, grays, and browns with ties, or perhaps good quality jeans and a sports jacket over a button-down shirt. Women wear pencil skirts or suit pants, high-heels, fitted jackets and colorful blouses, with dangling necklaces, polished nails, and smart make-up and hair.[7] [8] Conference outfits are versions of 'office wear'—clothing that is highly gendered, but rendered so by society-at-large rather than the archaeological community specifically. Field clothing conspicuously contrasts to office wear because it is less gendered, is low-cost, and low-maintenance. To wear the same pair of shapeless and stained cargo pants to work each field season is as much a statement about *not* being the kind of person whose professional identity includes having to wear a pencil skirt every-day, as it is a matter of practicality. After all, there is no inherent reason why one *couldn't* wear smart or gendered clothing in the field. Thus North American Andeanists' clothing is a conspicuous and conscious means of a) constructing a contrast between two aspects of one's professional persona, that of the field and that of the non-field; and b) demonstrating how the ethic of informality central to archaeology is formed through contrast to other professions or sciences that are perceived to be more formal.

Pushing this further, we can think about the conceptualization of field work as not-office-work by exploring the class connotations of both the clothing worn and the work undertaken.

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7 The cost of such outfits can be a worry for North American graduate student archaeologists who often live on very restricted budgets. Addressing this issue directly, one professor, actively concerned with mentoring her female graduate student and serendipitously of similar size, would lend or gift items of clothing when the student couldn't afford to purchase her own 'conference outfits'.

8 See also the discussions on the popular academic etiquette site *The Professor Is In* and on the anthropology blog *Savage Minds.* E.g., McGranahan, Carole. "Conference Chic, or, How to Dress Like an Anthropologist" Savage Minds November 20, 2013. Accessed 4 Dec 2013. http://backupminds.wordpress.com/2013/11/20/conference-chic-or-how-to-dress-like-an-anthropologist/ and Kelsky, Karen. "How Not to F**k Up Your Conference Interview" 15 August 2011. Accessed 4 Dec 2013. http://theprofessorisin.com/2011/08/15/how-not-to-fk-up-your-conference-interview/

Undertaking physical labor in the field was another aspect of fieldwork that people would comment on as enjoyable, particularly in contrast to the kind of work undertaken at home: writing, lab analysis, teaching, and university administration.[9] As discussed in the previous chapter, the different values attached to manual and mental labor are a factor in the discipline's problems in legitimating its knowledge claims: tacit knowledge and manual labor are understood as being less skilled or authoritative, to the extent that the embodied expertise necessary for bringing into being archaeological facts is blackboxed. But the physical labor involved in field work can also be a point of pride—the image of Flannery's mythical Old Timer comes to mind (1982). I suggest this can also be seen as a matter of a controlled breaking of norms. If archaeologists engaged in manual labor full time it would take on a different connotation. Archaeologists would simply *be* manual laborers, with the associated low socio-economic status. Manual work that involves getting one's hands dirty can be enjoyed because it is indulged in for a short period each year, as a break from the other 10-11 months of non-manual work back home in one's university—and perhaps even in an office (c.f. Shortland 1994: 33-8 on miners and geologists). In fact, archaeologists who *do* excavate all year around in contract/commercial archaeology have considerable lower status as 'non-scientific' archaeologists. The physical labor of the field is made meaningful through its contrast with the non-physical labor undertaken most of the year back home in the university, in the sense of being associated with or evocative of romantic working-class ideals without actually being a working-class job. This echoes the divisions between oceanographers and the crews of their research ships discussed by Bernard and Killworth (1973). The oceanographers relished the social-ambiguity of their temporary life at sea

9 This was less common in Andean archaeology because on projects in Bolivia archaeologists themselves are less likely to physically excavate.

because it was a periodic break from their normal university routine. But for the crew, the ship was both a source of employment and their year-round home. In the crew's eyes the scientists' sloppiness, lack of sea-faring expertise, and disrespect for the crew-members' skills were a source of constant antagonism—one understood as an unbridgeable *class* division.

The comparison to office work and the stereotypy of wild partying are undoubtedly part of archaeology's ethic of informality—the idea that archaeology is less formal and more egalitarian than other disciplines. In the previous chapter I discussed this concept in relation to the difficulty of performing appropriate informality across cultural divides—a distinction that also has an economic/labor component, as many of the Bolivian students discussed in the previous chapter were working on projects for pay and thus were more likely to worry about losing employment if they engaged in too much drinking or partying. The ethic of informality is not confined to the Andeanists working in Bolivia, however, as the Chilean PANU project described in this chapter illustrated. In terms of day-to-day sociality, the ideal of a team of colleagues working together with few personal divisions and little formal hierarchy except for the director, is hard to achieve. Being in the field requires being constantly around the same people, 24 hours a day, for weeks or months at a time. And as Martina and Luciano's frustrations suggested, fieldwork can become oppressive and frustrating when striking out alone or being pushed forward as the 'boss' is seen as a betrayal of collective cohesion. Equally, while the intellectual collaborations that conducting research together allows were frequently described as intellectually stimulating and exciting, it can also be tiring to work in such close and intense contact. As Amanda, a Bolivian archaeologist, described:

> Because [sometimes] you feel too suffocated when you are part of a big group.

> And you as a scientist or as a digger, you need to take time in many things. And you have five or six people over you... you have to *balance*! You have to be very creative with these people [the workers, standing around waiting for you]! … Sometimes I feel the necessity to be on my own. Because it's a work of patience.

Field science communities are by no means the only place where a balance is necessary to preserve the harmony of a small community, but the intensity of the 24-7 living and working conditions are not found in many other sciences or academic disciplines. It is also the case that the ideal of a group of researchers and friends happily working together in close quarters is sometimes achieved—there is no reason to assume that it is always an unrealized ideal! Senior archaeologists who had become directors would speak with some wistfulness of their former lives as members of the excavation team, now they had responsibilities that took them away from the details of excavation but also cut them off from the camaraderie of being just one of the gang. The isolation of being the boss, in contrast to the positive values they associated with fieldwork as an experience of shared responsibility and team-work, was one reason given by the three co-directors of the Tiwanaku Project for their unusual directorial collaboration. The experiences of camaraderie in the field would not necessarily continue back home in the university, however, structured as it is in terms of seniority and cohorts. Again, this serves to make more meaningful the distinction between the experience of being in the field and not-being in the field as a structural opposition.

How a field becomes The Field

On a different excavation in the north of Chile, over the course of a week, we worked in teams of two putting numerous 1x1 meter square test units into the fine sand of the desert. Each morning after breakfast we would drive into the desert, divide into pairs, and walk off to where small scatterings of stone could be seen poking through the rolling sand dunes. My partner Emilio and I would measure and mark out a square with nails and string, remove all the sand within the square as best we could, write notes on what we found, push the sand back in again, then walk onto the next one. For a full day we found nothing in any of the squares other than the large stones: but each of these areas became, for an hour or so, part of our field. At the end of the day I could not have located the places we worked on again; nor—it turned out—was there anything materially to distinguish those 1x1 meter squares of yellow sand from the rest of the desert. But our actions had briefly made those squares of sand into a field.

The field is not defined by its geographic location or its materiality: it is not a particular place or physical thing. The field is instead defined by the way archaeologists act within it during a specific period of time—how they conceptualizes and handle objects, their performances of archaeological expertise, and their sociality, particularly the ethic of informality. In this way the field is created through practice—but like the squares of sand, it can stop being a field once those practices end. Thus in addition to not being an institutional or technical space, the lack of temporal or spatial permanence is perhaps the most compelling contrast with the laboratory. This still, however, leaves open the question of how epistemically secure knowledge that is meaningful *beyond* the field itself is created within the temporary lifeworld of the field.

PANU project, Monday, 3pm. Josefa and Ignacia stand discussing what to do next in their excavation area. Noticing me watching, Ignacia turns to tell me that they are going to excavate it in natural rather than arbitrary 5cm levels. She points to three separate soil features they have identified in the 2x2 meter quad. One a lighter color sand, the other two darker. We will dig the *lighter sand first, she says, and I ask why. She leans over with a trowel. "I think underneath it is this darker soil", adding that this is just "a hunch". Before they start they describe each layer. Josefa stands on one side with the notebook and writes down the descriptions that Ignacia, bent over the ground with a trowel, calls out to her—but they discuss them together before agreeing what to write down.*

When these initial descriptions of what they can see on the surface finished, they begin removing the lighter sandy layer with brushes and in the process explain to me what kinds of things they usually write down in the notebook. A description of the soil in terms of its color, texture, the ratio of sand to loam, its compaction, granulation, and its inclusions; the extent of this soil feature, its depth and placement; sedimentology, and this feature's relationship to other soil features and the material found within it. Josefa points out that she has also drawn a sketch map of the three different areas they have identified, and then after listing the above they both agree that "mainly your write your impressions." This includes, they explain, a narrative of the process: describing what they intended to do and what they actually found. Ignacia emphasizes this point. When they get home next week they will type up the hand-written field diaries on a computer and email them to Matías. With a smile Ignacia explains that you have to be "honest" and "serious". The temptation is to "tweak it" and make it look better—in other words, it is tempting to write a narrative that is clear and purposeful, that starts with you expecting or knowing that you would find what subsequently was revealed. Instead you have to be 'honest' in recording what you thought it was at the beginning, and therefore why you took a particular course of action, rather than re-writing it to reflect what you actually found so that you look better.

Objects of enquiry become literally cleaned up as they move out of the field: they are transformed from physically and conceptually messy material objects like darker and lighter soils, into ordered and narrativized textual concepts in handwritten notebooks, before being cleaned further into typed documents that can be sent electronically from one person to another and included with hundreds of other documents that Matías and his collaborators will analyze back in Santiago. In the process of moving and being cleaned, usable and useful epistemic objects are created. The soil itself is not the end product of archaeological practice: archaeology's objects of enquiry are the relationships that are being mapped and established through practices similar to those of Ignacia and Josefa. The salient information in the exchange above was the stratigraphic relationship between the lighter, sandy soil and the two darker soils. The information Josefa wrote down served to record for the future *how* the two women knew, as they stood there, that there was a distinction between two different material entities, and how they interpreted this spatial relationship between them. If the forms are taken at face value (rather than, as I discussed in the previous chapter, as mimetic devices), during later analysis it will not be necessary for Matías to see the soil himself to trust the existence of this material relationship: if he trusts Ignacia and Josefa's judgment of it, he only needs to know as much about this relationship as they have recorded. This knowledge will leave the field, while the way this information came into being on this particular afternoon and in this particular location will be left behind—Ignacia's hunch, her and Josefa's discussions, the advice that Matías came by to give them a few moments later, their jokes about being more careful because I was watching them, the excitement they felt about being outside in the countryside and in a place they understood to be more authentically indigenous than their home towns—everything that made up the lifeworld that had temporarily been constructed in this space for a limited period of time, within which this knowledge could be brought into being. This is true of most sciences—few record the context of knowledge creation. However, unlike the lab, the field *is* this context and has no physical or conceptual existence without it—it comes into being as an epistemically productive space *only* as a result of all these transient social practices.

Moreover, movement away from the field and the dismantling of this lifeworld are just as essential to its existence as its construction. People who spend too long in the field without returning, who do not re-situate the knowledge they have excavated by bringing data home to analyze and write-up, risk being identifiable as mere amateurs, treasure hunters, or 'diggers'—people who are not 'real' archaeologists. Archaeologists and archaeological knowledge need to leave the field to become valid. Thus the clearest moral distinction is between scientific/research archaeology and non-scientific/commercial archaeology, or archaeologists who only excavate and never write-up. What gives archaeological knowledge its validity and knowledge-making its moral value is its ability to circulate widely. Archaeologists who only dig are seen as problematic because they are always in the field: they never return 'home' to write up and publish so their knowledge cannot circulate. As I will discuss in more detail for Chile in Chapter Six, commercial archaeology is seen as one of the biggest threats to archaeology and its scientific professionalism.

Given that the field is understood in contrast to the non-field, this is understandable: the temporary nature of the field as a lifeworld is a salient aspect of its construction. If it were permanent there would not be a non-field to contrast it to. In part this works to bring into focus the salient objects, as described above: the 'cleaned up' version of the field is more useful than the entirety of the field itself. If archaeologists' fieldnotes were like an ethnographer's fieldnotes, and included every detail of every conversation about each archaeological object, they would be unusable. But separation from the field also allows each individual field to become comparable with knowledge from other places.

This relates to a problem discussed in relation to other field sciences by Anna Tsing, in reference to the work of Celia Lowe and Corinne Hayden (Tsing 2005: 94-5). Facts and objects need to be purified of their histories and locations in order to circulate beyond their location of origin, because circulation requires that unique things from different places can be meaningfully compared to one another. In describing the development of botany as a scientific field, Tsing argues that achieving comparability required first the creation and then the erasure of global connections between otherwise unique places/things. Collaborations with indigenous and non-western Others were necessary to discover new plant species; but increasingly European botanists began to imagine themselves as communing directly with plants and so the collaborations with non-western Others were erased to create the idea of a universalist science (Tsing 2005: 91). Tsing's point is that universalism requires a double-take: first the creation of global connections that allow objects of study to be made comparable, then the erasure of those same connections to support the idea that the comparability was, all along, inherent.

In the case of archaeology it is not necessarily collaborations with non-western Others that must be erased. The knowledge of the Other, when in the form of indigenous or local communities, does not necessarily pose a challenge to the *archaeological* meaning of objects because archaeological objects and field sites can simultaneously be archaeological entities and other kinds of entities (e.g., a farmer's corn field, a sacred object, dirt) without this being ontologically problematic. But *within* the archaeological epistemology a similar kind of universalizing double-take is necessary, that first makes global connections through positing that all fields are comparable, and then erases those connections by demonstrating that the comparability was, all along, inherent. Thus the field is always remade conceptually through being understood in relation to the non-field. The field is constructed in contrast to the non-field, home, and then this act of construction is erased so that they both appear, all along, to have been inherently oppositional.

When 'global' or universal and 'local' are contrasted to each other, however, the implication is that the local is specific, unique, and particular, while that which is global is abstract, comparable, able to have relevance everywhere (Weismantel 2001: 3, Basu 2003: 69). But Charles Briggs and Carla Mantini-Briggs (2003) remind us that we should be skeptical of the idea that global = abstract and local = particular. Looking specifically at how claims to globalness authorized medical and scientific knowledge, when tracing narratives that sought to explain a cholera outbreak in Venezuela in the early 90s, Briggs and Mantini-Briggs argue that, ironically, those seeking to abstract knowledge and make it circulate relied on and propagated images of local places that did not acknowledge the reality of these places' global connections. Abstraction requires that the local is conceptualized as timeless and spatially cut-off. When a place is situated within its social, political, geographic, and economic context, it is obvious that even rural, indigenous, 'backwards' places are connected into global networks:

> [I]t was public health officials – the supposedly global subjects – who localized the epidemic. In contrast, many of the quintessential locals – the members of an 'uncivilized' population – sought to make the most global connections they could imagine, linking cholera to global capitalism, the marketing of local culture in national (or nationalistic) arenas, and the transnational politics of race. The officials abstracted cholera from time as radically as the extracted it from space (at least any conception of space that extended beyond the delta), imagining unchanging, inherent properties of the environment and millenarian beliefs and cultural patterns. The locals, however, tied cholera to the social, political-economic, and environmental parameters of the historical moment in which the epidemic occurred. (Briggs and Mantini-Briggs, 2003: 252)

This can help us understand how the field in archaeology can be both a place that is localized and yet can also be made circulatable. For archaeological knowledge to circulate, it needs to be detached from the specificity of the place in which it was created. This can be achieved by imagining each field site as comparable, rather than unique. As Tsing describes for the creation of universalist botanical knowledge, however, this requires a double-take. Connections are made between disparate and unique places all over the world, then the act of making these connections is bracketed out in order to make their similarities appear inherent. This can be achieved by seeing every field site not in terms of its uniqueness, but as comparable instantiations of the same thing: many instantiations of 'The Field' rather than a multitude of fields. The Field still keeps its associations with 'localness' in comparison to the non-field professional home as a 'global' space, but in the sense implied above by Briggs and Mantini-Briggs: to the extent that local places that are rural, indigenous and far from home are assumed to be timeless and outside of the rest of the modern world, in comparison to the scientists' 'home' which is always more modern, more globally connected, and more rational in comparison.

Each Field is thus always structurally and conceptually the same because the lifeworld of The Field is created through the same set of practices, rather than being an intrinsic part of that physical location. So each field site becomes another instantiation of The Field: an interchangeable 'local' space which is defined through its opposition to a controlled, rational 'home', rather than the actual specifics of the particular location.

But the specificity that is bracketed out in leaving the field includes the relationship between local economies and field methods, the contingencies of choosing specific excavation locations, the processes of argumentation and embodied expertise through which knowledge is actually created, and so on. And also, inevitably, such broader historical and political contexts of local places that make them tied to global processes, such as the involvement of the US throughout the 20th century in South American political and economic life that mean the South's claims to global rationality appear quite different, today, than those of the North.

Why the South cannot be not-the-field

The structure of the field : non-field relationship is relatively basic—it is in the specific cultural-historical enactment of this structure that the difference between the two is experienced. As a result, the field will be conceptualized and experienced differently by a Bolivia, Chilean, British or North American archaeologist. The Chileans described at the beginning of this chapter expected to encounter the field as an indigenous and rural space that was different from Santiago, and in a parallel but different manner North Americans working in the Andes expect to encounter a field in Peru or Bolivia that is foreign in comparison to the US or Canada.

This model of the field can help us understand why archaeologists from the South face problems being recognized as equally expert by their North American colleagues. For North Americans working in South America, the experience of the field begins as soon as they step onto an airplane. The city of La Paz is as much The Field as the small-town plaza of Tiwanaku; the cosmopolitan streets and cafes of Santiago, as much as the Tarapacá desert. The Field is strongly associated with travel to foreign countries—which over the years might become very familiar and well-known spaces, but are still 'not-home'. As a result, national archaeologists encountered in these spaces inevitably become associated with the field rather than 'homw': the (still archaeological) 'non-field' which is the field's opposition. Bolivian and Chilean archaeologists' bodies and their knowledge remain tied to the field site and all the problems of contingency and negotiation of authority that are contained within it. Indigenous people are clearly definable as completely Other. But local Bolivian or Chilean archaeologists are not categorizable as either enough like ‘us’ nor enough of an ‘Other’ to be either entirely of the field or entirely of home. As long as Southern archaeologists are unable to travel to conferences and universities in the North and as long as Northern archaeologists avoid universities and conferences in the South, this is likely to remain a problem. In the second half of this dissertation I will explore the wider context of academic life, as structured by the changing culture of universities in both hemispheres, that make it difficult for this to happen.

Of course many Bolivian archaeologists also see themselves as going into a field that is separate from their own home: they are traveling from the city of La Paz to the countryside and for them this is a substantial contrast. That the difference between urban, middle-class, mestizo academics and Aymara campesinos can be collapsed so easily by foreigners should be a matter of concern (see also Weismantel 2001: 28, 76). But the tendency in US archaeology to associate the field with foreign travel is also felt by archaeologists in the US who work in non-rural US locations: historical archaeology in urban settings, such as the Chicago field school I mentioned earlier, has long carried the suspicion of not quite being ‘real’ archaeology.

The association of the ‘global’ with abstraction and disinterested rationality and the ‘local’ with parochialism and particularist interests has far reaching implications. North American archaeologists can work as easily in Southern Peru as Northern Bolivia—and not only because it is easy for them to acquire visas to cross geopolitical borders, but also because the ease with which they can exchange one site for another is taken as a further demonstration of *their* ability *not* to be tainted by local specificity, and thus to be *more* able to make abstractions than their ‘nationalist’ colleagues.

The Field, as a non-specific, comparable, 'local' entity, is a decontextualized space outside of normal life, separated from politics, culture, and social hierarchy. In practice, of course, the fields in which archaeologists work are never encountered separately from their historical, political, or cultural specificities, nor devoid of hierarchical relationships organized according to nationality, race, gender, and class. In the following chapters I will elaborate on this argument, by exploring the extent to which the ability to conduct research outside one's own country is associated with a supranationalist claim to hyper-objectivity.

Chapter Five

Neoliberalization of the university: from theory to practice

Introduction

Archaeology might be associated with fieldwork, but archaeologists spend the majority of their time each academic year back 'home' in a university. Understanding the circulation and production of archaeological knowledge thus requires that we look at practices in universities as well as in the field. In the second half of this dissertation I continue to explore *how and why* the North is imagined to be inherently more 'global' than the 'local' South in these other spaces: the universities archaeologists come from and return to when their excavations are finished. In these chapters the focus will widen from a concern with the specific lifeworld of the archaeological field, to the construction of academic knowledge, expertise, and professionalism at a national and supranational scale as seen more broadly through the example of archaeology.

My overarching argument is that one cannot understand the conditions of scientific practice and labor in universities in North and South America without engaging in a conversation about neoliberalism. Over the last few decades, universities in both continents have been transformed according to a neoliberal conceptualization of knowledge and professionalism, in the process making universities as institutions essential components in the process of transforming 'neoliberalism' from a theory into an explicit set of practices and cultural meanings. As a result, during this process universities have become central to national and supranational projects of citizenship and sovereignty, expertise and authority, and struggles over democratic participation. On the one hand I take 'neoliberalism' to be a social and economic theory, discussed within the context of economic departments and academic texts. On the other, however, I intend to trace how neoliberalism becomes a set of practices and meanings that touch ground in policy decisions that appear to 'come from nowhere', but in fact are the result of specific actions by international and national agents. To illustrate this argument I will focus on the example of Chile between 1970-2010: the period just before the 1973 military dictatorship, through the 1990 return to democracy, and into the period of my ethnographic research with Chilean archaeologists.

Chile serves as an important example for several reasons. The first is that Chile was always considered a test case for neoliberalism. The dictatorship that came into power in 1973 violently and explicitly translated the economic theories of Milton Friedman into economic policy, years *before* the same policies were put into place in Reagan's United States or Thatcher's Britain. Neoliberal 'reforms' of the universities were central to every stage of this political and economic transformation—and to its resistance. Chile thus serves as an explicit example of the centrality of universities to the transformation of neoliberalism from theory into a project of social transformation at the national level.

The second reason for looking at Chile also illustrates why archaeology is a good place to explore the supra- and transnationalism of the neoliberal university. In contrast to places like Bolivia, Chilean archaeologists have resisted the kinds of collaborations with North American archaeologists that were described in the previous chapters, in part because of the historically and culturally specific meanings of professionalism and education that Chilean archaeologists hold. These meanings, I argue, are the result of thirty years of resisting the neoliberalization of their universities—spaces central to the education of future members of the archaeological community but also to conceptualizations of archaeology as a science, a profession, and a form of expert knowledge.

I use the example of Chilean archaeologists, therefore, to explore the movement and translation of both people and meanings between supranational, international, and national contexts, and show how assumptions of universalism and easy comparability—both central components of the philosophy and practice of neoliberalism—collide with the reality of cultural difference and North-South inequality. In effect I will argue that the questions of race, colonialism, and nationalism that were being implicitly bracketed out of the knowledge-production processes during archaeological excavation, as described in the previous three chapters, have also been bracketed out of conversations about university reform. However, as the massive and on-going protests against neoliberal education reform in Chile demonstrates—protests that have brought thousands of people onto the streets in the last three years and are being seen as the biggest social movement since the uprisings that toppled the dictatorship—these issues are becoming ever harder to ignore.

The next three chapters therefore illustrate how control of universities, and through universities control of both the education of future citizens and the development of the professional classes, is central to the creation of a neoliberal social system; and explore the implications of universalism for an understanding of nationalism/supranationalism in relation to academic knowledge. I intend to make this argument in three steps, each of which will be the focus of a single chapter.

First, neoliberalism as an economic theory rests on the idea that all individual and collective human action and motivation can be explained according to the rational pursuit of economic gain; from this comes a very narrow theory of value and specifically of the value of knowledge. Knowledge is understood as a kind of bounded, measurable *thing*, valuable at the individual level but not at a collective level. Universities are primarily conceptualized, according to this theory, as knowledge-making and knowledge-transmitting institutions. Additionally, neoliberalism is a totalizing theory: one that argues that *all* human action can be understood as being fundamentally the same and motivated by the same basic economic motivation. The process of creating *educational policy* from this theory thus rests upon the assumptions that a) changing the institutional structures within which people live their lives will encourage, enable, or force them to become their 'true' neoliberal selves, and b) this will be equally applicable in any country. As sites of education, universities have been central to the process of neoliberalization because they are perceived to be the spaces in which individuals' entrepreneurial, rational impulses can be focused and skilled through the acquisition of relevant knowledge. This is universalizing because it imagines that both people and knowledge are standardizable. The tension comes from the extent to which this is treated as being an already existing, naturally occurring condition, or something that must be brought about explicitly through changing the norms of participation and discourse.

Secondly, there has been active resistance to this ideology and the policies it underpins. Chile is an example of a very aggressive attempt to transform the institutional structures within which people live, as a means of bringing about a truly neoliberal society. This occurred through a dictatorship initially, but since 1990 through democratic processes. Despite, or perhaps because of, the brutality and explicitness of this social engineering project, resistance has been considerable. Using the case of Chilean archaeologists, I show how academics actively resist the attempt to transform them into neoliberal subjects and to normalize the logic of neoliberalism, but must accommodate their values and practices to the changing institutions in which they work.

This serves to undermine neoliberalism as a credible social and economic theory: people continue to act according to non-economic motivations; and in the case of Chilean archaeologists, they continue to uphold quite different concepts of the role, nature, and value of knowledge.

This is not a simple narrative of heroic resistance, however. The 'they' against which Chilean archaeologists see themselves resisting becomes hard to isolate, illustrating the extent to which something called 'neoliberalism' becomes a nebulous agent, exchangeable and entwined at different moments with 'the State' or 'the US'. Thirdly, then, I explore the problem of pinning down what and who exactly neoliberalism *is*, when seen from the perspective of academics in the Global South. I connect this view-from-nowhere approach to the problem of comparison and the assumption of universalism, specifically to the discourse of globalization in scientific communities and within universities. This will address two issues arising from the ethnography: that US archaeologists assumed a single, global academic community, and that Chilean archaeologists associate the impact of neoliberalism with the imposition of foreign (i.e., US) values.

At stake here are a nested set of questions. How do we understand academic communities to be universal or international in nature, as opposed to national? How is an economic and social theory like neoliberalism transformed into a set of ideological or policy decisions that impact practices on the ground with the aim of transforming society? Who can be understood to be the agent of such processes—and how can we understand this to be happening, in practice, in relation to education and the academic profession? What is the relationship between the US and international organizations like the WTO and OECD that are the agents of neoliberalism as a set of policy decisions? My concern in these questions is to keep agents front and center; to explicitly problematize the degree to which neoliberalism can become a nebulous, all-encompassing and disembodied concept, something always imposed from outside, or easily interchangeable with equally non-specific entities such as 'US Imperialists'—an argument that chapter seven will explore in more detail, by looking again at how English language texts and US/European systems of academic accreditation, circulate among an imagined supranational communities of academics (c.f. Anderson 1987).

A critique of universalism and what I later refer to as *supranationalism* runs through this analysis. While in the previous chapter I argued that archaeological knowledge needs to be made universalizable and circulatable to have meaning beyond the field site; here I explore the implications of the association of the 'global' with abstraction and disinterested rationality, and the 'local' with parochialism and particularist interests, when some places are always seen as more global than others.

I therefore focus on Chile to demonstrate the problems of universaliziation: both the univeralism of neoliberal theories of education, and the universalism of North American academics who assume that they share a single disciplinary and educational culture with their South American colleagues. To make this case I need to move back and forth between non-country specific generalizations that might use examples from any particular country as illustration (but primarily drawing on the US in order to explain and make recognizable a neoliberal theory of education to a presumed US academic audience); and the specific case study of Chile that shows how these generalizations work out in a particular historically and culturally grounded example.[1] I will therefore begin this chapter with a broadly historical explanation of

1 Another reason for the less country specific approach I take in the first half of this chapter is that the non-anthropological literature on the neoliberalization of higher education tends to focus on country-specific case studies. Anthropologists have paid remarkably little attention to higher education and I was unable to find other anthropological ethnographies of universities. What has been written by anthropologists falls mainly into the category of informed self-reflexion and internal institutional critique, for instance Strathern's (2001) influential

neoliberal theories of education and knowledge, before looking in detail at the Chilean ethnographic case study in chapters six and seven.

Neoliberalism as a social and economic philosophy, in relation to higher education

Accounts and analysis of changes to higher education within the Global North written by those working in universities in the US—for instance in the form of articles in professional magazines such as the Chronicle of Higher Education and the Times Educational Supplement, articles circulating in popular media, or books written for both academic and general publics—tend to talk in terms of crisis and decline (e.g., Hersh and Merrow 2005, Newfield 2008, Deresiewicz 2011, Schrecker 2010, and Kroll 2012). The university is under attack, the profession of the brink of collapse, and whatever the cause or solution proposed, it is clear that something is drastically wrong. From another perspective, however, the current situation is exactly the intended outcome of a series of policies that deliberately aimed to produce the effects that are now taken as symptoms of decline. Increased job insecurity for professors and teachers as a result of adjuncting and linking pay to test scores; the deprofessionalization of teaching and the centralization of curricula; higher debt for students; a decline in subjects that require complex and critical reflection in favor of those that focus on easily compartmentalized facts and skills; and greater

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work on audit cultures; or looks at university graduates as a means of exploring other transnational circulations (e.g., Ong 2006). In 2000 the anthropologist Richard Wisniewski speculated as to why anthropologists have been reluctant to do ethnographic research in the same institutions they tend to work within. But his call for more ethnographic attention to be paid to universities by anthropologists of education has, to date, gone unanswered. In contrast, a great deal of high quality research has been conducted by other social scientists, notably sociologists. Additionally there are many fine historical studies, and self-reflexive commentaries by academics from the humanities. These case studies come from a very broad range of places, but concentrate around countries that have been at the forefront of neoliberalization: New Zealand, the UK, the US, and post-dictatorship countries in South America that previously had very strong democratic university systems such as Chile, Argentina and Brazil. I draw on these works extensively in these chapters, while hoping that this project will make a strong case for how anthropologists undertaking ethnographic research can add to these discussions.

competition for scarcer resources: all are seen as negative outcomes by those working within universities, but as positive and desirable results if considered from the perspective of a neoliberal philosophy.

Neoliberalism as an economic and social philosophy constitutes a transformation of the ideas of Liberalism to extend the logic of the market to all spheres of life. People are no longer conceptualized primarily (or even at all) as citizens or members of cultural or social groups, but instead as individual, self-interested competitors and consumers, who chose courses of action based on rational decisions that will enhance their own economic advantage. Central to this logic is the belief that competition and self-interest are foundational human characteristics, over and above other motivations arising from 'local knowledge' such as culture or religion. Because individuals' decisions are always economic decisions, all spheres of social life can logically be organized as a marketplace.

The neoliberalism advocated by economists such as von Mises, Friedrich Hayek, and Milton Freidman was a reaction to Keynesian economics and the development of social welfare following the Second World War. If people are only motivated to any form of action by economic advantage, public organizations such as public health, transport, and education will always be unproductive because the individuals working within them cannot make a profit from their work. Without the incentive of making a personal profit, public workers have no incentive to do their jobs. As a result public institutions are always less effective and efficient than private, profit driven institutions. Further, dependency on state resources such as unemployment insurance stifles individuals' natural self-interest and assertiveness, because without immediate economic need they have no motivation for action.

These precepts contradict each other when combined, however, resulting in an argument in favor of state protection of the market even when state intervention in individuals' lives is deemed problematic. The contradiction occurs because if individuals working in institutions are only motivated by their own economic advantage (and not, for example, because as doctors they want to make sick people better, or as teachers they want to help children learn), the most rational course of action would be to accept a pay check but do no work, or to do work that benefits the individual but not the institution as a whole. Privatizing institutions so that workers will be incentivized by the ability to make personal profits, only makes sense if the incentives of the individuals and the institution match up: something that will not be the case, according to the same theory of self-interested individualism. Individual workers thus require constant monitoring, auditing, and cajoling to force them to align their own economic interests with that of the institution.

This accounts for the difference between liberalism and neoliberalism. Liberalism holds that the market, as the collective expression of individuals' self-interested and rational economic choices, is the only force capable of rationally organizing society. The state is always an impediment because it is a rival form of organization and agency. Neoliberalism drops this idea of market equilibrium, and argues instead for a small but market-friendly state that uses intense, constant auditing to force individuals to align their interests with that of the market (Ward 2012: 23). In practice this means that public institutions are treated in the same way as individuals: as in need of constant coercion to avoid non-productive, 'selfish' behavior.

> On the one hand, the market is to be further deregulated and allowed to function with minimum oversight. However, agencies that use tax dollars, such as schools, hospitals and universities, are audited and regulated more intensely and frequently than ever before since they are, in the neoliberal conceptualization, dependent on private wealth and property for their existence. (Ward 2012: 43)

This results in what Strathern et al (2000), drawing on Power (1994), have described as 'audit culture'—a situation whereby individuals and bounded groups are forced to monitor themselves in a panoptic fashion, and to take on the burden of their own incessant accounting and monitoring.

The description of neoliberalism outlined above is necessarily very simple; there is not space for a more detailed historical discussion. But it is also simplified to allow us to focus on the most basic assumptions about human nature that underlie any neoliberal argument or policy decision. Such assumptions are invalid: human beings act according to many more motivations than simply economic gain (Solimano 2012: 40). As a discipline anthropology is predicated, one could argue, on the fact that human action is not motivated by a single, universal economic rationale. It might seem strange to explicitly state that neoliberalism is a social theory with little evidential basis, one that has been continually proved false to the extent that it is an academic theory. But this point needs to remain at the front of our discussion when seeking to understand how a disproved economic theory of human motivation has been transformed into political ideology.[2]

Neoliberalism became the favored political ideology of governments around the world from the 1980s onward. It was embraced by Margaret Thatcher in the UK and Ronald Reagan in the US, and then promoted through their governments' foreign policy. During the dictatorship ushered in by the 1973 coup, however, Chile was explicitly used as an experiment in the

2 Once considered a 'secret sin' (McCloskey 2002), since the economic crash of 2008 there have been signs that economists are beginning to ask this question as well. See for instance: Inman, Philip. "Academics back students in protests against economics teaching" *The Guardian* 18 Nov 2013. Accessed 9 Dec 2013. http://www.theguardian.com/education/2013/nov/18/academics-back-student-protests-neoclassical-economics-teaching

immediate application of neoliberal theory to an entire country, several years before such thinking influenced policy-making in the UK or US.

Universities provide a salient illustration of the way public institutions have been restructured in countries whose governments have embraced a neoliberal ideology. A form of managerialism known as “New Public Management” (NPM) became popular in the 1980s, particularly in professions which traditionally had an autonomous, self-regulating professional culture. NPM serves as a form of auditing, such that “…true decision-making was made and implemented from above and imposed on professionals, not usually in the form of a manger directly observing and directing, but through faceless, ‘third person’ accountability systems designed to insure efficiency, timeliness and adherence to the organization's output goals.” (Ward 2012: 60) It thus is a solution to the problem of having to force individuals to align their interests with that of an institution, and resulted in five key changes to the way universities and other public institutions were organized:

> 1) An emphasis on constant improvements in efficiency and productivity. Results in constant auditing to measure improvements. Because the private sector is considered to always be more efficient, outsourcing becomes the preferable source of action.
>
> 2) The implementation of performance related bonuses and pay. Assuming that without financial incentives managers and workers have no reason to be productive, employees’ pay is linked to measurable outcomes such as student grades or tallies of publications. In practice this has been demonstrated to result in

a profound drop in pay, and increased stress because employees are required to be in constant competition with each other.

3) Development of the idea of “customer service”. Marketization of public institutions like universities requires that someone is conceptualized as the “customer”. In practice, this leads to increased auditing to measure whether customer service is being provided, and PR and marketing departments becoming far larger and more prominent within universities.

4) Devolution and decentralization. To ensure individual departments are incentivized, responsibility for budgets is passed to the local level. In practice this means that individual departments within an organization have to become financially independent, generating enough profit from their services to cover their expenses. This remains coupled, however, to elaborate accountability procedures and auditing, administered from the center.

5) Outsourcing of “secondary” services, such as cleaning or food. (Ward 2012: 53-54)

This list demonstrates how many of the symptoms of crisis mentioned at the beginning of this section make sense when considered from the perspective of neoliberal assumptions of human motivation. These strategies have been applied to other non-university public institutions, such as hospitals, local government services, transportation and so on. But universities and schools can stand as a special case in the restructuring of public institutions because of the way knowledge becomes conceptualized according to the same logic.

Audit culture results in a situation where only that which is measurable and quantifiable can count as real. This resonates with the idea that economic accumulation is the only motivation for human action and thus measuring profit is the only way of measuring increasing value. The focus on quantifying value has the effect of framing audit culture as objective and scientific (Shore and Wright 2000), as much as it frames political decisions based on economics as apolitical (Ward 2012: 37, 72). In educational contexts, however, it means that knowledge must also be *measurable* to be real and valuable.

On the most obvious level this results in an emphasis on knowledge forms that generate profit, for instance through patenting drugs or developing new technologies. It also accounts for the increased emphasis on outcomes that can be quantified, such as the production of peer-reviewed articles (as a measure of professors) and grades (as a measure of students). Rising and falling grades and tallies of publications, like increased revenue from drug patents, are measurable and auditable forms of value. More subtly, however, the application of auditing to universities and schools transforms understandings of the nature and purpose of knowledge, as a logical outcome of seeing society primarily or solely from the perspective of a self-aggrandizing, economically rational, individual entrepreneur. This shift in emphasis is encapsulated in the concept of the "knowledge society" (Hargreaves 2003; Delanty 2001).

Central to the idea of a knowledge society is the conceptualization of knowledge and skill as a thing, like capital, that can be measured, produced, and transferred. A piece of knowledge or a skill can be transferred from a teacher to a student, resulting in the student becoming measurably more knowledgeable and thus more able to be an innovative, entrepreneurial rational actor (c.f. Freire 1970: 71-86). In the case of primary and secondary education, this conceptualization of knowledge and skill as measurable things that can be unproblematically transferred from teachers to students accounts for many recent changes in education policy, but most controversially the rapid increase in high-stakes testing in public schools (Jacob 2005, Hursh 2005, Bovaird et al. 2012, Ferriter 2013). Additionally, when knowledge is conceptualized as a concrete and bounded thing, it does not matter who the teacher or student is: all that is required is an effective method of transfer from one to the other. Knowledge is thus decontextualized and always the same no matter who/where it comes from or who/where it goes to. From this reasoning it follows that teaching is nothing more than a technical position, involving the mechanical and standardized transfer of knowledge-objects. And it makes sense, then, that teachers are interchangeable and replaceable, require only minimal training in standardized transfer methods, and can be evaluated according to the amount of knowledge-objects their students have received and retained.

Teachers, like all self-interested rational actors, are understood within this logic as only being incentivized by economic gain. As rational actors they are also highly likely to seek out and take advantage of any opportunity to gain economically without achieving the institution's aim. The school or university's aim is to educate children; but if teachers act rationally, their aim is to do as little teaching as possible for as much money as they can get away with. Teachers therefore need to be constantly monitored and financially rewarded only for measurable results. In addition, any form of professionalism or guildism should be treated with suspicion because collective claims to hold non-economic, non-rational values (for instance, an interest in or passion for teaching) are unrealistic. Teacher unions or academic associations are thus best understood as elaborate and deceitful strategies to get paid to do something other than what the institution wants the teacher or academic to do: teach students. (A clear example of this logic in practice—and the backlash to it—can be seen in the discourse surrounding the program *Teach For America*. See, for example, Sleeter [2008] and Lahann and Reagan [2011] but also popular critiques such as Olivia Blanchard "I Quit Teach for America" *The Atlantic* 23 Sept 2013, James Cersonsky "Teach For America's Civil War" *The American Prospect* 9 July 2013, and Andres Hartmann "Teach for America's hidden curriculum" 17 Feb 2013 *Salon*)

Within such a formulation of teachers' motivations and values, the ideal situation is one in which teachers are: a) responsible only for delivering standardized course content, because they cannot be relied on to deliver the institution's aim of imparting knowledge; b) are interchangeable and in competition with each other, to ensure incentives; and c) only work individually and have no broader professional affiliations or organization, because these might encourage the pursuit of personal, non-rational values such as doing research. In other words, the ideal scenario looks remarkably like the position of an adjunct professor in a US university or college: one of the thousands of non-tenured, temporary, interchangeable, and badly paid professionals who currently do roughly 70% of college teaching[3]. According to related arguments, unless students pay for education they will have no incentive to take courses.

Picking through the intricacies of these arguments helps us understand why educational policies are proposed and carried out that appear to people who are actually working within universities to be patently destructive. For instance, increasing student debt, the deprofessionalization of faculty, high-stakes testing, and eliminating from the curriculum concepts or entire disciplines that are not based on or reducible to quantifiable knowledge

3 Williams June, Audrey. "Adjuncts Build Strength in Numbers", *The Chronicle of Higher Education* 5 Nov 2013. Accessed 12 Jan 2014. https://chronicle.com/article/Adjuncts-Build-Strength-in/135520/

objects. It also unpins a conceptualization of the relationship between knowledge and society.

Neoliberalism posits that there is, as Margaret Thatcher famously claimed, “no such thing as society”—only the aggregate of predictable individual actions. Knowledge, therefore, can only be conceptualized in terms of its value to an individual, and arguments in favor of knowledge creation for the sake of larger social units *do not make sense*. Knowledge is thus only that which enables individuals to make rational, calculating decisions. This argument both reinforces and comes out of the idea of knowledge as a bounded, quantifiable object. Two implications flow from this. The first is that the university is primarily framed as a place in which students come to collect a sufficient quantity of knowledge-objects to prepare themselves for a life spent making rational, calculated decisions. The second is that academic research has no discernible value unless it is otherwise producing quantifiably-measurable (economic) value through, for example, patented pharmaceuticals or new technologies. Added together, the university is a space in which professors are first and foremost educators who transmit knowledge to students. Anything other than this is a distraction, to be viewed with the same suspicion as public school teachers’ unions.

We can see traces of this logic in recent discourse about universities in the US that assumes that the primarily role of the professor is to teach. A recent article in Forbes magazine listed University Professor as the least stressful job of 2013 (Susan Adams “The Least Stressful Jobs Of 2013” *Forbes.* 3 Jan 2013). The web-version attracted hundreds of angry commentators who roundly criticized what was indeed a remarkably shoddy piece of journalism. But the article made sense (and in some cases was defended) within an understanding of professors and universities outlined above. If a professor’s job is only to teach, then the long summer months without students would indeed suggest that they have a light work load. Likewise, there is little to distinguish an adjunct from a tenured professor, if both are primarily understood to be teachers.

The insinuation that research is somewhat frivolous, only pursued in order to satisfy professor's personal egos, and a distraction from their real job (i.e., teaching students), underlies this and many other related debates.

In a similar vein the recent arguments over Florida Governor Rick Scott's attack on anthropology as a "useless disciple" centered on whether undergraduates do or do not benefit from taking anthropology classes. Anthropology's defenders framed their response in the same terms as Scott—that students do benefit from learning anthropology in college (e.g. Jaschik 2011, Weinstein 2011, livinganthropologically.com 2012). This stands in contrast to an idea that academics collectively generate knowledge that is beneficial or relevant to *society*, not just knowledge that can be used by individual students; that practicing anthropologists have a role and purpose in society at large, other than teaching. The ongoing attempts by Senator Tom Coburn to restrict all National Science Foundation funding to projects that demonstrate economic value or national security, further illustrate the extent to which non-economic knowledge production is considered to have no collective value (Mervis 2013).

So far I have outlined how the move is made from abstract theory of human motivation to justification for educational policy. In the remainder of this chapter I will discuss how this happens in practice.

The OECD as a "Trojan horse" of neoliberalism

David Harvey, drawing on Gramsci, argues that neoliberalization occurs in democratic countries through the construction of consent as a matter of transforming "common sense" (2005: 39). In keeping with my interest in focusing on actors, I will argue that this happens at the national and supranational level in the realm of education through the participation of organizations like the World Bank and the OECD (Organization for Economic Cooperation and Development) through strategies of normalizing neoliberal values and standards of measurement. Other supra-state organizations involved in education policy on a supranational scale include UNESCO, the IMF, and the WTO. But the OECD is particularly invested in education policy, and in 2009 published an important report on Chilean education that I found referenced in both governmental documents and media reports discussing higher education reform in Chile (OECD 2009).

To understand how, in practice, the OECD helps shape university reform in countries like Chile, I draw on a pair of essays that appear next to each other in a volume on global trends in higher education (Malee-Bassett and Maldonado-Maldonado 2009). As the title of the first essay—"OECD Work on the Internationalization of Higher Education: An Insider Perspective"—suggests, it is written by two members of the organization (Schuller and Vincent-Lancrin 2009). The second, "The OECD and its Influence in Higher Education: A Critical Revision", is a response from two education policy academics, based in Spain and the Netherlands respectively (Amaral and Neave 2009). The latter study draws on extensive qualitative interviews with people working in the OECD, conducted by Henry et al (2001) and Marcussen (2001, 2004).

The OECD was originally the Organization for European Economic Cooperation (OEEC), formed in 1948 to oversee the Marshall Aid Plan. But when that mandate expired it morphed into the Organization for Economic Cooperation and Development and extended its coverage to 30 new member states outside of Europe. The aim of the organization is to "… promote economic growth, full employment, and financial stability." (Schuller and Vincent-Lancrin 2009: 64), but Amaral and Neave include the aim of "…assisting countries in their drive to economic development, and finally, to advancing world trade." (2009: 83). It has been centrally involved in education reform since the 1960s, and uses information gathering and dissemination as its primary means of influence, rather than legal or financial coercion. Specifically, the OECD collects information about education in countries around the world and publishes these in several forms, including "reviews" of education policy in a single country (as in the case of the report on Chile mentioned above), and their yearly *Education at a Glance* editions, which are comparative studies.

> The OECD differs from many other international organizations in that it is not a regulatory, still less legislative, body (though with some "soft law" exceptions). It does not distribute financial support, either. Its legitimacy and influence depend to a high degree on the perceived quality of its information and analysis, not on its power as a rule-making body (as with the EU) or as a provider of funds (as with the World Bank). Countries differ in the way that they interpret this knowledge-generating remit … One determinant of these different attitudes is the policy-making structure of the countries themselves, and especially how far the nation state is the unit for making educational policy. The less this is the case the more that countries tend to look to the OECD for information only. (Schuller and Vincent-Lancrin 2009: 66)

Schuller and Vincent-Lancrin also emphasize that the rationale for the OECD's focus on education comes from member governments, who see education as "an investment, following more or less straightforwardly the human capital paradigm" (2009: 63). Amaral and Neave argue, however, that its lack of legal or financial power only strengthens its authority:

> OECD's authority flows from laying down a set of distinctive norms and practices, grounded in what is desirable and appropriate for liberal, market friendly, economic policies (Henderson 1993). It sets out the role institutions should assume in developing and ***handing on those norms that cause actors in a given community to switch to the logic of appropriateness*** (March and Olsen 1989). (2009: 83, my emphasis)

Amaral and Neave explain how this happens in practice through the recruitment of individual civil servants from national governments into the information gathering process.

> OECD routines fulfill a dual purpose. Amongst member country civil servants they create a sense of identity with the particular interpretation the organization currently promulgates: modernity, market friendliness, liberalism, and efficiency (March and Olsen 1998: 961). ***This experience amounts to an induction both pedagogical and, in a broader perspective, a form of socialization into the mental set of an international technocracy and a technocratic rationale.*** In this, the review process is a powerful agent. For the OECD itself, the review process is central. It defines and forges a community of "policy practitioners". It gives civil servant delegates from member countries the opportunity to apply specific knowledge from their particular system in shaping the process of review within the OECD. ***It defines the conceptual knowledge they need*** (OECD 2002: 9). Regular participation in reviewing policy, so the OECD holds, developed a new frame of mind among the participants (ibid 11). Nor does the organization deny its explicit proselytizing. On the contrary, ***'the aim is to have intellectual influence on policy makers'.*** (ibid: 7). … Clearly, the forging of intellectual and interpersonal ties through the procedures and routines within the OECD is a prime and prior condition to the dissemination and reproduction of key concepts in the domestic corridors of power. This aspect of the process of dissemination summons up a number of illuminating analogues to describe it. They range from the ecclesiastical—evangelization, spreading the word—to the medical (Levin 1998) and oncological—the multiplication of sites of infection, metastasis—though contemporary jargon alludes to it with the more neutral descriptor of 'networking'. (Amaral and Neave 2009: 85-6. My emphasis)

They argue, however, that while the OECD can be considered as the "Trojan Horse" of neoliberalism (2009: 92), this in itself is not enough to explain its authority. Rather, we need to appreciate how its success derives from the process of *normalizing the application of economic theory to policy problems*. It is not enough to say that the OECD is wedded to neoliberalism, but rather we need to see how it brings about those ends (privitization, marketization of institutions, the reduction of the state to the provision of services or regulations) "not only by showing that the doctrine works, but that it *is* an appropriate framework within which plausible solutions may be sought, identified, and acted upon." Amaral and Neave 2009: 94). The OECD is thus effective because it changes the parameters of debate around education, and enculturates key civil servants and policy makes into both these values and the international technocratic networks that support them.

In contrast, Schuller and Vincent-Lancrin describe the OECD in entirely neutral terms as a provider of information that by no means leads towards a single model (2009: 80). The language of descriptions such as the following suggests a role for the OECD as responding to changes happening in the world, rather than contributing to them: "The locus of educational policy making is shifting, under a number of pressures. One possible consequence is the diminution of the importance of the nation state as policy maker." (2009: 79) Similarly their presentation of their role as *responding to* change, rather than *bringing it about* through the dissemination of comparative information, extends to their argument about the importance of internationalism. They present themselves as *responding to* an otherwise neutral and inevitable global harmonization of education policy, rather than actively working to create this situation. In this respect of the OECD's language does not differ from that used by other international organizations; for instance the WTO, which also talks about its role in education policy as a matter of responding to an increasing internationalization and marketization of education, rather than being culpable for bringing it about (e.g. Verger 2010: 4).

Neoliberal theory has been presented as a rational, objective, depersonalized and thus more scientific form of social organization (Constable and Valenzuela 1991: 188). The OECD makes the same argument for the depoliticized nature of pure information. The role of the OECD is thus to respond to a natural, inevitable process of globalization and economic rationalization: a progress narrative. Amaral and Neave have demonstrated, however, how the OECD's role is one of *creating* the situations it claims to be passively responding to. Neoliberal ideology is transformed into educational policy in different countries around the world, through the very active and deliberate actions of this supranational organization. Through "missionizing" to local civil servants, and the provision of reports that are taken as authoritative and objective not only in governmental circles but also in the media, the application of neoliberal solutions and frames of reasoning to the question of education becomes a powerful norm.

So far I have outlined the logic of a theory that has already been used to shape education policy in many countries and looked at how it travels to different countries through the actions of supranational organizations like the OECD. I will now turn to the specific example of Chile, but with the caveat that the case of Chile is both illuminating and distracting. Chile is an example of an explicit experiment by a visible set of actors to "test out" Milton Friedman's theorization of neoliberalism on an entire national economy, and in so doing, to literally revolutionize society itself. As such, it is an example that is easy to follow. But this in itself makes it problematic, when we look back to places like the US or the UK where the process of transforming society was no less radical but less *explicitly* enacted. It is the combination of this social experiment with a dictatorship, rather than a democracy, that makes it so graphic and thus so easy to see as something entirely separate from what took place in other democratic countries. While I do not have space to compare the US case to Chile in explicit detail, similar results certainly *have* been achieved in the US (see contributions to Gusterson and Besteman [2010] and specifically Graeber [2010]). With this caveat in mind, I will turn now to the history of neoliberalism in Chile to describe first the explicit movement from economic theory to nation-wide policy and secondly the central significance of the Universities and education policy from 1973 to the present.

Neoliberalism in Chile: education and democracy

Universities have always been at the center of Chilean political upheavals. Associated first with progressive liberalism and democracy following independence from Spain, later with the socialist popularism of Salvador Allende; universities were among the first and last sites of active repression during the dictatorship, and most recently have been the locus of struggles to redefine public participation in the democratic era. In this section I will sketch a brief history of this relationship, demonstrating how the significance of the university comes from its role of social reproduction through education and its position as a central bastion of the professional middle-classes. The purpose of this history will first be to demonstrate how the neoliberal theories of Chicago-trained Chilean economists were developed into a political ideology that explicitly aimed to transform Chilean society; and secondly to illustrate the place of universities in this process.

Chile was unusual in South America in having a long standing tradition of democracy: since its independence in 1818 there had been only two very brief periods of military rule. In the 1950s and 60s, left-leaning but still relatively conservative governments came to power that sought to narrow the gap between the rich and the poor. In 1970, however, Salvador Allende, a Marxist leader of the socialist Unidad Popular party, was elected president under the promise of ushering in a new socialist revolution. During the 1960s it was accepted that that state management of the economy was part of the democratic process, even by private entrepreneurs (Valdés 1995: 8). But both Allende's socialist revolution and Pinochet's neoliberal experiment that followed were explicit attempts to bring about a complete transformation of Chilean society and its democratic tradition, using the economy as an engine of change.

> [The mediatory state of the 1960s] was the state that both experiments tried to destroy, for reasons that were not wholly different. Some (the Unidad Popular) sought to do so because 'the state role had always been to favor the basic interests of monopoly capitalism'. Others (the neo-Liberals) believed that the state was, by definition, inefficient, favored monopolistic groups and, worst of all, hindered market freedoms. It was thus a major obstacle to the formation of a 'free society'. … The democratic tradition that had distinguished Chile in Latin America was never deemed worthy of contributing to reform. Quite the contrary, it was rejected [by both] as a distorting element; an allegedly corrupt farce concealing either class domination or a political caste's use of the state for personal gain. (Valdés 1995: 9)

Allende aimed to use the economy to transform Chilean society according to socialist principles: to close the gap between the wealthiest and poorest members of society. The explicitness of his aim, and the reaction it provoked in middle and upper class citizens, is significant to understanding what happened next. Although the US did help bring about the coup of 1973 (as Henry Kissinger, Nixon's national security adviser, famously put it: "I don't see why we need to stand idly by and watch a country go communist due to the irresponsibility of its own people."), it is generally accepted that this would not have been possible without the support of a large portion of the population.[4] The middle and upper classes supported the coup and the vision of a neoliberal society the Chicago Boys promised, because it would end Allende's programs of nationalization and wealth redistribution.

Allende's goal was to restructure the economy around the needs and desires of the majority of the population rather than the relatively small middle-class. This directly affected people's day-to-day experiences, either for the better or the worse. While meat, for instance, had

4 Indeed, even long after the full extent of the human rights abuses associated with General Augusto Pinochet's dictatorship had been brought to light, a sizable 40 percent of the Chilean population still believed it was justified (Stern 2006: 7).

previously been consumed regularly by richer people, it now became affordable for all. But this led to shortages in supply, and middle-class housewives having to stand in line for hours to acquire what they considered to be a basic necessity (O'Brian and Roddick 1983: 28). Shortages were exacerbated by a US imposed selective embargo on trade with Chile, that included putting pressure on international banks to refuse the country credit and not allowing the export of vital machine parts used in the newly nationalized factories. In the face of shortages, working class communities that overwhelmingly supported Allende's government organized local distribution of food and other goods, punishing those businesses and individuals who attempted to resist state-imposed price controls or to hoard (as documented in the film "La Batalla de Chile: La Lucha de un Pueblo Sin Armas – Primera Parte: La Insurreción de la Burgesía" [The Battle of Chile: The Fight of a People Without Arms – Part One: The Insurrection of the Bourgeoisie] directed by Patricio Guzmán 1978). Examples of housewives not being able to buy steak might appear frivolous, but as the oral historian Steve Stern (2006: 7-34) has documented, it was the rapid deterioration of everyday life, coupled with a growing sense of fear, that led to increasingly vocal calls for the military to step in and "save" the country. On a more organized front, the sense of fear was provoked by deliberate misrepresentation in national newspapers (almost exclusively owned by right-wing members of the business community) of the new economic policies, and increasing reports of violence by left-wing terrorists that were almost certainly staged by the right.

Two attempts by the US ambassador to rouse the military into rebellion failed. In 1972 a strike of lorry owners that aimed to bring the country's transport system to a standstill alongside a coordinated lock-out by factory owners was defeated, when workers took over factories themselves and organized a massive impromptu transport system from any vehicle they could lay hands on. A change in military leadership, however, meant that those in both Chile and the US agitating for a coup finally found a group willing to carry it out, and on the 11th of September 1973 they were able to seize control.

Almost immediately, members of the military were installed at the head of all major institutions, including the universities. A group known as “The Monday Club”, made up of members of the Christian Democratic Party, the military (particularly the navy), business leaders and prominent economists at the Universidad Catolica, were ready to distribute a plan for the economy that they had finished writing and printing only days earlier. The Monday Club’s origins were in a program initiated in the 1950s between the University of Chicago and UCatolica, whereby Chilean students traveled to Chicago to study economics and then returned to highly prominent positions in academia, business, politics, and the military in Chile. The formalization of the resulting network of shared experiences:

> ...was a natural development, and merely demonstrates the wisdom of the original US aid project which set out to establish close ties between the departments of economics at the Catholic University in Chile and the University of Chicago. The Catholic University has a long tradition of educating Chile’s right, and the network of those influenced by Chicago ideas was now a very important one. Its links stretched into the main opposition parties, leading business and gremio circles, the Monday Club itself and now into the armed forces. It was a socially cohesive network, bound by strong ties of friendship, marriage, and, for most of its members, common life experiences at the department of economics and the University of Chicago. (O'Brian and Roddick 1983: 39).

Lacking training themselves for the management of civilian institutions, the military were happy to accept the economic plan of the Chicago Boys (as the members of the Monday Club became known). By 1974 the new regime embarked on what was in effect a massive and immediate experiment in the implementation of Hayak and Milton Friedman’s economic theories to an entire country, by force.

> Preparations for 'shock treatment' had begun long before the official text of the government Economic Recovery Programme was released on 24 April 1975. This treatment involved the deliberate creation of a massive deflation through a drastic cut in public expenditure which would cause mass unemployment, wage cuts, bankruptcies and widespread deprivation. ... [Shock] was meant not only to strengthen Chile's hand in world markets by stimulating new, non-traditional exports of the goods no longer being consumed within Chile. More important still, it was designed to force acceptance of the Chicago economic model. State expenditure was to be reshaped in a fashion which would ensure that Chile would no longer depend on what Hayek calls artificial stimulation by the government to keep its economy going. Structures and expectations were both to be revolutionized. (O'Brian and Roddick 1983: 54)

Shock therapy primarily involved "a drastic cut in public spending through the reduction of state employment and the elimination of agricultural subsidies. The result was a 12.9 percent drop in GDP" (Valdés 1995: 20. See also Klein 2007 for further discussion of 'shock therapies'). It had devastating social consequences as wages plummeted, unemployment in some urban shanty-town areas rose as high as 80%, and soup kitchens organized by the Catholic church fed entire neighborhoods. Meanwhile mass bankruptcies of private businesses and the immediate sale of nationalized assets at low prices meant that individuals able to take advantage of the sudden deregulation of foreign credit could make quick fortunes. Although Shock officially ended in June 1976, variations on the same model continued in Chile throughout the 1980s.

It might not be surprising that a dictatorship accepted this vast suffering, given the level of extreme political violence and state terrorism being meted out on the vast swathes of the population at the same time. However, the continued belief in neoliberalism as a viable economic theory, despite the visibly catastrophic effect it was having, can only properly be understood when we recognize it as an ideological crusade to transform the whole of society.

> The free-market model launched by the military regime in the mid-1970s was not only an economic program of market liberalization and integration with the new global economy. It was also an attempt to introduce a new set of values and to change the culture of Chilean society. It amounted to a cultural revolution. This new utopia was built around an idealisation of the free market, the promotion of an individualistic ethic, the legitimatization of the profit motive extended to a vast array of new activities (education, health, pensions, roads use). (Solimano 2012: 39)

The current example of widespread deprivation in Europe caused by austerity, particularly in Greece, demonstrates that even without a military dictatorship extreme social suffering can be justified in this way. The Chicago Boys, for their part, believed that democracy was "a luxury poor countries could not afford", and that in any case the most free form of government was that which was entirely impersonal: a dictatorship following scientifically objective economic science (Constable and Valenzuela 1991: 188).

By 1980 Pinochet was facing increasing calls to justify his hold on power, and so a new wave of 'modernizations' was introduced that covered labor, social security, education, health, agriculture, regional policies and the judiciary. "But the first and most crucial of them was a bid to institutionalize the labour market in a way favourable to the new model, while simultaneously 'liberalizing' the political climate and weaning Pinochet away from the worst excesses of repression and towards a Hayekian 'Constitution of Liberty' as a mode of political control." (O'Brian and Roddick 1983: 77). Following the ratification of this constitution, however, state-sponsored murders and terror rose again as the country entered another prolonged 'state of emergency'.

The 1980 constitution included a new model for Chilean universities. During the first seven years the regime's approach to universities had simply been one of violent repression through strategies of closing entire departments and regional campuses, the murder or exile of professors and student leaders, outlawing student and faculty organizations, and more insidious restructuring that led to a shift in what subjects could be taught. Overall the military junta had focused more on primary and secondary education than on universities (Levy 1986). But universities were seen as places of left-wing resistance, particularly because Allende's government had actively opened up public universities to the working and lower-middle classes. They were also strongly tied to professional organizations called *Colegios* (see Chapter 6), which were influential organizations of the middle-class. Universities were therefore subject to oppressive control as a means of crushing potential opposition. Between 1973 and 1980 the number of students had plummeted as a result of the regime's repressive measures and because of harder standardized tests for admission. Strict curricula that emphasized memorization and exams, and that were not concerned with students' interests, meant that "by the late seventies, many claimed to see the toll on student character, with harsh requirements breeding docility." (Levy 1986: 113). Among the faculty, those who remained (and remained alive) faced increasing pressure to focus on rigid training in the legally recognized professions, rather than critical reflection. This was a reversal of a movement in Chile since the 50s, to emphasis research over a narrow focused on producing professionals (Levy 1986: 114).

The 1980 constitution, however, set out to formally restructure the entire education system in line with the new economic order, and specifically to promote the privatization of schools and universities. The reforms had three goals: deregulation (only minimal requirements for opening new private universities); diversification (a tiered system of vocational and academic institutions); and the introduction of student fees that would also force institutions to seek alternative funding sources (Brunner 1993: 36). Previously there had been only two major universities in Santiago, the Universidad de Chile (UdeChile) and Universidad Catolica (UCatolica), and six private universities. Each of these had campuses across the country, and crucially all were free and up to 90% publicly financed (Brunner 1993: 35). During the first decade under the new reform, over *three hundred* new private institutions of higher education opened their doors. The Chicago Boys argued that rectors would only cut waste if their budgets were severely restricted, and students would finish faster and "be more responsible" if they were paying for their degrees. "The notion of responsibility thus provided a convenient link between cutting public expenditures and diminishing political activity", and was supported by an argument was that university education favored the middle class and therefore free universities were supporting social stratification (Levy 1986: 115).

The effect of the 1980 reform was that enrollment increased dramatically, particularly in the vocational institutions (Brenner 1993: 37-8). But coupled with an ever-increasing gap between the quality of education in public and private high schools, the result was rapidly growing inequality. Children of richer parents could afford to go to better quality private high schools, which would enable them to pass the exams to get into the cheaper and better quality established universities, UCatolica and UdeChile. Meanwhile poorer students suffered from the chronic under-funding of public schools, and were then only able to get into the much more expensive but lower quality new private universities.

In 1990, four days before handing over power to the new democratic government, Pinochet signed into law another new reform to the education system. Its main goal was to concertize the existing system, making it harder for subsequent democratic governments to change it. Although there it not space here to describe in details the problems built into the new electoral system, the effect was that the right and the military continued to have disproportionate control of parliament. This made it difficult for the coalition of left and center-left parties that was elected to power (known as the *Concertación*[5]) to enact any major changes in policy.

> This was especially so in education, given the high threshold required to change the outgoing military regime's Ley Orgánica Constitucional de Enseñanza (Constitutional Statutory Law of Education), which guaranteed the market oriented educational model (Bloque Social, 2006). Given the circumstances, the Concertación adopted a relatively narrow set of priorities that could be achieved by executive order rather than through the legislature (Scope, 1997); any attempt at reforming the education law would have to wait. (Burton 2012: 39)

Commissions set up by the Concertación to oversee education drew on the expertise of former activists and academics who had been part of the generation that resisted the dictatorship: notably including José Joaquín Brunner, a former director of the Facultad Latinoamericano de Ciencias Sociales and a prolific writer on Chilean education. At the international level, the World Bank in accordance with the OECD continued to stipulate that loans should come with conditions attached that ensured the continuation of the privatization process (Burton 2012: 41, Figueroa 2010: 251). But while they worked with these international organizations and representatives of the education sector, the commissions administrating education policy for the Concertación failed to include students' or teachers' organizations in the deliberative process.

This neglect resulted in massive resentment and ultimately the largest protests seen in the country since the uprisings in the late 80s that brought down the dictatorship. From April to June 2006 as many as a million secondary school students, joined by teachers, parents and university students, took to the streets to demand better quality education at lower costs (Gutiérrez and Caviedes 2006). Known affectionately as the "March of the Penguins" because of their distinctive

5 The Concertación, under Particio Aylwin (1990-1994), Eduardo Frie (1994-2000), Ricardo Largos (2000-2006) and finally Michelle Bachelet (2006-2010), won every post-democracy election in Chile up until 2010.

school uniforms, the students were largely successful and the government was forced to include them in the next round of education policy making. University students I met in Chile in 2010 were still proud of their involvement in this protest, but it ultimately did not achieve all that was hoped. In 2011, not long after I had left Chile, another wave of protests brought the country again to a halt, as university students and teachers across the country went on strike against the first right-wing non-Concertación president elected since 1990, the multi-billionaire Sebastián Piñera.

Since the return to democracy education reform has thus been the most visible and effective arena of resistance to the continuation of the neoliberal social experiment. The most potent example that the policies originally implemented by the dictatorship are being carried forward, are the fees that students must pay to attend university. Rather than heeding the persistent call for education to be free for all, fees have in fact risen. Anthropologists in Chile consider themselves to be of the left, and the UdeChile—as the oldest university, and as the only state university—retains its reputation for being politically radical in contrast with the right-wing UCatolica. Indeed, the announcement in 2010 that Catolica was opening an anthropology department was met with amazement and some derision by the archaeologists I discussed it with, so much is it presumed that anthropology will always be on the left. It is against this background—of resisting a neoliberal social order imposed at gun point—that we must understand debates in Chile about education, and particularly the framing of these debates by Chilean archaeologists in conversation with North American colleagues, as I will discuss in the following chapter.

Two specific points stand out from this history. The first is that in Chile education and universities have always been central to the struggle to define democratic participation (or the lack thereof). The second is the belief—held by both the left through Allende and his supporters, and the right through Pinochet, the Chicago Boys, and their supporters—that it is possible to transform society and the way people conceive of themselves and their social relationships, through structural transformations of the economy and institutions. The Chicago Boys believed that when social institutions such as schools, universities, hospitals, the market, and above all government were reconfigured, people's true, inherent selves (self-actualizing, rational entrepreneurs motivated by economic values) would be set free. If the structures of daily life were altered, people would begin to act according to a different set of values. To achieve this, very specific strategies were adopted: changing institutions, but also changing the parameters of debate such that new norms could be established. Historians of the dictatorship point to the role of the media in normalizing neoliberalism, particularly given that the only national newspapers unaffected by censorship were those associated with, or actually owned by, former Chicago Boys[6]: "Through *El Mecurio* and *Qué Pasa* (a conservative weekly magazine), a systematic indoctrination was carried out in the style needed to spread a dogma." (Valdés 1995: 32). As commonly happens in repressive regimes, this kind of dominance and control of public discourse through the media occurred alongside of, and can be seen as a continuation of, the extensive campaigns of State murder, torture, and terrorization:

> State terrorism … served to unify and make coherent both 'mild' and 'severe' authoritarianism. Scientific and technical authoritarianism was protected by physical force and wholesale intimidation of the population. The authoritarian system refused either to discuss the measures it implemented, or to amend them in line with their cost, while physically threatening all those who tried to organize protest or demand their rights. Seen in this light, authoritarianism was indeed an efficient mechanism for the drastic transformation of the Chilean economy.

6 E.g., *El Mecurio*, a right-wing national newspaper still in circulation, was owned by Fernando Leniz—a Chicago Boy and business man who the junta appointed as its first civilian in charge of the economy in 1974 (O'Brian and Roddick 1983: 50).

(Valdés 1995: 32)

Media propaganda that goes hand-in-hand with state authoritarianism during a dictatorship is not surprising. It is when stepping back to look at the democratic period that we can think about the role of the media in normalizing neoliberalism in terms that reflect back on other countries and other periods. Although my purpose here is not to hold Chile up as an example, only so US-based anthropologists can reflect back on themselves, an acknowledgment is required of both the *differences* in the historical structure of the debate around education in the two countries, and the fact that the models of neoliberal which have been imposed (and the means of/reasons for doing it) in each place share similarities that can be lost when focusing on foreignness. Looking at the Chilean case from the UK or US may allow a comparative framework in which the observer can easily dismiss what happened 'over there' as the 'typical' behavior of a volatile South American state, 'naturally' prone to dictatorships and violence. This stereotyping is doubly problematic. To start with, it has often been said that Chile's strong democratic tradition prior to 1973 led to a 'it will never happen here' attitude, that actually blinded members of the left to the imminent danger in the weeks leading up to the coup. As a result of their faith in their own democratic tradition, they did not seriously consider preparing themselves for armed defense and as a result were more rapidly defeated when the military struck. To dismiss Chile as inherently more likely to fall under a dictatorship is thus misguided as well as insulting. Additionally, this perspective makes it easier to ignore how parallel processes of neoliberalization in other countries (such as the US) have come about through recourse to the same arguments about efficiency, rationality, and accountability that the Chicago Boys and the military junta used, and propagated through parallel 'reform' policies and parallel domination of public discourse in mass media.

Conclusions

In this chapter I have shown how the assumptions of universalism that we saw in the practice of North American archaeologists in Bolivia, have parallels at the larger international scale. Archaeologists, like many other scientists, work primarily in universities, and to understand universities in North and South America today we must understand how they have been restructured over the last three decades as part of a larger neoliberal project. Universalism is central to neoliberal theory and policy making, because of the fundamental tenet that all human action can be explained by recourse to a single universal human value, the drive for individual economic accumulation. Moreover, through the example of Chile I have argued that state officials looking to engage in the reorganization of society according to neoliberal theory, see the control and restructuring of universities as essential to this process. International organizations promoting neoliberalism, such as the OECD, also focus on education. This is because universities (along with schools) are sites where individuals acquire the knowledge-objects necessary to become entrepreneurs: universities are where individuals are socialized and trained into being their true rational selves.

A concern in this chapter has been to ground discussion of 'neoliberalism' in concrete examples. To move from abstract theories of human motivation, to how these theories create arguments for particular conceptualizations of knowledge, teachers, and students; and how these arguments are followed-through to justify specific ways of organizing labor, pedagogy, and funding within universities. These new organizational forms were imposed in Chile at gunpoint during a violent dictatorship. The explicitness of the Chilean example makes it easier to see how this was imposed, why, and what resistance it met. The explicitness of a dictatorship arguable also made it easier to push back against, as I will explore in subsequent chapters: it is easier to identify one's enemy when he wears a uniform, harder to fight back against when the problem stems from a set of ideas that are presented in terms of 'freedom' and 'choice'. But meanwhile the example of the OECD demonstrates how 'soft pressure' can be applied to transform the parameters of debate, so that neoliberal values become normalized during democratic periods as well as during dictatorships. The jump from a theory of human motivation to arguments about the status of teachers and students, and from arguments about teachers and students to restructuring universities in-line with these arguments, has happened in the last three decades in democratic countries such as the US, the UK, and New Zealand. Of course, on the ground these processes, and the resistances to them, have played out in quite different ways in each context. But the OECD's involvement demonstrates how this has become an international phenomena. In the following two chapters I turn to look in more detail at how this plays out on the ground, through the example of Chilean archaeology.

Chapter Six

“Privileging the Critical Spirit”: Chilean cultures of professionalism and education

Introduction

What is the alternative to a neoliberal theory of knowledge as a kind of bounded thing, a ‘knowledge nugget’? Chilean archaeologists, I will argue in this chapter, see archaeological knowledge as a process and a practice of critical reflection; something explicitly generated for and out of larger political and social contexts, rather than a unit of information that has an inherent economic value; and something that requires expertise—specifically a scientific community—to come into being. This conceptualization of knowledge does not exist as a pure opposition stance to the neoliberal ‘knowledge nugget’ concept, but instead in relation to it. It is articulated and practiced through the ‘friction,’ to borrow Anna Tsing’s term, that is generated when individuals work within institutions that appear to be shifting under their feet.

Chilean university reform since 1980 has presupposed that universities are primarily educational institutions and therefore those working within them should see themselves as teachers serving students. Archaeologists working and learning within these institutions interpret their roles differently, but this act of interpretation is relational—it is understood to be a reaction to a neoliberal (and to a certain extent a US) model being imposed upon them. At the same time, the development of commercial archaeology, known as ‘Impacto,’ envisages an even more ‘nugget-like’ approach to archaeological knowledge—and here archaeologists are also involved in a process of articulating an alternative that is neither entirely a rejection nor a response but instead an on-going and relational re-interpretation. In examining Chilean responses to institutional change I wish to avoid both an overly romantic celebration of autonomy/resistance and a narrative of unproblematic domination on the part of the either the State or the Global North. Instead I seek to understand how the friction generated by the circulation of neoliberal education reform can lead to “new arrangements of culture and power” (Tsing 2005: 5) that are not necessarily synonymous with either resistance or advancement; but neither do they represent the image of an easy, global movement or transmission of ideas.

Creating future colleagues

In 2010 I interviewed Jorge, a senior academic at the Universdad de Chile (UdeChile) who began his career in the late 80s as a student in the same department. While we were reflecting on the changes he had seen in the profession over the last two decades and a new shake-up of the archaeology department’s curriculum that was currently being debated, I mentioned an article I’d read in a nationally circulating newspaper. The article was one of many similar opinion pieces I had seen around that time: the problem with Chile’s education system, the author explained, was that it is not focused on the production of flexible workers. Jorge’s response was one of unequivocal rejection. Although the system is changing so that education serves the market, he was not interested in perpetuating this in archaeology.

Jorge: There is no interest in becoming people who are more adapted to the world

> of the market. No interest in being people who are efficient, but in being people who are scientific. And interested perhaps in the market, but not for the sake of the companies. I believe that education in Chile can be improved. But improved to generate people who are more prepared and more educated. Not more sufficient for work.
>
> Mary: more prepared for a *careera*[1] or in general?
>
> Jorge: For the person. And as well for your, for your professional development. Yes, we could make the *licenciatura*[2] easier and only leave them with technical field skills. To prepare them only to excavate. This doesn't interest me. I am interested in thinking. For this... you need to have a critical capacity. To discuss the system, and to discuss the interpretations, to discuss the hypothesis. To discuss, with the aim of having a critical perspective on society. Because, after all, we also are social scientists. We are not technicians. We can see this very clearly at least at the Universidad de Chile. This is what education is for. Probably, in Chile's economic situation, there is the idea that we need people who are more sufficient for work as well. But we also need people who are more critical, of this system. The social and political system, and the economic system. And this is also the characteristic of this university, and this *careera*-- particularly anthropology, where we privilege the critical spirit.

Chilean students begin their professional career as novice archaeologists and are conceptualized as junior colleagues when they enter university at the age of 18. In comparison, the Liberal Arts university is founded on the idea that 17 or 18 year old students gain a general education through a bachelors degree and begin their archaeological professionalization during a masters or doctorate. In practice this influences the way that the students in Chile are conceptualized, in relation to the disciplinary community. Namely, the archaeology department is not orientated around students but instead around the discipline, and debates about student education or professors roles are first and foremost debates about how the discipline will move forward in the future, not about individual students' needs. Educating students is necessary to

1 A term which can mean an individual's career, but also refer to a discipline within the university

2 The first degree that Chilean students take. Not necessarily the same as the degrees of the same name that students in other South American countries like Bolivia, Peru and Chile take. See next section of this chapter for a longer discussion.

ensure the future of archaeology, but the ultimate goal is the discipline not the student—and certainly not the student's needs as an individual entrepreneur who needs to acquire skills and quantifiable knowledge that will be of use in the marketplace.

As Jorge and his colleagues understood too well, this conceptualization of a university department is at odds with that argued by the author of the newspaper article referred to above. Both the faculty and students in the UdeChile anthropology department actively reject the idea of evaluating knowledge in terms of its economic value or any concept of the student as 'consumer'—i.e., a situation whereby the student and his or her needs is central to the purpose of the department. As Jorge's comments illustrate, for Chilean archaeologists the student-professor relationship is structured around an understanding that the primary role of academics is the production of scientific knowledge that benefits society as a collective, not the teaching of individual students. Yet socializing and educating the next generation of students is essential to scientific knowledge production because, as future colleagues, the quality and direction of future research depends on how current students' knowledge and skills are shaped.

Although *aspects* of the ethos behind this perspective are similar to that shared by some North American archaeologists I interviewed, this Chilean orientation results in quite a different classroom culture than that of the US. When I arrived at the UdeChile in 2009 I was struck by the difference in classroom culture and the way students and professors interacted with each other, when compared to the US archaeologists I had been working with.[3] Separating out what is

3 I was also reminded of how my own experience of graduate education in the US had been markedly different from my experiences of university culture in the UK. Through my participant observation in Chilean classrooms and excavations, and through interviews with students and professors, I made use of what I found to be points of difference with the US to explore what characterized the Chilean case. I pushed up against instances or lacks that felt strange to me, or reminded me of the UK, in order to explore alternative interpretations and meanings held by the Chilean students and professors. For instance, when I realized I was the only person made uncomfortable by particular student behaviors in classrooms, this led me to question what work discourses of 'student civility'

different in the Chilean case from the US, therefore, requires paying careful attention to the different meanings that underpin apparently familiar spaces. I will attempt to make these differences legible through the following examples.

Looking specifically at archaeologists as professors, those working at the UdeChile saw themselves and were seen by their students primarily as archaeologists and only tangentially in terms of the classes they taught. This lead to a different culture of the classroom, for example one in which the professors' relationship to students was not one in which it would be considered appropriate for them to assert control over the bodies and actions of students in the classroom. Classroom behavior that in the US would be considered very rude because it is taken as symptomatic of disrespect and a tendency to view the professor-student relationship as nothing more than a commercial service-based transaction, is *not* interpreted in this way in Chile. It is not just that this kind of behavior does not indicate "incivility", but that *the conversation about incivility itself is absent*. An article in the US based *Chronicle of Higher Education* argued that "student incivility" is particularly a problem for professors who are young and/or female, and types of student incivility were listed as: "passive behavior, such as sleeping or texting in class; more actively disruptive behavior, such as coming to class late or talking on cellphones in the classroom; and behaviors that appeared directed at the instructor, such as open expressions of anger, impatience, or derision." (Schmidt 2010). The concern for 'students as consumers' exists in Chile, but is not connected to a discourse on 'civility' *or* to the kinds of behavior listed above.

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do in US classrooms (i.e., how social norms of politeness structure the understanding of student-professor hierarchy and authority), that they evidently were *not* doing here in Chile. I was reminded that these same discourses and the associated formalization of hierarchies between students and professors had been strange to me when I first arrived in the US. My articulation of the way students and professors in Chile reject a particular concept of the university, one that is orientated towards a specific configuration of the student-teacher relationship, has therefore arisen out of a triangulated perspective between my personal and ethnographic experiences in the UK, the US, and Chile.

Many of the behaviors listed are in fact quite common but not seen as problematic, reflecting a different conceptualization of the relationship between professors and students: namely, one that more closely approximates junior and senior colleagues, rather than the disciplinarian nature of a teacher-pupil relationship.

Although this conceptualization of the professor-student relationship might appear at first glance to be similar to that found elsewhere, particularly in the US, I argue that it is a subtly different understanding of the nature of the professional/educational relationship reflecting a different understanding of what the role of a university is. This was a distinction that the Chilean archaeologists recognized. Jorge had been involved in a North American Field School project and had seen US professors working with their own students. Like many of the Chilean archaeologists and students, he thought that US students had an immaturity and consumer-like approach to their education that he associated with the neoliberal market-orientated model being pushed in Chile. The difference between US and Chilean students, he argued, is that at the UdeChile students are “already mature” when they enter university at 18 because they expect to have a career in archaeology: “Not like in the US or Europe where they come in like children, who don’t even know where they are going!”

Chilean students are thus seen as proto-professionals and already a component of the discipline, rather than necessarily as ‘students’. Professors, too, are seen first in terms of their position within the disciplinary community, only secondly in terms of any role they might have within the university as an educational institution. Another UdeChile professor, Sofia, complained that the university kept encouraging them to see themselves as teachers first and academics second: “We are archaeologists, but increasingly we are becoming less archaeologists and more teachers”. The pressure on archaeologists of all ages who worked within the university to see themselves first through the student-teacher relationship, was interpreted as an attempt to re-frame the university as commercial and educational enterprise: a place where professors would be obliged to cater to the needs of consumer-students and look after them like quasi-parents.

The means by which a student becomes a colleague was not assumed, however. A good student, likely to be successful, was described as someone who took initiative, showed single-minded dedication and determination, and was intellectually curious but also hard-working and reliable. Most importantly, they had to become part of an existing research project. Archaeological projects are run as teams, with an established group of researchers who tend to have different specialties (e.g., in the analysis of textiles, ceramics, rock art, animal bones, and so on) working together on a succession of inter-related projects in the same geographical region over many years. A professor might choose one or two students to take on as part of their research team, but they had no obligation to take *anyone*, let alone everyone in their class. A student who could not find someone willing to take them on would have difficulty forging a career in archaeology. But the onus was on the student to make the effort to be noticed, not on the professors or the department as a whole to make sure that all students each year had found a mentor.

Shy students I spoke with were particularly concerned about their ability to draw attention to themselves in class, or to knock on the professor's office door and thus show sufficient initiative and interest. They certainly understood that their reluctance to put themselves forward would be detrimental to their careers, but this did not lead them to suggest that the professors had a *responsibility* to help them. Once taken on as part of a project, older team members would establish a much closer mentor relationship with the student, helping them find a project for their thesis, training them in technical and analytical skills, bringing them in as co-authors on publications and eventually helping them get their own projects started. Students who are able to establish a relationship with a project beyond the classroom could expect to work with that team for many years. Indeed, most younger professors at the UdeChile still worked on their former professors' projects in addition to their own. Jorge still had strong connections with the professor who had helped him get started twenty years previously.

To briefly illustrate these arguments, I will discuss three quite different classrooms from the UdeChile archaeology department. The student-archaeologists were in their third or fourth year, and around 20-22 years of age. While observing lectures and laboratory practicals it was clear that there was a great deal of personal variety in teaching methods. Some professors were thought to be more fun, more pedantic, more engaging or more challenging than others. In general, however, there was an expectation that the professor's authority does not rest on controlling the students' bodies within the classroom. Rather the relationship was seen as one that is certainly hierarchical, but motivated towards the same goal: both students and professors were concerned about the discipline of archaeology, and understood that the ultimate aim of the university classes was to ensure the future of archaeology would be in good hands.

Jorge's prehistory class

Jorge's classes were notorious among the archaeology students at the UdeChile for their icy atmosphere. But he was also known for grading very low and this led some students to make careful calculations about when to take required classes. If they took a required class the year he was teaching it, they felt they were almost guaranteed a low grade. But to leave it till the next year in the hope of a less demanding professor potentially meant putting themselves out of sync and behind a year. When mulling these options, however, it never occurred to the student to consider contesting a grade or to ask the professor to reconsider. When I described how US students would go directly to a professor to ask for a grade to be raised, or even in some cases aggressively fight with the university administration over grades, Chilean students were astonished. Sometimes students felt grading was unfair: in one instance a student taking an oral exam for an anthropology class was asked 13 questions and told that although she had passed she would have to come back a week later to answer the one question she had failed. She later discovered, to great indignation, that the other students had only been asked a single question and had all passed easily. But unfair grading was something to complain about in private, and not a matter students had control over. Jorge also had loyal defenders, however, and those students that stuck with him past the compulsory classes and into the closer working relationship that developed in the fourth and fifth years, or who worked with him on his field projects, insisted that his high standards and constant pushing made them into far better scholars. They argued that his grading was tough because he expected more from them: he expected them to break out of lazy and old-fashioned thinking, to be more critical of their assumptions, and to think more laterally of the implications of their data. Even the students who complained about his austere teaching agreed that the high standards he set for students were the result of the high standard he set for his *own* research, and that ultimately he was forcing them to be more rigorous and critical.

Jorge's teaching style illustrates both that the relationship with students can be *highly* hierarchical, and how his authority rested on the perception students had of his *academic* credentials, rather than his ability to control their access to rewards such as grades. Grades, as I argued in the previous chapter, are taken within a neoliberal logic to be a qualitative and objective measure of both the teacher's and the student's performance and a means of ensuring motivation.

In practice, many professors in the US believe this has led to a system in which instructors are under pressure to capitulate to students' desires for high grades in return for less work, in order to guarantee good scores on student evaluations. 'Grade inflation' is consider to be symptomatic of the student-consumer problem (Rojstaczer and Healy 2012). Whether or not students in US universities are indeed motivated by grades and a desire to do little work is a complex question, dependent on many variables that include the type of institution, the balance of compulsory and chosen courses, and whether they are undergraduate or graduate students (see also Armstrong and Hamilton 2013). But grading itself remains tightly coupled to the discourse of teaching and learning, to be commented on when an "obsession" with grades is either evident or absent. In the US the idea that students are motivated by grades, and that a teacher's authority is connected to their ability to give or withhold good grades, permeates the debate surrounding classroom relationships *whether or not it is seen as present or absent to the particular interaction, or as a good or bad thing*. In contrast, in Chile the question of grades and their relationship to a professor's relationship with students was barely present. Perhaps this concern will arrive in the future—I heard one off-hand and slightly bemused comment from a professor about the existence of student evaluations. But Sofia's lectures, for instance, serve as an example of how classroom culture is based on an understanding that students are motivated by their interest in the topic at hand, and interactions between the professor and the students within the classroom are framed around an assumption of a shared archaeological purpose.

Sofia's lab class

Sofia's teaching style was far less formal than Jorge's and her classes were both lively and highly focused. A brief snapshot of a weekly hour-and-a-half long lecture-based class that accompanied a very hands-on lab practicum, gives a flavor of the classroom atmosphere. During these lectures the students sat facing her in the small classroom and took extensive and detailed notes. Sofia talked quite fast, leaning back in a chair placed at the front of the room, and she made extensive use of a powerpoint presentation filled with a proliferation of diagrams, tables and images. The material covered related directly to discussions they had been having in the lab practicum and questions the students raised about the material they had been working with. As she lectured there were one or two quick clarifying questions, but the session was mostly about conveying information that would then be put into practice and discussed in more detail in the lab sessions. Eventually, however, the number of questions increased. The atmosphere was one of buoyant but intense concentration, as everyone in the room focused in on a shared set of concerns.

More than once people got up to walk out for various reasons, without the other students or the professor taking any notice. This was something I found notable in many of the classrooms I observed at the UdeChile because my own enculturation into US university classrooms led to the perception that such behavior is disruptive and disrespectful. I was reminded, when seeing this happening in Chile, of the fact that when I first came to the US from the UK I had found the classroom culture highly restrictive and had had some difficulty adjusting to the more rigid emphasis on maintaining formal hierarchies between professors and students than I had experienced in my undergraduate institute, despite them both being comparably research-orientated universities. Reflecting on my experiences in the US and UK, and commenting on the fact I seemed to be the only person in her class that noticed or was distracted by students getting up and leaving the classroom, I asked Sofia about whether she ever considers this kind of behavior to be ‘disrespectful’ or ‘uncivil’. But Sofia seemed surprised and a little offended by question, particularly the suggestion that job involved policing the bodies of adults in their early 20s or telling them how to manage the intersection of their work and personal lives. If someone is hungry and they aren't disturbing anyone else, why shouldn't they eat? If they have a phone call that's important and it happens to come during class, why shouldn't they go outside to take it? Likewise if the situation demands it, a professor might occasionally be called out of the classroom to answer a call or because they have a visitor. Disruptions are only problematic and annoying when they hinder the students' and the professor's shared aim of covering a certain amount of material that day. But there is an assumption that these young adults are able to judge for themselves the priorities of taking what might be an important phone call versus missing a few minutes of the lecture.

I take this attitude to be indicative of the conceptualization of both students and professors as motivated by a shared interest in the material being covered in the classroom and the *absence* of an assumption that a) students need to be encouraged or coerced into learning, and b) that they need socialization into normal adult behavior. This echoes Jorge's assertion earlier, that couples the idea of students-as-consumers to students-as-still-children. Archaeology students in their late teens and early twenties are seen as adults embarking on a career, not as children or consumers.

Neither faculty nor students see their relationship as one of coercion or service of students. Faculty also resist broader definitions of themselves as primarily teachers. Sofia is clearly an engaged and dedicated teacher. But when talking with her about teaching styles in an interview, she complained that there is too much teaching required of the faculty—she'd much rather be doing research or in the field. She laughed and brushed off my questions about a recent award for teaching that the university had given her, claiming that she was not even sure where, under the piles of papers and boxes of artifacts that fill her small office, she had left the framed certificate. The award can be seen as an attempt by the university to make the faculty evaluate themselves as teachers, but it made so little difference to her perception of her role in the university and her routine work, that she barely even remembered it existed, much less took it seriously.

Matías' theory class

Motivated by a desire to become good archaeologists, what then are the stakes that motivate both novice and established archaeologists? Matías' history and theory of archaeology class was an example of an explicit discussion of this question. The class was a standard requirement for all students, but for the last few years had been taught by this young and charismatic teacher. It was also the closest class I had observed to a seminar, in that it involved a mixture of discussion and lecturing. The syllabus description stated at the outset that it was structured around the three "most fundamental questions to social sciences": "¿Cómo conocer? ¿Qué conocer? Y ¿Para qué conocer?" [How do we know? Who is it that knows? For what purpose do we know?]. The debate in each class was rigorous but clearly challenging. When students were reluctant to respond to his questions, Matías would call them by name, lighten the mood with a brief joke, respond to their suggestions with encouragement but always be quick to push them into a deeper critique of their answers.

Sometimes, when learning I was observing this class, students who hadn't yet taken it would ask me if it was as good as everyone said, confessing that they hoped Matías would still be teaching it when they got to that stage in their course. Those who were already in the later part of their studies talked about it with a mixture of excitement and rueful frustration, and described how it had upended their whole idea of archaeology. Matías himself was concerned about improving his teaching, and we discussed it frequently in our conversations and interviews. He read widely on pedagogical theory, sought advice from friends in more traditional teaching careers, and wished that the university provided more training on how to, for instance, stimulate discussion in seminar-based classes for the younger students. Unlike Sofia, he *did* think about his role as a teacher. But he was also clear that the students' desires would be central to the educational process. He was critical of the students who did not stretch themselves, framing their inability to do so as a problem *for the discipline*. His overarching concern was with creating a more critical kind of archaeology in Chile, and he saw himself as advancing this goal through his research, his publications, and through pushing students (who would later become practicing archaeologists) to be more reflexive. Uncritical students would lead to uncritical, damaging archaeology. In this sense, like Jorge and Sofia, his orientation was towards the production of a particular kind of archaeological knowledge, not towards the students as individuals.

His classes appeared more light-hearted than Jorge's in many respects, but were considered to be just as intense in that he also constantly pushed students beyond their comfort zones and had high expectations of them. The atmosphere created was one in which the stakes appeared to be high: good archaeological work was expected of them—was *needed* from them—but it was not yet clear whether they would be able to live up to these expectations.

During the year these classes took place, 2009, the faculty of the anthropology and archaeology department at the UdeChile were in the process of restructuring the *Malla*—the curriculum students took in order to qualify as archaeologists. This was in part a response to deficiencies they themselves, and the students, had identified within the existing system as a result of the rapid growth in 'commercial archaeology'. But it was also a response to ongoing pressure on the UdeChile as a whole to fit into a neoliberal model. The new Malla illustrates how the conceptualization of the university and of students as future-colleagues is bound up in the idea of what Chilean archaeology is as a professional, a science, and a form of labor. This idea, to which I now turn, further illuminates how Chilean archaeologists conceive of knowledge as being "a critical perspective on society": a conception that is expressed in opposition to the neoliberal model of knowledge.

The changing institutional structure of Chilean archaeology

The prioritizing of students and professors within the university reveals something of the nature and significance of knowledge. Rather than a model that sees *students* as needing knowledge from university professors for the *student's individual advancement*; there is a model of *knowledge for society* created by professors who need students to ensure that *future knowledge* will exist. The community of professional archaeologists is front and center, therefore, as knowledge producers. Yet there are further complications to this story, because the concept that an academic community produces knowledge that is of benefit to society is challenged in more places in Chile than just the university. The idea of a professional class as a source of socially beneficial expertise has a strong historical basis in Chile. This became clear in 2009-10 when both the new *Malla* was being developed and a new professional organization was being founded: the *Colegio de Arqueólogos de Chile*. Following why these two institutions arose at the same time, and what core values were brought into debate by their creation, allows us to consider what is at stake for Chilean archaeologists as they articulate and defend archaeology as a project of socially relevant knowledge production, in response to three decades of aggressive neoliberalism. This relates to the earlier epistemological argument articulated through the Bolivian case study: debates about labor and methodology—*who* can know objects and *how*—are fundamentally questions about the nature of archaeology's epistemic object. In the case of Andean archaeology the question was whether a maestro, student-archaeologist, or director can see/know an archaeological object without embodied experience of the material. Here the same questions become significant in relation to the State's intervention in commercial archaeology and university education, structured around an even more reductive concept of what knowledge is and what is needed to become an expert.

The Malla

In Chile students study for two degrees: the *licenciatura* (license) and the *titulo professional* (professional title - referred to here as "titulo"), rather than a BA, MA and PhD. The curriculum that they study while attaining these degrees is known as the *Malla*. The licenciatura and titulo professional are rarely properly understood by academics outside of South America, and in recent years there has been a trend for students and professors to travel abroad to get additional qualifications in the form of a foreign PhD or MA that will be recognized by foreigners.

Just as the BA+MA+PhD model in the US is different from that in the UK, and that in the UK from the model in other European countries; so too the licenciatura in Chile is not directly comparable to the degree given same name in, for instance, Argentina, Bolivia, Mexico or Peru. In Chile the licenciatura takes approximately 4-5 years, and consists of taught classes graded through exams. The titulo consists of two independent research projects known as the *practica* and *tesis* (practical and thesis; the latter is sometimes also called a "*memoria*"), and can take anywhere from two to (in some cases) eight years to complete. Significantly, after being awarded the licenciatura the recipient is still known as a "student". Only after completing their titulo can they refer to themselves as an "archaeologist", and be employed as such. This is a legal as well as a social distinction, as I discuss below.

For the practica the student masters a practical skill (such as lithic, ceramic, or textile analysis) and completes the analysis of a dataset from an ongoing research project. This analysis results in a written technical report which may then be presented at a conference or incorporated into the publications of the larger research project it was derived from. In other words, the practica is a student's first disseminated contribution to the discipline. The tesis that follows may or may not be connected to the research undertaken for the practica, but is expected to be larger in scope and to make a significant *intellectual* contribution. Having demonstrated that they are able to master the analysis of archaeological material, the tesis shows that the student is also able to think critically, write professionally, and make significant theoretical contributions. It was considered by 2009, however, that the tesis had got out of hand, as increasing numbers of students wrote longer and longer dissertations of several hundred pages, that took nearly a decade to complete. The new Malla therefore aimed to strictly limit the page length, while also streamlining the courses taken during the licenciatura. In addition, there was a plan to introduce in the near future a new "Masters" program in archaeology, that would be taken *after* the licenciatura and titulo.[4]

The new, shorter Malla recognized in part that increasing numbers of students were under pressure to get PhDs abroad that replicated work they had already done for their titulo. The pressure to go abroad for a PhD could not be helped, because it was seen as an unchangeable

4 This Masters program will be launched in January 2014

condition imposed from "elsewhere"—something I will discuss in more detail in the next chapter. The only viable response was to move the Chilean half of the equation by easing the requirements of the titulo; but this was still a less pressing concern than the question of 'Impacto'. Throughout the debate about the new Malla, then, the concern was to focus on what was *most essential* for students to know and be able to do, by the time they received the official status of 'archaeologist', given what archaeological knowledge is and how it can be accessed.

Impacto and the Malla

The *Ley 19.300 Sobre Bases Generales del Medio Ambiente* (General Law no. 19.300 on the Environment), nearly always known as the "Ley Impacto", introduced regulations governing the protection of archaeological and environmental resources in the landscape. It is similar to others introduced through the 80s and 90s in Europe and North America: like *Cultural Resource Management* (CRM) in the US (Green and Doershuk 1998) or *PPG-16* in the UK (Darvill and Russell 2002, Bradley 2006, Everill 2009), Impacto works along a 'polluter pays' model, meaning that companies or individuals who construct a new building or drastically modify a landscape must first pay for an assessment of damage likely to be caused to the archaeological or environmental resources, and then must pay for the conservation of those resources. In the case of archaeology, the term 'resources' envisages *artifacts* to be the focus of archaeological knowledge production—not the kind of material relationships or even the spatial objects that archaeologists consider to be their epistemic object. 'Conservation' usually means rapid excavation focused on the recovery of these artifacts. Impacto therefore promotes a very different idea of what 'archaeology' is, as a form of knowledge and as a practice.

As in other countries, the model is also based on competitive tender—different Impacto companies can bid to undertake the work. And again, like in Europe and North America, the pressure of competitive bidding and the fact that excavations are undertaken as a response to impeding destruction rather than because of specific archaeological research goals, results in work that archaeologists both inside and outside of Impacto consider to be very far from ideal, if not outright scandalous. These laws conceptualizing the material traces of the past as resources that can be unproblematically identified, and that exist as conceptually and physically finite objects: echoing the emphasis on quantifiable, exchangeable, and comparable knowledge 'nuggets'. It also implies that generating archaeological knowledge is a simple matter of labor, rather than a form of expertise: easily identifiable artifacts just need to be dug out of the ground.

One feature makes Chilean Impacto distinct from either CRM or PPG-16, however. Unlike in Europe of North America, Impacto work in Chile is comparatively very well paid. The rapid growth of the Impacto industry in the last fifteen years has resulted in many more young archaeologists getting jobs. At the same time, academic archaeologists are increasingly concerned that good students are being tempted away from conducting high quality academic research by easier and more lucrative careers doing low quality salvage archaeology in Impacto. The faculty at the UdeChile, many of whom themselves undertake freelance analysis work for Impacto projects, responded by emphasizing a greater separation between the licenciatura and the titulo. The reasoning behind the new Malla was that, as increasingly numbers of students chose to not take the rigorous and time consuming titulo but instead head straight for impacto, the most essential skills and knowledge that even an impacto worker must have should be packed into the licenciatura. Ideally all practicing archaeologists should have the critical and analytical skills that only come from undertaking guided but independent research during the titulo. But after much debate the new Malla was conceptualized as a compromise position that would best respond to inevitability of a a two-tier profession. The result is a licenciatura that emphasizes the acquisition of foundational skills and knowledge through lecture and laboratory-based classes and field practice, but left more complex analytical and written skills to the seminars and independent research of the titulo.

The intensity of the discussion over the new Malla illustrates how a concern for students was conceptualized primarily as a component of conducting good archaeological research. Defining what students learn and the essential skills they had to acquire was an explicit exercise in defining what the future of the discipline and the Chilean archaeological community would be. In this sense, the production of archaeological knowledge through research is indeed the most important role of the university and the education of the next generation of archaeologists is understood to be significant through its role in guaranteeing the high quality of future research.

The Colegio de Arqueólogos de Chile

But a point that remained emphatically unchanged in the new Malla was that an individual could not refer to themselves as an 'archaeologist', or be counted as a professional, unless they held a titulo. The skills it recognized were essential, even as it was lamented that many non-titled archaeologists working in impacto would not have them. While holders of only the licenciatura could work for other qualified archaeologists in Impacto or academic excavations, they were not referred to as archaeologists and were not legally allowed to direct excavation projects. Nor could they become members of the *Sociedad de Arqueologos Chileano*s (Society of Chilean Archaeologists, referred to hereafter as the 'Sociedad'). This situation was in the process of being challenged in 2010, however, by the formation of a new and rival organization, the *Colegio de Arqueólogos de Chile* (College of Chilean Archaeologists, referred to hereafter as the 'Colegio'). With the support of some members of the UdeChile faculty and opposed by others, the Colegio was spearheaded by a group of young archaeologists who did not have their titulo but had been working for many years in Impacto. Their manifesto included a call for more direct political involvement of archaeologists and anthropologists in wider debates within Chile, such as publishing statements in national newspapers condemning police oppression of the Mapuche in the south. But a central argument for the Colegio's existence was to recognize the professionalism of archaeologists working in Impacto without a titulo.

The term Colegio is usually translated into English as 'Professional Organization' or 'Union'. But Chilean Colegios have a complex history that straddles these two English terms while not quite being captured by either. The Sociedad is a straight-forward academic professional society comparable to the *Society of American Archaeologists*. It organizes the biannual *Congreso de Arqueologos Chileanos*, publishes journals, and members must be holders of a titulo. During the argument over the formation of the Colegio, Jorge, as a former director of the Sociedad, partially dismissed the Colegio as nothing more than a labor union and argued against the necessity of a new organization that would speak for archaeologists as a collective because archaeology is a *scientific*, not a *political*, discipline. The Sociedad should not comment on political matters, he argued, because as a scientific organization it should be concerned only with the promotion and circulation of scientific knowledge. The founders of the Colegio would have agreed, because they saw this as exactly the problem of the Sociedad: archaeologists *should* recognize that their scientific research had political and social implications, and if the Sociedad was not prepared to represent the discipline in this way a new organization was needed.

The tangled arguments over the relationship between the Colegio, the Sociedad, and Impacto are therefore revealing of the struggles to define the status of archaeology and its role in Chilean society. This struggle initiated a debate about whether the practice of science should be above politics or have a responsibility to be involved in social debates; and whether it was desirable or possible to talk explicitly in terms of archaeological work as *labor* and thus discuss problems such as short-term contracts and insurance for archaeologists as workers. For archaeologists like Jorge, however, being scientific did not preclude being involved in critical engagement with society: he argued, after all, that this was the purpose of archaeology at the UdeChile. His objections to the Colegio were that it came out of Impacto archaeology, which was primarily conducted non-scientifically by archaeologists who did not have a titulo. This distinction, alongside the central involvement of students in the Colegio argument and the degree to which it provoked a restructuring of the university curricular, illustrates the longstanding relationship between professions and the university. At stake is the ability to continue thinking of archaeology as a *scientific profession*, and as *the university* as the natural home of professions rather than just places of teaching. To understand this point, we need to think about a) the historic centrality of the professional classes in Chile, and b) the extent to which professions are associated with universities. I will briefly outline this connection, therefore, in order to explain the specific form of politically and socially authoritative expertise that the Colegio's founders were calling upon, and the high stakes of using this term.

If archaeology had been an older discipline, it might well have had a Colegio before: as it happens the Sociedad was only formed in 1963, reflecting the relatively new status of archaeology in Chile (Rodríguez 1996). Prior to 1973 each established profession had its own Colegio, and these organizations had considerable authority within society as well as strong connections to the UdeChile and Catolica. At this point it is also important to make clear that the term ‘professional’ in Chile has a specific legal and historically situated meaning that is in *direct opposition* to the neoliberal use of the term professionalism to refer to having flexible skills that are suited to the marketplace. When the UdeChile was founded in 1842 it was granted the right by the state to award standardized recognitions of professional credentials for a limited number of professions, such as medicine and law. These credentials are known as a ‘Titulo Professional’. Colegios were formulated in parallel to the universities as state recognized professional organizations whose statutes were legally binding documents. When the Catolica was founded in 1888 it was also granted the right to award degrees although they had to be overseen by the UdeChile. The definition and organization of a profession through the universities and through the Colegios, and the recognition of an individual as a professional through the Titular Professional, are therefore *legal* as well as social definitions. The creation of the Colegios and the universities in the 19th century was thus specifically part of the new Chilean republic’s project of crafting, through the use of the law, a modern, liberal democracy.

On September 11 1973, as it became clear that the military junta had won, President Salvador Allende delivered his last address to the country from inside the besieged and mortar-bombed presidential palace and then turned his gun on himself. In the days that followed one of the first targets for bloody and violent repression were the university campuses. Direct control of the universities marked both the beginning and the end of the dictatorship: just as the universities has been among the first of the junta’s targets, so it became one of the last, as in his final days in power General Pinochet signed the 1990 *Ley Orgánica Constitucional de Enseñanza* that concretized his regime’s education policies.

But the universities had strong links to the professions through the Colegios, so attacking the university was also a means of attacking the professional class. While working class political and social organization was structured through unions, the middle-classes were organized through the influential and powerful Colegios. Perhaps inevitably then, the Colegios were banned by the military regime and replaced with *Asociaciones Gremiales*: general trade unions in the traditional sense, not structured around particular occupations (Levy 1986: 109). While a union might appear to European or North American readers to be more progressive than a professional organization, this does not hold in the circumstances of Chile in the 1980s and 90s. As Vicente Espinoza describes through the example of the medical and teaching professions, the dismantling of the Colegios was part of a concerted and deliberate effort to undermine the power of the middle class:

> The 1970s and 1980s brought detrimental changes to both the teaching and the medical professions, both in their working conditions as well as their ability to influence social policy. On the one hand, the changes in public policy involved a loss of professional guarantees. Teachers were stripped of their position as public servants to become employees of municipal corporations, without the professional and labor guarantees to which they were entitled previously. … The dismantling of the Medical and Teachers Associations [i.e., their Colegios] added to stimulating unbridled competition in the area of social policy. Physicians, as well as members of other liberal professions, were affected by the decision to change the legal status of the professional associations. Arguing that these associations were structures of privilege and monopoly, the military government decreed freedom of affiliation and eliminated the requirement of enrollment in these associations for practicing their profession.
>
> As a result, the professional associations became trade unions with voluntary affiliation and lost their control over the practice of the profession. Originally organized in a national labor union, teachers' leaders had suffered harsh repression from the very moment of the coup d'état, and their possibilities for action as an association were reduced drastically. In order to assimilate them with the other liberal professions, the military government dissolved the teachers' labor union and forced them to affiliate as a trade union. The new structure was very different from teachers' traditional forms of organization. (Espinoza, 2001: 3-4)

Colegios were influential organizations, expected to actively participate in public discourse. The transformation from Colegio to Gemailes challenged this authoritative role, while also reducing the professional middle-classes' ability to organize and resist the policies of the dictatorship. as the example of the Colegio Médico illustrates:

> Since its foundation in 1948, the Medical Association [Colegio Médico] has played an active role in the country's public health concerns. It has also taken care of all aspects of medical practice, including: scientific improvements, hourly and per-service pay scales, professional ethics, fees, job security, the expansion of coverage and of the health service institutions. As a legally incorporated entity, the association successfully proposed a number of important laws, decrees and regulations in an effort to guarantee the health of the population as well as to benefit the physicians themselves.
>
> In 1981, the military government modified the Medical Association's status as a corporation and reduced it to a trade union dependent upon the Economy Ministry. As a result, the Medical Association lost significant powers, such as the power to require all physicians to belong to the association, to set medical fees, to determine who could practice medicine, and to set professional and ethical standards for their peers. All these powers were considered monopolistic, distorting the allocation of resources by the market. Despite this situation, the association has remained united and preserved not only its prestige but also its considerable social influence. (Espinoza, 2001: 9-10)

When in 2010 a group of archaeologist students and archaeologists working in Impacto decided to form a "Colegio", therefore, they were drawing on a concept of a professional organization that had a highly charged cultural and legal resonance in Chile.[5] Since the return to democracy Colegio's are no longer repressed and they have retained the aura of professional authority that the dictatorship's labor reforms attempted to erase. This goes some way towards explaining why the Colegio's first public statements were printed immediately in national media outlets, despite them being such a new and (within the archaeological community) controversial

5 Although the lines are slightly blurry, the Colegio de Arqueologos is called a "Colegio" in its title, but an "Asociacion Gremial" in its legal documents.

organization. But it also led to unanticipated problems. The Colegio's founders were all non-titulo holding archaeologists and intended their new organization to reflect their radical reinterpretation of the notion of professional status. Unfortunately, the lawyer who drew up the legal documents for the new Colegio de Arqueologos was not informed of this intention and when the documents arrived it was discovered that they followed the still existing legal model of a Colegio. Thus only holders of a legally recognized Titulo Professional were able to be members. The founders were forced to find legally recognized colleagues to sign their own organization into being for them, much to the amusement of some of their critics. Ultimately a solution was reached that created a two-tiered membership: those without a titulo would officially be known as honorary members.

Younger archaeologists and students I spoke with expressed excitement about the new organization, but it would be misleading to see the arguments about the Colegio as only a generational split. Rather, the opposing sides represented different opinions on how best to respond to what everyone considered to be the challenge of Impacto: archaeology as a non-expert practice and archaeological knowledge as kind of tangible, quantifiable thing. One afternoon I sat talking with Felipe, a young third year licenciatura student, about his excavation experiences. He had so far only been to the field twice. Once to work on an impacto project and the second time as part of a practical class on excavation theory and practice that had involved participating in one of Jorge's excavation projects. His experience on the impacto project had been terrible, he told me. It was very stressful because they were under enormous time restraints and there was a great deal of pressure from the mining company who were employing them. Even with his limited experience in archaeology, he had been disgusted by the way they were forced to excavate the site. It felt like they were only "mining for artifacts": there was no recording of stratigraphy or levels and barely any data on the soils collected at all. They just kept digging and pulling out artifacts all day until the site was clear of visible objects. At Jorge's excavation, in contrast, he told me he had been able to learn how archaeology is really done, and the atmosphere had also been both enjoyable and educational.

Although he was still a novice, Felipe was already attuned to the stakes involved in the debate over impacto archaeology. Impacto laws generate practices that are aimed towards the mass recovery of artifacts. This chimes with an understanding of archaeology as only the collection of valuable things. Impacto and other policies around the world, including CRM and PPG-16, arise out of a model of archaeological practice that focuses on artifacts in the landscape as a resource to be recovered, rather than archaeological excavation as a practice that seeks knowledge of the past through the study of material relationships within a landscape that includes, but is not limited to, artifacts. This model is doubly destructive because of its implications for archaeological professional and the value of their labor. If the aim is only to collect artifacts, then indeed little skill is needed on the part of archaeologists other than to know where to dig, and how to pick up the things they find. Archaeologists thus become interchangeable employees rather than skilled individuals possessing trained judgment. Relatedly, impacto archaeology assumes that archaeological knowledge is a bounded resource: it is literally a set of physical objects buried in the earth that can be dug up by anyone, and that only have relevance in terms of their physical preservation (e.g., as symbolic heritage objects, rather than as scientific data points).

The relationship between a neoliberal politics, impacto-style heritage resource policies, and the transformation of archaeological practice into deskilled labor, exists in many other countries. But the example from Chile draws attention to the entangled role of the university in this story. The attempt of the UdeChile archaeologists to use the Malla to define what an archaeologist should be and through this what archaeological knowledge production should look like; the desire of the Sociedad to define archaeological practice as a matter of pure scientific research; and arguments put forward by the founders of the Colegio who call for a more humane recognition of the reality of the labor market among young archaeologists: all point towards a struggle over the nature of what it means to be a professional, and from this what professional (i.e., expert, authoritative) knowledge can be. Both the Sociedad and the Colegio are framed as organizations through which the community of archaeologists can disseminate knowledge to wider society. The argument between them is over who gets to be part of that community and what they are going to say. Not, crucially, over the assumption that there *is* something to say, and that it *ought* to be said.

Conclusions

This chapter has traced how Chilean archaeologists have responded to changes in their institutional structures, specifically the reorganization of the university and commercial archaeology along the lines of a neoliberal logic of knowledge. Rather than seeing universities as sites where units of economically useful knowledge are passed from teachers to paying students, Chilean archaeologists insist that universities are sites where authoritative professionals create knowledge that is of critical use to society. Both current archaeologists (faculty) and future archaeologists (students) are orientated towards the production of this socially relevant knowledge. Further, this knowledge is not unproblematically knowable. To access it, novices must become experts by acquiring a wide range of subtle skills: technical and craft skills involved in artifact analysis, formal training in field methods, a knowledge of prehistory and ethnography; but also the social skills necessary for living with other people on field projects, the ability to develop research questions independently and as part of a team, to engage in critical reflection on academic questions, to draw upon established literature and knowledge of other projects, and to appreciate the significance of archaeological and anthropological knowledge for wider society. These skills are not so easily taught or learned, they insist, but are instead part of a professionalization process that incorporates individual students into an already existing and adapting disciplinary community.

I do not present Chilean's rejection of the 'knowledge nugget' model as a simple matter of heroic resistance, however. As some of the complications over the Colegio, Impacto, and the Malla have indicated, Chilean archaeologists are in many respects placed in the position of constantly responding to conditions that they feel are beyond their control. In the next chapter I will consider in more depth this feeling of disempowerment by looking at who or what is considered to be responsible. Before doing so, however, I want to consider what the theory of knowledge Chilean archaeologist's claim to uphold, actually is. What does it mean, "to discuss, with the aim of having a critical perspective on society"?

During an interview, Matías and I discussed the points-based system of awarding funding for research project through FONDEYCT (Fondo Nacional de Desarrollo Científico y Tecnológico/National Fund for Scientific and Technological Development), the Chilean equivalent of the NSF.

> Matías: But now, the [ministry that controls FONDEYCT], they are not thinking about the interest of this knowledge in society. What they are interested in is the-- in the productivity. So if, if archaeologists can show, prove that they have good

levels of productivity-- that means they have enough papers, indexed papers-- especially in these papers with good citation indexes or averages. Then they can make a point that it's good to receive funding for research. Even through research can be completely of no use.

Mary: It's just a matter of getting those numbers of papers.

Matías: Yes exactly. Actually the head of the, the, scientific commission of FONDEYCT, he happens to be a historian, very close to anthropology. And he came a few months ago, because he said he was a little worried. Archaeology for the last few years, was not applying for so many projects as it used to do. So he said that means, contrary to what one expects. One would probably say that means we have less competition, that is better for us. But he said that is the opposite, because "I don't have arguments to try to give more funds to archaeology things", because there are very few projects and stuff. So you see, he is not thinking "what is the use of archaeology", to see if we will fund them or not. He is just thinking of numbers. How many projects are being applied, and if those projects have good levels of productivity. If those two numbers are good, then let us give them the money.

Mary: So it's a sort of circular argument that if more people apply, then you'll have more publications, then more money will come to you.

Matías: Exactly. Exactly. So it's science for science itself. It's not eh... nobody is thinking about the social relevance of this. It's a matter of scientific indexes of numbers.

Mary: So it's sort of they're, they're trying to... They're trying to make a point about Chile as a research center. And to produce... huge amounts of research rather than, the, the-- publishing in international forums... if it ticks all these boxes--

Matías: Mm. Mm. I suppose so yes. There must be an idea behind this. It must be, eh, what is very common nowadays, is that this, eh, this... what they call in Spanish this "*Sociedad Conocimento"*, this "Knowledge Society". And the importance of science and technology for the development of our country. It's not a matter of only economics, but also of your human... capital. So they are investing in human capital and they think investing in human capital is to have good scientists. Well that's at least one way. Not the only one. But I think that's the logic behind it. I mean having good human capital and to have good international standards. I would say probably a mature-- I have not talked to these people about this. But I suppose that's what's behind it. [My emphasis added.]

Matías and I had previously talked about the remarkable *lack* of interest in archaeology in

Chile. Archaeological research is supported by the government through FONDEYCT funding, but during the 14 months of my fieldwork I rarely found any references to archaeology in national or local newspapers, nor in other popular media forms. On a trip to San Pedro de Atacama, a town considered by archaeologists to be the most significant site of archaeological tourism in the country, I eventually gave up asking either tourists or locals working in the hotels, restaurants, and tour companies about archaeology, because so few people even knew what archaeology was. My inquiries were frequently met with corrections: didn't I mean "astronomy"? Particularly in comparison to neighboring Peru or Bolivia, archaeology is not part of the national imagination in Chile. At the same time nearly all the archaeologists I met commented on the disparagement or ignorance of Chile's prehispanic history and contemporary indigenous populations. The lack of interest in archaeology was the result of a very racist nationalism, they argued. A preference for tracing the roots of the nation to Europe, choosing to selectively appropriate and incorporate only elements from a suitably historicized past: the Mapuche's "warrior" nature, for instance (Richards 2010). The significance of undertaking archaeological research, therefore, was bound up in a project of counteracting the racist narratives of modernity that dominate national discourse. But also the possibility, as archaeologists like Matías argued, of using the social sciences to imagine alternative futures—even though it is hardly easy to do so and he mostly focuses on a local scale rather than the national.

> Matías: The role of the social sciences I think, is to make people knowledgeable about their conditions. So that they can *choose*, that they can choose from a critical standpoint, from a knowledgeable standpoint, not just because that is the way the world is going. Because I think the world is going in an *awful* direction! And I would not like Latin American societies to become like European societies – due respect of course! Or like North American societies. I don't think they are models to-- in *some* respects, of course they are. But I don't think they are models

> for... so... so... I, I think we have to contribute, then, at least to think critically. And one way as I told you then, is make them-- try to make them reconstruct amongst them, amongst that, reconstruct what that traditional living was. To make instances in which they can reflect what is happening, why-- what are we losing, where are we going?

Such considerations—of how to model alternative modes of being in the world, of how to recognize the prehispanic past outside of narratives of primitivism—would always be lost within the techno-bureaucratic logic of the FONDEYCT funding system, leaning as it does on a *quantifiable* system of evaluating knowledge. The Malla and Impacto debates are ultimately debates about knowledge—what it is, and how do we train someone to become an expert so that they can access it. These debates do not yet have clear answers, except that they must be addressed in response to a reductionist, knowledge-nugget model. What this conversation with Matías also raises, however, is the complex relationship between the national and international context of knowledge production. It is to this that I will turn in the next chapter.

Chapter Seven

Looking for agents between the US and Chile

Introduction

Throughout this dissertation I explored the effects of universalist assumptions at local and then national scales. Here I want to turn towards a transnational or supranational scale. Universalism posits that things, people, and ideas are able to circulate freely and thus be compared against a single standard of measurement. But circulation is not just about the movement of ideas and things into new places from old: circulation itself is always a cultural process. Cultures of circulation, to use Lee and LiPuma's (2002) phrase, involve not just the things that move and the forms that movement takes, but also the presupposition of an interpretive community into and out of which ideas, people, and commodities come and go. This kind of circulation has been understood in terms of the problems of translation: the production, transmission, and reception of things and ideas is not an simple matter (Gal 2003) because "These interpretive communities determine lines of interpretation, found institutions, and set boundaries based principally on their own internal dynamics." (Lee and LiPuma 2002: 192). Circulation is thus not just a matter of how ideas and things "flow" from one place to another, but involves "friction," as movement encounters resistances and reincorporations that themselves generate novel and unexpected forms (Tsing 2000, 2005).

The idea of unimpeded universal circulation, however, is a powerful one. Benedict

Anderson's (1987) concept of the nation as an imagined community points to how the idea of being part of mass circulation works to create a sense of a collective public: "[R]eaders of narratives disseminated translocally through print identify with both the audience addressed by the narrator and the narrated-about characters, and become aware of the existence of like minded readers who share similar identifications. The 'We' of nationalism is the tropic embodiment of these two identifications." (Lee and LiPuma 2002: 195-6, discussing Anderson 1987). But Susan Gal points to the historical specificity of this concept of 'the public', in relation to who it excludes through its recourse to disinterestedness and decontextualization:

> [G]roups can become politically dominant, at least in some kinds of societies and in particular historical periods, exactly by constituting themselves as the natural, unquestioned members of a disinterested, anonymous public. … Thus, public is not an innocent or transparent term linked only to audiences… . Rather, within the Western tradition, the broad notion of a public is a form of political legitimation in which the decontextualization and depersonalization of language produces the image of a social group uniquely fitted to govern because it speaks to no-one-in-particular and thus can stand for everyone. (Gal 1995: 418)

'Interests' and 'local contexts' are seen as impeding the otherwise natural flow of ideas and things through a shared public sphere, and thus the act of assuming unimpeded circulation in a shared public sphere can be interpreted as a claim to authority by those who present themselves as the most disinterested and decontextualized.

I want to use an example from Chile to extend this argument and particularly the concept of a national public, by considering the complexities of *supranationalism*: a claim to inhabit a public sphere that transcends the national and can therefore be utterly disinterested, that thus becomes a claim to greater authority. This arises out of a consideration of the way accusations of "nationalism" were frequently used by North American archaeologists to cast doubt on the credibility of their Chilean colleagues. Debates within archaeology during the 1990s and early 2000s were heavily critical of 'nationalist archaeologies', pointing to the oppression and silencing of those often excluded from a national public sphere on the basis of race or ethnicity (e.g. Kohl and Fawcett 1995, Hamilakis 1996, Gathercole and Lowenthal 1990, Abu El-Haj 2001, Scham 2001, Meskell 1998, Díaz-Andreu and Champion 1996). Smith (2004), however, has argued that archaeology now finds itself in an untenable position, as it simultaneously seeks to promote universalist 'world heritage' as a way of combating nationalism, while privileging localized, bounded identifications with the past as a means to empower the subaltern (c.f. Schmidt and Patterson 1995). The subaltern who must be empowered by archaeologists is by definition never 'national'. In this formulation, a collective smaller than the supranational and greater than the indigenous is always treated with suspicion.

In practice, I will argue, the nuances of the debates about nationalism found in these journal articles and books—read, like Anderson's novels and newspaper, by archaeologists around the world—blur on the ground and nationalism becomes an easy label to apply to any archaeologist working within their own country who objects to a foreigner who wants to work there as well. The insinuation is that those who are able to transcend the national and become *supranational* have a more disinterested and thus legitimate perspective than archaeologists bound to their own national context. Moreover, as I argued in Chapter 4, as a field science archaeology relies on a physical and epistemic separation between the field as a generically 'local' space in comparison to the non-field/home as a 'global' space. When movement into the field is strongly associated with foreign travel, as is the case for North American Andeanists, the country into which one moves (Chile, Bolivia, Peru, etc.) becomes an extension of the field site and thus a less epistemically secure space. Building on this argument, in this chapter I will use two further examples from Chile to argue that both this concept of disinterestedness and the idea of supranationalism are coupled to a specifically US perspective on legitimation at the national and global level. As Sheila Jasanoff (2005) and Theodore Porter (1995) have both argued, in the US expert authority is rooted in the denial of personal or political interestedness to the extreme that "[t]he ideal is a withdrawal of human agency, to avoid the responsibility created by active intervention. Subjectivity creates responsibility." (Porter 1995: 196). Supranationalism, as a claim to authority based on the negation of interestedness, may thus be more pronounced among US scientific communities than those in the Global South.

As I began to describe at the end of the previous chapter, Chilean archaeologists' conceptualization of nationalism is complicated, drawing as it does on both the idea that their work is orientated towards 'Chilean society' and a critique of the way archaeology is used by the Chilean State and much of the non-indigenous population to promote a racist, Eurocentric Chilean national history/identity (see also Uribe Rodriguez 2003 and Troncoso et al. 2008). At the same time they are critical of North American and European archaeologists who work in countries other than their own and who have hegemonic control over international standards of academic production through control of, for instance, the internationally ranked journals that Chilean archaeologists need to publish in to secure research funding within Chile (see also Politis 2001). Such criticisms were frequently reduced to accusations of 'cultural imperialism' which I intend to interrogate alongside the use of the term 'nationalism' by North Americans.

In essence I see the accusation of 'imperialism' to be a rejection of supranationalism. Through two examples—the first of a dispute over a North American field school in the north of Chile, the second the trend in the last decade for Chilean archaeologists to go abroad to get a PhD—I will work through two related problems. The first is a problem of universalism or commensurability. When North American archaeologists assume that Chilean archaeologists share the same concept of what 'good archaeology' is, or that their professional and educational cultures are the same, they are invariably assuming that Chile is commensurable with the US. If a single standard of measurement is to be used, it comes from the US: but at the same time this standard is assumed to be disinterested because it is posited as supranational in opposition to an insufficiently disinterested nationalism.

The second problem is that of tracing who, exactly, the agents of these processes are. To what extent are individual North American archaeologists responsible, given the structures of neoliberal educational theory and policy that have shaped the institutions of scientific practice in both South and North America? Relatedly, to what extent can a group of North American archaeologists be understood as representing, coming out of, or *being* 'The US'? To a certain extent and within particular debates, Chileans conflated the US, the Chilean State, supranational organizations like the WTO and OECD, and neoliberalism. All become interchangeably termed "The Global Model": something that is faceless, abstract, and hard to pin down, but for this reason all the more unstoppable. While the previous chapter looked at means of resisting or purposefully mistranslating neoliberal conceptualizations of the value of knowledge, education, and professionalism, here I paint a less optimistic picture and look at moments when acquiescence to the Global Model is seen as negative but inevitable. In this chapter, therefore, I will attempt to connect these two problems of commensurability and agency by not necessarily *answering* the question of who the responsible agent is in any one case; but by exploring the extent to which we can understand the Global Model as a kind of supranationalist public, always already assuming the unproblematic circulation of universal values, things, and people. That which claims to be everyone because it comes from no-where, but in fact can be traced back to a set of US values which are just as local, and just as historically and culturally contextual, as anywhere else.

The problem of the North American Field School

"We are all one big family" Chilean archaeologists would tell me, when describing their own community. Which made the biannual *Congreso Nacional de Arqueología Chilena* held in Valparaiso in October 2009 a giant family reunion, as much as it was a chance to publicize or discuss academic papers. This comment implied a sense of relaxation and general informality, but also the sense that everyone knows each other well and is—somehow or another—connected to everybody else. On the one hand this sense of connection and informality led to a highly relaxed and enjoyable atmosphere for those within the family. At the same time, however, it made it very hard for outsiders to find a way in.

Like the interactions between the North Americans and Bolivians described in previous chapters, Chilean archaeologists held onto an ethic of informal casualness that was performed in very specific ways and was held up as a contrastive moral value: in this case, Chileans prided themselves on being less hierarchical than 'Gringos'. The same concept does similar work in both places (North Americans : Bolivians, Chileans : Gringos) but it would be a mistake to assume that this means archaeology is the same world-wide. During the Congreso in 2009 I observed some of the local features of the Chilean archaeology "family" (i.e., the socialization of new members into a disciplinary and epistemic culture through students being interpolated as future-colleagues; the stylistic emphasis on informality and familial casualness), as well as the frustrations of a North American archaeologist, Ethan, who encountered serious resistance to his attempts to work with and in this community. The very negative and contentious reaction to Ethan's field school project brought to the surface the work that accusations of "nationalism" and "imperialism" are doing within these cross-national encounters. But they also illustrated the effects, for those in the North *and* South, of supranationalist assumptions of commensurability held by North American archaeologists encountering quite different scientific, educational, and professional cultures in countries like Chile.

Casual collegiality

The 2009 Congreso was being held in two separate buildings, one of which was an archaeology museum. During the morning and afternoon, the timetable stipulated that sessions would break more or less simultaneously to allow everyone to gather downstairs in the museum, in rooms furnished with tables, chairs, and a generous spread of coffee and cookies. Here small groups talked animatedly, as old friends and colleagues greeted each other and embraced. At one table two men were hunched over a laptop, examining pictures of arrow heads on a power point. A group of students from the Universidad de Chile (UdeChile) stood in the center of the room, comparing notes on the talks they had heard that morning. Nicolas, a slightly older student, came in and said hello to his classmates before noticing the men with the laptop and going over to greet them affectionately. Catching sight of Nicolas coming towards them with a big smile, one of the older men jumped up to embrace him, laughing and slapping him on the back. They stood chatting for a while before Nicolas turned to get coffee and in doing so ran into another older female archaeologist he knew, before yet *another* came over to say hello as well—this time a younger woman with a bright pink backpack, who greeted him delightedly as if they haven't seen in each other in a long time. The younger students from UdeChile carried on gossiping among themselves in the middle of the room, one of them watching Nicolas closely as he moved around laughing, smiling, and hugging colleagues from around the country. Hopefully in time these younger students would have as much difficulty walking quickly through the conference coffee room as Nicolas. Indeed, during the question and answer part of the sessions, the hands of young *licenciatura* students seemed to go up as often as those of older professionals and professors, and more often than not the moderator acknowledged the student by name. Outside of the UdeChile classrooms and among the broader archaeological community, the students were performing their role as desirable “future-colleagues” to a wider audience than their immediate professors.

Nearly everyone at the meeting was Chilean, apart from several Argentinians who mostly appeared to be presenting the results of projects conducted in collaboration with Chilean teams and a few Bolivians I recognized who had studied at Chilean universities. This was certainly not an international conference, but I spotted two ‘Gringo’ men in the crowd almost immediately. Like me they appeared over-dressed—both were wearing light summer suits with button-down shirts and ties—and were usually standing on their own during the otherwise highly social coffee breaks. In contrast, only a small handful of Chileans were wearing shirts and suits. On the first day I too had donned one of the conference outfits I have put together after years of attending North American and European archaeology conferences: a selection of formal gray and black pants or pencil skirts, matched with fitted blouses in shades of blue, dark purple or deep red, worn with a smart overcoat and under-stated jewelry.[1] In Valparaiso, however, my student equivalents were wearing exactly the same casual jeans, sneakers, and bright colored sweater

1 See the longer discussion in chapter four of the significance of clothing at Andeanist conferences in North America and specifically the way gendered ‘office dress’ at conferences is used as a means of contrasting with androgynous, casual clothing in the field.

ensembles they wore everyday on campus. The female students invariably had long colorful scarves and elaborate earrings, while several of the male students wore their hair below their ears, with band t-shirts or the kind of hand-knitted sweaters popular with tourists in places like Cuzco and La Paz. The non-student attendees were equally casual in battered jeans or colorful velvet skirts, brightly colored Doc Martins or well-worn hiking boots. A few more sober black polo-necks matched with leather jackets and serious expressions. But on the whole the feeling was studiously casual, if not outright bohemian. The sense of kinship-like collegiality and meritocratic informality that the Chilean archaeologists boasted of (and contrasted with North American hierarchical stuffiness) was being acted out in contrasting clothing. On the second day one of the Gringo men was somewhat self-consciously wearing the same suit pants as before but with a plain black t-shirt, while the other, Ethan, continued to wear his formal blue blazer and white shirt the entire week.

Controversies over the Field School: accusations of 'nationalism'

But the style of Ethan's dress was not the only reason for the side-long glances thrown his way, or for the fact that at each coffee break he stood on his own amid the animated conversations. Ethan was at the Congreso to present almost an entire session's worth of papers from the North American Field School (NAFS): an archaeological field school for US undergraduates that had recently finished its last season of excavation in the Tarapacá desert, and of which he was one of three directors. Unfortunately, in 2009 none of the other non-Chilean members of the team had been able to attend the Congreso. For reasons personal (a graduate student was giving birth to her first child) and impersonal (drastic budget cuts in the immediate aftermath of the 2008 financial crash meant the conferences stipends of participants from two separate universities had been rescinded), Ethan was the only North American team member in attendance and would be reading out all the other papers.

Just before his session started he came over to sit next to me and whispered loudly that the Chileans were angry with him for not letting them known ahead of time he would be the only person there. Slots were tight and apparently the organizers would have given them to other Chilean archaeologists if they had known. “It’s another insult to Chile that the presenters couldn’t be here” he sneered, with a heavy dose of sarcasm. The stakes for this session were always high, however, without the added disaster of the mass no-show. As Cristobal, one of the professors from the UdeChile had explained to me over coffee earlier that day, Ethan’s session was always going to be very tense. There had been a highly controversial show-down over the NAFS at the 2007 Congreso, when the idea was first made public of allowing a US team of archaeologists to set up a field school for paying US undergraduates in the highly prestigious Atacama desert region.[2] It had gone ahead despite vehement objections, making the NAFS the subject of an acrimonious debate ever since. Cristobal explained that *this* year the NAFS was under pressure to produce results that would salvage the reputations of Ethan, his team, and their few Chilean supporters.

When I initially proposed studying the NAFS in 2007, Ethan had first given me a reading

2 Without space here to go into details, I can briefly explain that the Atacama desert region in Chile is where most archaeological work has been conducted and subsequently was described to me as the most “crowded” archaeological region. As Chileans would phrase it, “the division of territory is feudal” and archaeologists with long established seniority are reluctant to allow newcomers onto “their turf”. This incidentally also points to the significance of the mentor-like relationship that young archaeologists have to build up with older, more established archaeologists, if they want to have a claim to working in regions like this in the future. But it is not merely a matter of happenstance that archaeologists have been working in the deserts of Chile the longest. Being the second driest place on earth, the preservation of archaeological material in the north of Chile is truly exceptional. Working there and having access to the outstanding material that is often found, is considered a great privilege.

list that included many of the classic texts on archaeology and nationalism I refer to above. Before and during the 2009 Congreso he saw the controversy surrounding the NAFS as a matter of "irrational Chilean nationalism". When, after only three years of excavations, the situation became so contentious that they had to relocate entirely to Peru, he had not taken it personally. As he explained to me over lunch the day before the session, the problem lay with the Chileans. Chileans have a lack of ambition or interest in the rest of the academic world that makes them closed off to foreigners, he told me. What's more, they have a lot of undirected anger about the dictatorship, because it ended without a proper reconciliation that would result in either punishment or forgiveness. For this reason they struck out against a strawman (namely the US, although this would be better described as a partial strawman, he conceded, because it was true that the US had indeed been partly responsible for the coup). That Chilean archaeologists today should still resent US archaeologists was nothing but misplaced anger, Ethan explained. So when he was attacked at the Congreso two years before, in his mind it had been nothing more than Chileans defending what he sarcastically referred to as "their honor".

Since arriving in Santiago a few months previously I had heard other versions of this story. Many volunteered the topic not long after hearing of my earlier fieldwork in Bolivia looking at relationships between Bolivian and North American archaeologists. Although I did meet a few Chilean archaeologists who were sympathetic to the NAFS, I met many more who saw the project as yet another example of "US imperialism" and Ethan's behavior as the typical arrogance of a "cultural colonialist" who had little respect for his local colleagues.

For many of the Chilean and North American archaeologists I spoke with, the controversy remained at the level of angry accusations of Chilean nationalism and US imperialism. To an extent, there is truth in this perspective. But the controversy has broader implications that make it important to look for an explanation beyond a single individual's inability to choose diplomatic words, or a series of miscommunications in planning a conference program. The NAFS controversy has its roots in differences in the cultures of education and professionalism in Chile and the US, but more specifically in the dynamics between these two different systems. Although such differences encompass matters of taste and style that might be sources of initial discomfort or misunderstanding among those trying to pass in a new academic culture (such as wearing jeans rather than a suit), these performances of particular forms of sociality are only the more obvious signs of larger differences in disciplinary culture and structure that make them into separate epistemic cultures. Specifically in this case, differences in conceptualizations of professionalism, the education of new members of an academic community, and the relationship between the profession and wider society: all of which encompass deeply-rooted ethical considerations.

Ethan's accusation of nationalism was in essence an accusation of irrational interestedness. According to his understanding, Chileans are weighed down by a misplaced sense of history and their wounded pride. He believed that their supposition that they have the moral (if not the legal) right to control who conducts research within their national boundaries and to deny access to foreigners like himself is thus irrational, arising as it does from nothing more than a wish to punish contemporary US archaeologists for the US government's foreign policy mistakes in the 1970s. Further, because Chilean archaeology is closed-in on itself—i.e., does not explicitly speak from, or to, a non-national audience—the kind of work being presented in other sessions at the conference is of limited value. The logic of this criticism is thus that because Chilean archaeology is concerned only with Chilean data and a Chilean audience, it is unable to circulate: it remains nationalist in the sense of being bound to a specific location, and in the sense of serving politically suspect nationalist ends. The contrast is with his own *supranationalist* perspective and practice: supranational because it can move easily across national borders, seeing southern Peru as conceptually no different from northern Chile; is located within a scholarly discourse that circulates beyond any single country (i.e., beyond *this* single country); draws on a non-nationality based right to conduct research in any specific location; and is thus *rationally disinterested*. This is explicitly a value judgment justified along epistemic grounds: nationalist science is interested in political, social, or historical contexts and therefore is suspect, while disinterested supranationalist science separates itself from such local contexts and is therefore more authoritative.

Unrecognized non-commensurability: paying for field schools

Much of Ethan's critique ignored the reason Chilean archaeologist gave for actually objecting to the NAFS: namely, that it was an undergraduate field school. This argument remained opaque to Ethan, leading him to dismiss it as an excuse to hide their underlying, irrational prejudice against foreigners. In fact, the field school problem is revealing of how the Chilean and US communities of archaeology lack a common conceptualization of the stakes involved in the definition of archaeological education and professionalism. This lack of commensurability is perhaps the best illustration of the mistranslation that can occur within 'cultures of circulation': an inability to see difference and the unintended consequences of assuming similarity. It also raises questions about the extent to which Ethan's supranationalism can be equated with or separated from either the neoliberal concept of knowledge promoted by supranational organizations like the OECD, or a US concept of education, expertise, and knowledge that is just as historically, culturally, and politically bound to the national sphere as that of Chile.

As described in the previous chapter, academic culture in Chile differs from that of the US and particularly in terms of how Chilean students are conceptualized as already member of the archaeological community. When asked what qualities a student should have if they wanted to be successful, both Chilean students and professors discussed the need to be self-motivated and persistent. As one student put it, to get to the position of Nicolas (the older student at the Congreso described above) you have to be prepared to spend a lot of time knocking on office doors. First contacts are made through initiative and persistence, and you must then prove yourself in the field by working hard and demonstrating you are worth taking into a mentor-like relationship. The desired result is that you will be taken on as an established member of a larger research team and learn the embodied social and technical skills necessary to be able to perform archaeological expertise through an apprentice-like participation in and out of the field (c.f. Chapter 3). Importantly, while working on these projects students will always be paid, signaling to themselves and others that this archaeological fieldwork is *professional* work rather than an amateur pursuit. But with only a limited number of professors running active projects and an over-abundance of new students each year, it is the responsibility of the student to make sure they are given the initial invitation.

In contrast, in the US 'field schools' are an integral part of the education and socialization of new archaeologists (Baxter 2009). Although the distinction is debated, a field school is usually quite different from a purely research orientated field project. Like the North American-run projects discussed in previous chapters, Chilean excavations are first and foremost research projects, and as such they are staffed by experienced archaeologists who often have their own responsibilities and nested research areas within the larger collective project. These kinds of projects only take a very small number of inexperienced students at any one time because watching over a novice takes up valuable time and resources. Moreover, the novice-research team relationship involves an investment in communicating both technical skills *and* professional socialization, and preparing the novice to become one's colleague and eventual successor. In contrast, US field schools are composed almost entirely of undergraduates who are overseen/taught by a small number of archaeologists who are often themselves only grad students. The focus is shifted towards teaching rather than research, and the model is framed around a conceptualization of archaeological expertise as have a separate 'field skills' component that can conceivably be acquired relatively unproblematically during a 4-6 week standardized course taught by graduate students who themselves might only have limited experience.

This field school is likely to be the only substantial methodological training US archaeologists receive before beginning their own research projects. This suggests a difference in the way Chilean and US archaeologists understand archaeological skills to be teachable and learnable. But more crucially for the Chilean archaeologists' objections to the NAFS, US educational culture is also based on the expectation that students *pay to learn*. Because undergraduates pay to participate and receive course credit, field schools are an acceptable way to fund and staff excavation projects that otherwise would not take place. This dynamic—that undergraduate students pay to take part in an process that is expected to be educational, but also provides labor to conduct academic research—is at the heart of the NAFS controversy.

Ethan genuinely could not conceive of field schools as objectionable because in the US they are integral to undergraduate education and socialization. From a Chilean perspective, however, the matter of payment took on quite different connotations because it was seen as the most explicit instantiation of the student-as-consumer model that Chilean archaeologist have been pushing back against for the past three decades.

A week or so after the Congreso I found myself discussing the NAFS with Sofia, a Chilean archaeology professor at the UdeChile. Sitting together in a dusty artifact processing lab in the anthropology department, we began chatting while working at either end of the thin room. After asking about my work in Chile and describing her experiences in a European PhD program, we began talking about the US. Almost immediately she started to tell me about a field school that had come to Chile a few years before that I quickly recognized as the NAFS. In the context of a conversation about different disciplinary traditions around the world, she brought it up as an example of something both bizarre and slightly scandalous: “Have you heard about this crazy thing? They make students *pay* to go and work with them! At least here in Chile, we pay students for their labor. Sure,” she said, holding up a finger and thumb close together, to gesture a minuscule amount, “what we pay is a tiny amount. But the point is *we pay them*. It’s the principle!”

There was a great deal of indignation in her voice as she warmed to the topic and a palpable sense of scandal and outrage. These were opinions and arguments I heard from several Chilean archaeologists and students during my time in Chile, but Sofia was particularly eloquent on the point. Our artifacts now put aside, we lent against the high formica-topped lab surfaces and discussed the field school in detail. The problem, she insisted, was that making students pay changed the whole dynamic of the relationship. Students would expect something more than to learn—they want some kind of “experience” and specifically to work with mummies. “So our Chilean heritage, the amazing archaeology we have here, just becomes something to be sold as an experience to American undergraduates. And they don’t even intend to become archaeologists! *Of course* you would treat two students differently, if one was paying to be there. But this is not the way we do things here in Chile. We don’t just lay out mummies for people to learn from, no matter how much they pay!"

The insinuation in her description was that the field school was not being run in order to do legitimate research but instead to make a profit. Further, the actions of the Gringos and their arrogance had made things even worse. When Ethan first arrived in Chile to propose the idea at the 2007 Congreso, Sofia said, he came to speak to us and explain what he wanted to do. But when he arrived, the first thing he said was "We have lots of money!!" (At this point Sofia was on her feet, arms waving up in the air in a parody of a shouting Gringo). "We have lots of money! Lots of money! Not 'Oh thank you for inviting us, it's so nice to be here.'" (a theatrical hand-to-heart gesture of formal thanks). "No! The first thing he said was about money! Oh, we will build a big building and will *let* you use it!" Here she switched into acting out a grovelling Chilean, hands clasped and bowing low: "Oh thank you, thank you! So kind, so kind!" But then she suddenly stood upright and dismissively brushed aside her imaginary submissive groveling with a quick swipe of her hand.

The NAFS project was the first US field school to set up in the north of Chile and only the second in the country since the return to democracy. Unlike neighboring Peru and Bolivia, Chile was widely believed to be closed to foreign researchers after the military coup of 1973.[3] A small number of US archaeologists were invited to work in Chile, although this was seen by some Chilean archaeologists as being part of the abrupt shift during the dictatorship away from the previous tradition of Marxist inspired Latin American Social Archaeology and towards the more acceptable ecological functionalism of Processual Archaeology that dominated in the US during the late 60s and 70s. Only processualism was politically safe, and the only foreigners invited to

3 Prior to the coup a number of collaborative projects between archaeologists from Chile and the US existed, for example the excavations of D.L.True, Lautaro Núñez and Patricio Núñez in the Tarapacá region during the late 60s (True, Núñez and Núñez 1970, 1971)

come into the country were those from the US who had strong processual credentials.[4] The projects that *had* been set up, however, were widely seen as active and respectful collaborations. They had also been entirely research projects, rather than field schools of the kind increasingly common in Peru and Bolivia.

The NAFS, then, was a novelty to Chilean archaeology. But this alone does not account for the overwhelmingly negative reception it received. The controversy illustrates the difference in educational and academic culture among archaeologists from the two different countries and an unwillingness on the part of Chileans to normalize the marketization of education and research. Since democracy was restored in 1990, education reform in Chile has been the most visible and effective arena of resistance to the continuation of the neoliberal social experiment begun in 1973. The clearest symbol of these policies is the continued—and indeed rising—fees that students must pay to attend university. As such, the suggestion that students pay to take part in a field school is highly loaded. Being taken on a field project is a right that a student has to earn in Chile. It implies a commitment on the part of both the student and the professor to a professional relationship that will continue into the future. This commitment is not something that can be bought or sold: just as the products of field work—whether quasi-touristic ‘experiences’ or artifacts like dessicated mummies—should not be bought and sold. Against this background, the resistance to the archaeological field school can be seen as part of an on-going opposition to redefining educational culture that is embedded within a much longer history of resisting a neoliberal social order imposed at the point of a gun. The US archaeologists, however, were unaware or uninterested in this larger history, and were working on the assumption the Chilean and US archaeological communities and educational systems were commensurable.

4 Dante Angelo (2005: 194) discusses a similar situation in Bolivia.

Moreover, the assumption was that if Chileans do not act according to the norms of US academic culture, their reasons for doing so must be spurious and irrational.

This example illustrates how a neoliberal tenet of education policy (education as a commercializable transaction) has become normalized in the US to the extent that an insistence on free university education is unlikely to be taken seriously. To what extent can we see someone like Ethan as acting according to a US, a neoliberal, or a supranationalist logic, therefore? To try and work through this question, I will turn to a longer burning controversy in Chile, namely the question of going abroad to get a PhD.

Locating the 'The Global Model'

The NAFS became the embodiment of everything that Chileans like Sofia suspect about Gringos: arrogant, motivated by profit, and dismissive of their South American colleagues. Moreover, their work was criticized for making overly-broad claims that could not be substantiated by their results. This mirrors Ethan's argument that Chileans are 'closed' and 'nationalist' in both their practices of exclusion and their intellectual work. In an interview another archaeologist responded that the projects of Gringos are 'imperialist' not only because of the assumption they can *buy* the right to work wherever they wished, but also because they are more interested in jumping to large conclusions in order to make Grand Theories, than they are in spending time studying a place in detail or creating projects that have social relevance.

It is possible that the NAFS was, in a sense, an easy target for resistance and critique by Chilean archaeologists because it became the embodiment of an otherwise abstract and difficult to pin down process against which they are constantly struggling. Namely, the expectation that they should measure themselves according to unwelcome standards that are presented as objectively good, because they are *doubly* disinterested by virtue of being supranational *and* in-line with ‘objective’ neoliberal values of knowledge/expertise. This struggle is larger than the conflict over the NAFS. But if during the dictatorship it was easy to see who one was fighting against because changes to the education system and to labor/professional relations were imposed at gun point; when the same policies are advanced during a democracy, who is responsible and who can one even argue with?

Looking at the reasoning behind a recent trend to go abroad to get a PhD illuminates how a supranationalist Global Model of education and scientific practice is understood to be something that comes from outside the Chilean archaeological community. But also how it is increasingly difficult to work out who the agents of these changes are and therefore how they can be critiqued or resisted. What is certain, however, is that this pressure is seen as coming from outside the discipline. In working through these problems Chilean archaeologists position themselves as being on the side of *Society framed in terms of the nation,* in opposition to the *State framed as a colonial outsider*. This, then, is a more complex situation than the simple term ‘nationalist’ would suggest. It invokes histories of struggle against the imposition of neoliberalism and the defense of democracy and professional autonomy, and it also invokes a complicated understanding of claims by the US to be a disinterested arbitrator of supranational public good in matters of scientific (and political) practice. To illustrate this point, I turn to look in more detail at the problem of PhDs in Chile.

Why do Chilean archaeologists need a PhD?

Within the last decade a PhD has rapidly become seen as a necessity by current and recently graduated archaeology students. There are two significant characteristics of how archaeology is taught in universities in Chile. The first is that until very recently there was only one university—the Universidad de Chile—offering degrees in archaeology. This means that with a few exceptions every archaeologist in the country is a graduate of the same department and may have even taken the same classes under the same professors. There is a common base, therefore, not only of shared skills and training, but also of social contacts and experiences.[5] The second significant point is that in 2010, apart from the new PhD offered at the UdeTarapacá (which students I spoke to were wary of taking because it is was new)[6] there were no PhD granting institutions in the country for archaeology. As a result, more and more Chilean archaeologists were going abroad to get a PhD after finishing their titulo professional at the UdeChile.

5 The effect of this was noticeable on one of Matías' excavations. Although like most projects he has a regular team that always works together and know each other well, in 2009 he also took a number of students who were new to the team and the region. At first there was some caution and shyness on the part of the newcomers. But as dinner-time conversation and banter turned time and again to increasingly hilarious anecdotes about various professors and classes, and different generations reminisced about the same experiences, a camaraderie was quickly formed. This shared educational background is seen as the reason why Chilean archaeologists frequently describe themselves as one big family.

6 In recent years two other significant departments have opened: the Universidad de Tarapacá, and the Universidad International SEK. The SEK is a private university based in Santiago that at the time I visited in 2009-10 had problems retaining faculty and attracting students. As the first generations of students graduate, there was much speculation over whether it will remain open, and whether its students have any chance of integrating into the wider archaeological community. Private universities are still seen as the resort of inferior students: while one UdeChile student characterized them as rich enough to pay fees but too dumb to get into the UdeChile, professors more sympathetically acknowledged that getting the grades necessary to go to the UdeChile usually required having attended a good private high school. A professor at the SEK spoke with passion about the problem SEK students will face being taken seriously and making contacts. At the Congreso, a UdeChile student pointed out the SEK students to me at the boisterous closing party. They were standing on their own in a corner, hardly speaking to anyone around them: something that does not bode well for their ability to be accepted into the wider "family". In contrast the UdeTarapacá had a good reputation: it specializes in the archaeology of the north of the country, and offers a Masters and a fledgling PhD program. But even so, the UdeTarapacá seemed to be almost an after-thought when I spoke with archaeologists in Santiago, something that probably reflects intra-country divisions and specifically the antagonism and competition between Santiago and the regions more than anything else.

But why would Chilean archaeologists need a PhD? A Chilean archaeologist with a titulo from the UdeChile has completed 4-5 years of intensive courses in archaeological theory, history, and ethics, lab-training in at least one methodological specialization, detailed courses on the archaeology of the regions they will work in, and had training and experience in archaeological excavation and survey. In addition, they will have taken several ethnographic classes on the indigenous groups of Chile, a course in the philosophy of science, and classes in other anthropological subfields (social and physical anthropology). Having completed their titulo they will have undertaken two major independent research projects, will have presented their work either at a conference or in publications, and by this point in their career will probably already be affiliated with an established research project, integrated into regional networks, and establishing their professional reputation.

In comparison, their fellow students in a US PhD program may or may not have majored in archaeology during a liberal arts bachelors that will have required only a handful of archaeology classes and may not have any field experience *at all*, let alone experience conducting their own research and designing/managing their own projects. For those heading to European universities, most of their fellow students will hold a specialized undergraduate in archaeology and thus will have taken three years of coursework. But the European PhD is designed as a first major research project and limited in scope to something achievable within three or four years—quite probably less time than the titulo. What do Chilean archaeologists gain by adding a PhD to their existing training and set of qualifications? Additionally, it is not just recently accredited archaeologists who, having finished their titulo, are going abroad in search of a PhD. A number of the faculty at the UdeChile had recently embarked on the same journey, including one very senior professor who regularly traveled to Argentina to take seminars for his part-time PhD, before returning to Santiago to run his own lab and teach his own students.[7]

The debate surrounding why Chileans needs to get a PhD is a debate about who sets the standard for academic accreditation. A PhD is in essence a sign of professionalism and expertise, and the insistence that a PhD is necessary *after* the Titulo Professional is an insistence that all current Chilean archaeologists are insufficiently qualified in comparison to colleagues from other countries, despite the actual content of either academic program.

FONDECYT Funding

One reason frequently given for getting a PhD was that it had become necessary to acquire funding. Funding for scientific research in Chile is generous and organized through FONDEYCT (Fondo Nacional de Desarrollo Científico y Tecnológico/National Fund for Scientific and Technological Development). FONDEYCT is administered by the Ministry of Science and Technology, and legally came into being in 1981 through the Laws No. 33/81 and No. 834/82, in tandem with Pinochet's restructuring of the universities the previous year. Its purpose is to fund the development of science and technology for the good of the country:

7 One of his former students affectionately joked that he had become a born-again post-processualist in the process. The professor himself certainly seemed to be finding the experience highly enjoyable, seeing it more as an opportunity to discover new perspectives and make new colleagues. He was as untroubled by the idea of leaving his own students for a week or so during the academic year, as he was by taking on new theoretical positions that contradicted much of his earlier work. His position is relatively unusual, however, because there was not the same sense of this decision to do a PhD being an *absolute imperative* in order to continue in his career, as young archaeologists described. Therefore in what follows, I bracket out for now the motivation for leaving of simply enjoying the learning process. Other more junior faculty who had gone abroad were also more likely to talk about personal satisfaction, the pleasures of making new colleagues and friends abroad, and enjoyment of a different intellectual climate as a motivation of their foreign PhDs. But in my discussion here I focused on a slightly younger generation of current and recently titled students, for whom the *need* to get a PhD appears to be far stronger than it was for those even a decade earlier.

> Law No 1. defines the universities as an institution of higher education, of research, reason and culture that, in the performance of their duties, must adequately pay attention to the **interests and needs of the country,** at the highest level of excellence; (DFL No. 33/81. Translated from Spanish. My emphasis.)

It is explicitly directed, therefore, towards the national interest, but archaeologists argue that in practice FONDEYCT is skewed towards an assumed non-Chilean audience and a Global standard of research and education because it places more value on publication in international English language journals than in Chilean journals and because it heavily promotes the acquisition of PhDs over the Titulo Professional.

Chilean archaeologist also argued that FONDECYT has been responsible for shaping the content and orientation of research. I was repeatedly told about the restrictive nature of FONDEYCT grant applications, which are seen as holding back innovative, socially relevant research. Archaeologists who wished, for instance, to include in their applications public outreach programs, or ethnographically informed engagements with indigenous or non-indigenous communities living near the archaeological sites they studied, found it difficult or impossible to do so. I gathered from the frequent conversations we had on this topic, that the Chilean archaeologists I interviewed wanted to make their research more socially engaged, but they believed this was impossible or at least very difficult, because FONDEYCT practices pushed a more 'objective' model of science—i.e., an idea of science that is entirely separable from its social contexts.

When I asked one archaeologist about FONDEYCT in an interview, she began to complain that US archaeologists always want to make big, grand theories that are too sweeping to be properly supported by evidence, whereas in Chile they preferred to make smaller statements that could be properly verified. Unsure how this connected to my question about Chilean funding, I asked her to clarify her point. She explained that FONDEYCT's funding application process was forcing them to conform to this "over-blown and boastful" US style of designing and presenting their research projects. Several other archaeologists attributed the lack of time they could spend on public outreach or the dissemination of socially-engaged research to the same problem: the funding system only placed value on a narrowly defined set of research questions that would be appealing to US and European journals that have higher world rankings in terms of 'Impact Factors' that Chilean journals. While FONDEYCT was set up to promote research that is in "the interests and needs of the country", therefore, it is seen as actually promoting a supranationalist science that derives its relevance from being *disinterested* in the needs of country: divorced from local concerns and orientated towards the creation of abstract theory that can circulate *Globally*—i.e., beyond Chile.

But trying to trace out who the 'they' were that enforced these judgments opened further problems. FONDEYCT funding decisions are the result of peer-review, and many of the archaeology professors at the UdeChile are or have been on the committee that reads and evaluates applications. So who FONDEYCT is as an agent, and why is it being seen as a means of imposing the values of US archaeology, becomes more difficult to untangle. Particularly given that some of the most vocal critics I encountered of FONDEYCT's conservative grant-awarding practices also sat on its grant-awarding peer-review committees.

To return specifically to the problem of the PhD, however, I was often told that a PhD was becoming more and more necessary for getting FONDEYCT grants. The application for funding has two parts: a CV of the principle investigator (PI) and a project description. The evaluation of the CV is based on points—a researcher can score a certain number of points based on his or her qualifications (40% of total score) and on publications (60% of total score). So the more points a

PI scores, the higher their chance of having their research funded. This point system was the cause of a great deal of strategizing and contention, and it was argued that it created difficulties for younger researchers. Only one person could be named PI on an application, so to get the most points for a research project it would always make sense to name the most senior collaborator as PI. But this creates a vicious cycle whereby one project member in an otherwise equal relationship would always be positioned as more senior, making it harder for the junior partner to ever get enough of their own PI-points. When I was told that getting a PhD helped with FONDECYT points, then, this was presented as part of the on-going strategizing process, because a PhD adds 10 points to your CV score. Getting a PhD was thus a way for young researchers to gain more FONDECYT points.

This argument was explained to me many, many times, whenever I asked about the sudden rush abroad for PhDs. But the ten points added to the CV for a PhD are the same number as a single article published in an international journal, having a book published with an international audience, or having previously won another large grant. It ought to make *more* sense to spend a few months writing an article, rather than 5-10 years getting a PhD, if FONDECYT points were the only reason. The emphasis placed on FONDECYT, then, is less of an answer to the question of why Chileans need PhDs, and more suggestive of how the PhD question is entangled with larger concerns about the extent to which the archaeological community is controlled by FONDECYT, and FONDECYT is associated with supranational, rather than Chilean, values/practices—even though the ‘peers’ on FONDECYT’s peer-review boards are other Chilean archaeologists.

Younger students' assumptions of inferiority

Younger student archaeologists volunteered the explanation that PhDs were necessary to secure FONDECYT grants, but were less likely to associate this with a critique of "Global Models" or US "cultural imperialism" than their older colleagues. Maite and Florencia, two third-year licenciatura students, intended to leave Chile to get either a Masters or a PhD. Both talked with pessimism about the Chilean education system, taking for granted the fact that it was less detailed and lower quality than undergraduate degrees in North America or Europe. Over coffee one afternoon, a few days before the 2010 election that saw the first conservative president elected since the return to democracy, Florencia discussed her plans for the future with a tone of despondency. If the right-wing candidate Piñera won, she swore she would just leave the country and go abroad, and heading to the UK to do a PhD would be a good excuse to do that. But beyond the immediate depression of the political moment, she had been looking for PhD courses in the UK for some time. Her reasoning was practical: she wanted to specialize in archaeobotony, something not yet common in Chile although there is an abundance of incredibly well preserved archaeobotanical material from the Tarapacá desert. She spoke fluent English so her options were either the US or the UK, and she insisted she could not bear to live in the US.

But behind her practical evaluations was an underlying assumption that anything she had already done in Chile would be worthless once she got to Britain. In truth she seemed not to be enjoying her studies at UdeChile, and was taking extra courses to try and finish a year early. She spoke somewhat wistfully of Maite's plan to take a break in her studies to backpack around South America for a year. Both Florencia and Maite reacted with skepticism to my description of what students in a US liberal arts BA or a British BA learn, in comparison to their own licenciatura.

They found it difficult to believe that they had taken comparable, or in my opinion already far more detailed and extensive courses, than European or US undergraduates. The desires of students like Florencia and Maite to leave behind the boredom of the familiar and travel abroad to apparently superior and more exciting places, can in part be seen as typical aspirations of the young and affluent. Indeed, the situation of many (but by no means all) of the UdeChile archaeology students, coming as they tend to do from upper and upper-middle class families and thus having highly advantageous economic and social resources, complicates a simple First and Third World comparison with their debt-riddled grad student counterparts in the US. For those unable to leave Chile because of obligations to care for family members or lack of money, however, the imperative to leave to get a PhD was contemplated with more anxiety. But whether they faced the prospect of emigration with pleasure or concern, young students automatically assumed that the Chilean system was inferior to anything offered abroad and that they therefore needed a PhD to make up for what was lacking in their substandard education.

Chileans getting PhDs in any discipline is a very recent phenomena. One middle-aged archaeologist described it as part of a "boom in the PhD market". When her father had traveled abroad for a PhD it had been considered remarkable. Yet in the last ten years it had become expected of her own generation and was no longer nearly as noteworthy. Beyond the personal costs of emigrating born by the individual, the new trend is expected to have a rapid and radical impact on the archaeological community in Chile writ large in terms of diminishing their ability to set their own standards of acceptable professionalism, as the previous chapter's discussion of the new Malla indicated.

The first Chilean archaeologist most people can remember leaving to get a PhD went to the UK in the early 2000s and one of his contemporaries left several years later for the US.

Another member of the same year group stayed in Santiago and was a student in a short-lived Masters program run by the UdeChile in the 2000s. Of the three, the one that stayed in Chile had an academic position in 2009 as a professor at the UdeChile, while his classmates were respectively still working on the US PhD and looking for a permanent position in Europe. I was told that the student who went to the UK caused a minor controversy at the time, because he left with only a licenciatura. Over gossipy beers one evening, an archaeologist from the same generation speculated on how a British university wouldn't have known the difference between the two Chilean degrees (implying that he had somehow tricked them) and there remained some question as to whether he would be allowed to work in Chile if he ever returned. Legally he was not an "archaeologist" because he didn't have a titulo. If someone wanted to make a fuss, they could argue that despite his British PhD he should go back and study for a titulo before he could conduct research in Chile. The slightly bitchy tone of this beer-fueled speculation did reveal an important problem, however. If a precedent was set, what was to stop other students from doing the same, now that getting a PhD was more common? Why would anyone do those extra 3-5 years of a titulo if they then had to go and do a PhD afterwards, and foreign universities could not tell the difference between a titulo, a licenciatura, and a bachelors in any case?

Indeed, Florencia was considering leaving for the UK before getting her titulo despite the reforms to the Malla that would make the titulo only two years long. She argued that the titulo is just a bureaucratic loop to jump through now, and it is only inertia that makes people keep taking it. She complained that most students don't have a chance to choose what they do their titulo research on anyway: the professors with active research projects give students various options for small practica and tesis projects, and the students just pick one of the available options. The result is that, as she described it, they end up doing something just because it's available, not because they really care about it. They pick the “least bad option”. While others might argue that this enables students to become integrated into on-going research projects that they can then be part of in the long-term, Florencia saw this as students being shoe-horned into doing research they have little interest in. Florencia’s reasoning is logical—a Chilean archaeologist taking a Masters or a PhD in North American or the US *would* be repeating much of the work they had already done during their titulo. But the significance of the *titulo professional* still remains and few leave Chile without it.

Having credentials that are recognized in the US

Asking younger students why they consider it necessary to get a PhD at all, they seemed surprised by the question, assuming that a Chilean education was naturally inferior to a US or European bachelors. Older archaeologists with more experience interacting with foreign colleagues were more circumspect, being more aware that the titulo was of a similar level to a foreign Masters or PhD (depending on the length and depth of the tesis, because there is also a great deal of variation between different students’ titulo projects). Benjamin, a Chilean archaeologist in the middle of a US PhD, talked of his experience with wry amusement. With many years experience running his own research projects in Chile, and having already written a thesis for his titulo that was longer than some US PhDs, he found it telling that he was taking required ‘Introduction to South American Prehistory’ classes alongside US cohort-mates who had never been to the field before.

Older archaeologists were thus more likely to frame the necessity of getting a PhD abroad in terms of the problem of being recognized by foreign colleagues. At least one conversation I held with a US archaeologist confirmed that Chileans are not being paranoid. The US archaeologist, himself a few months away from losing his own non-tenured teaching position, concluded a description of a disagreement with one of the UdeChile professors by dismissing the Chilean as “not really an archaeologist anyway, because you know he doesn’t even have a PhD”. That the non-PhD holder was recognized in Chile as the leading authority on his region, had an astounding publication record and decades of experience, was currently head of the largest archaeology department in Chile and recently director of the Sociedad—in short, at the time probably one of the well-respected archaeologists in the country—was nothing compared to his lack of a PhD. The US archaeologist’s comment might be nothing more than sour grapes, but it is revealing of something Chilean archaeologists themselves report: that there is little respect for their credentials and if the divide is to be crossed it must be through Chileans becoming more like North Americans, rather than a demand that North Americans become more familiar with Chile.

In essence Chileans were pointing to the extent to which the PhD is being positioned as a shared international standard, when in fact it is particular to US and/or European educational, professional, and academic cultures. The act of comparing Chileans unfavorably to a naturalized universal standard that is actually based elsewhere is what South Americans frame as implicit imperialism, critiquing the extent to which the North is seen as always inherently ‘Global’ in comparison to a perpetually localized South.

Normalization in the media

My sense, however, when talking to younger students in particular, was that the aura of the PhD as a higher quality credential had a more complex origin than interactions with actual foreign archaeologists, particularly as it is still comparatively rare for Chileans to collaborate with foreign teams other than with Argentinians. We have to look, then, at the wider context in Chile of normalizing PhDs as part of the education process. In doing so we can trace the involvement once again of the OECD, described previously as the "Trojan Horse of Neoliberalism". The OECD is a supranational organization that promotes neoliberal models of education and the value of knowledge through soft, "missionizing" methods that normalize the application of economic theory to policy problems (Amaral and Neave 2009: 94). The OECD is effective because it changes the parameters of debate around education, enculturates key civil servants and policy makes into both these values and the international technocratic networks that support them, and publishes ostentatiously value-neutral studies of an individual country's education system that measure each according to the OECD's own entirely neoliberal standard.

We can trace the pervasive normalization of PhDs in Chile in part to the impact of these reports in the national media. Newspaper articles reported on the crisis and problems of the Chilean university system as a matter of it lagging behind the rest of the world, quoting again the lack of professors and students taking PhDs as a major source of concern. These articles either directly or indirectly echoed an OECD report published in 2009 titled "Reviews of National Policies for Education Reviews of National Policies for Education: Tertiary Education in Chile 2009". The analysis of Chile is damning and pays particular attention to how "tertiary education does not meet the needs of the labour market", and the lack of "adequate qualification":

> The number of PhDs in the academic workforce in Chile is generally considered too low. National policies to improve the quality of academic staff have concentrated on upgrading this indicator, which has a close relationship to research and development. The goal is that by 2015 half of the full time faculty will have doctorates, which would require the number of PhDs graduating from

> national programmes to increase to 600 doctorates per year. The government also has the ambitious aim of sending some 3000 Chileans a year to pursue post-graduate studies abroad. (Organization for Economic Cooperation and Development 2009: 55)
>
> [Among a list of problems with the current education system] Overspecialisation of the curriculum: Most academic programmes lay strong emphasis on preparation for a specific field of study – what is known as "professionalisation" in other Latin American countries. … excessive professionalisation of academic programmes limits mobility between programmes and levels. The curriculum places heavy emphasis on a variety of professionally orientated subjects but does not include general education courses. Mostly absent also are ways for students to acquire competencies in teamwork, communications, intercultural awareness, and entrepreneurship, among other skills critical for the knowledge economy. (Organization for Economic Cooperation and Development 2009: 144)

This diagnosis of Chilean higher education's malady and cure has been picked up in the media. The nationally-circulating magazine *Qué Pasa* devotes an issue annually to ranking universities. The 2009 edition sheds light on how a PhD has become a normal (if frequently lacking) academic credentials. The front cover of the ranking issue advertises it with the slogans: "The 10 Best. The Labor Market—a thousand executives from all over the country evaluate 57 courses", "Indicators: Quality of students, Quality of teaching, Research, Accreditation" and "Careers: ranking by quality". Inside are pages devoted to both the overall ranking of all universities in the country, and detailed breakdowns based on individual courses, regions, and kinds of evaluation. The top ten universities get half a page each; the major stats that earned them their place accompanied in each case by a rather awkward photo of prominent (and mostly male) representatives of the university's administration wearing dark suits. The UdeChile and UCatolica vie for top place in the vast majority of the tables.

Through the stats for each profile, the idea that universities offer both "pregrado" and "postgrado" degrees is normalized, as is the insinuation that this is a significant component in the ranking of Chilean universities. *Qué Pasa* equates the licenciatura and the titulo as a “pregrado”, and a doctorate or masters as a “postgrado”, and lists the number of each offered in each university.[8] Similarly, one of the major contributions to the rankings is the “Quality of Teaching”, measured most prominently by the number of professors with a PhD. The UdeChile comes only 19th in this table, despite being listed either first or second in every other measurement. The magazine describes how the “thousand executives”[9] were overwhelmingly concerned about the quality of teaching. As a result, “Les indicatores musetran estadisticas objectivas que permiten conocer el perfeccionamiento de los academics a traves los doctorados y magister que poseen.” [“The indicators show objective statistics that allow us to know how the academics have improved, on the basis of the doctorates and masters they possess.”] In this article the assumption that a doctorate is needed is made explicit, by arguing that professors with PhDs will be doing better research (c.f. the same cause-and-effect argument in the OECD report quoted above). In the case of the UdeChile this contradicts the magazine’s own reports, as the UdeChile is consistently highest ranked for research despite having very few professors with postgrados.

Discussing this ranking with archaeologists at the UdeChile, some pointed out that *Qué Pasa* is an offshoot of one of the two major right-wing newspapers in the country and as such it was no wonder it put UCatolica as number one, or focused so heavily on traditional professions such as law, engineering, and medicine. Indeed *Qué Pasa* is often mentioned in discussions of the dictatorship as a magazine owned by one of the Chicago Boys and used as a means of

8 This I take to follow the British terminology, whereby a “graduate” is someone who holds a Bachelors degree, so a person studying for a MA or PhD is referred to as a “post-graduate”. In contrast the US terminology considers a “graduate” to be someone in the *process* of studying a MA or PhD. Whether following the British or US terminology, the supposition is that the licenciatura and titulo together are equivalent to a Bachelors.

9 According to the brightly colored text box the thousand executives from all over the country are all prominent business leaders, 84% working in the private sector, 62.7% in companies earning more than US$4 million, and 47% holding a postgraduate degree themselves. The presumed relevance of this information itself points to the market-orientation of the critique, and the way authority is being granted.

normalizing neoliberal social philosophy (Valdés 1995: 32). But as a major magazine sold on every street corner in Santiago and distributed throughout the country, its influence is undeniable.

It is worth remembering, however, that the reputation of the UdeChile remains unassailable, despite it lacking the credentials *Qué Pasa's* one thousand executives consider important. Beyond the pages of the magazine there are really only two universities of note in Chile: the UdeChile and UCatolica. Right-wing media is not unproblematically able to shape reality. But rankings such as these constantly present the idea of wider choice and an even playing field. Around the time high school students are choosing where to go to university, the subways, sidewalks, and newspapers of Santiago are plastered for months in advertisements for private universities, thus saturating the environment with the names, images, and offerings of a plethora of different institutions. The idea that an objective market in education exists, and that students choose the best product based on quantifiable criteria, is hard to shake off.

The arguments made by the OECD—that Chile needs more PhDs, a pregrado system that has weaker affiliations with professional organizations, and a higher-education system that is more geared towards entrepreneurialism and preparing individuals for the labor market—are clearly extensions of the neoliberal conceptualization of knowledge and education that, as I argued in previous chapters, Chilean archaeologists reject. The OECD's role is exactly this: to normalize neoliberal solutions to social questions. In the case of university fees, Chilean academics have something concrete to object to. In the case of normalizing the acquisition of a PhD and the idea that the licenciadora plus titulo system is inadequate, this occurs more insidiously through the pervasiveness of certain arguments in popular media forms, as well as through interactions with academics from abroad. It is more difficult for those constantly hearing of the necessity of going abroad to get a PhD, and being faced with generous funding from the state to do so, to pinpoint where this shift in expectations comes from. As such it is harder to argue against.

It can be attractive to hunt for a ghost in the machine: the real agent who is ultimately responsible. The OECD looks like a good candidate, but to lay the blame solely at their feet would ignore the very real motivations, frustrations, and reasoning of students like Maite and Florencia, or the critique of State complicity and colonial histories that are embedded in the discussions about FONEDYCT. Rather than resolving the puzzle then, we can take from this the sense that the Global Model is seen as inevitable and all encompassing. But also that there are strategies to ignore, subvert, or make use of the processes that accompany it: for instance by rejecting the NAFS, by creatively using research grants to set up socially engaged outreach programs, and by not severing the link between Colegios and universities and thus continuing to assert a non-neoliberal concept of professional authority.

Nationalism, imperialism, and supranationalism

When North American archaeologists call Chileans nationalist, they are drawing on an intensely critical discussion of nationalist archaeology that has occurred in globally circulating archaeology books and journals since the 1980s and is most frequently associated with comparisons to the uses of archaeology in fascist European contexts during the twentieth century. In some respects Chilean archaeology is indeed nationalist, but in a complicated manner.

Chilean archaeologists see the purpose of their work as being the generation of expert, scientific, and critically reflexive knowledge for society. Society is framed as *Chilean* society. Although they would like to have more interactions with researchers in other countries, they do not *need* to do so. Aware of their own autonomy in comparison to colleagues working in neighboring countries like Bolivia and Peru, they are only willing to engage in collaborations with foreigners that are mutually respectful and beneficial. Moreover, they associate an interest in not working within one's own country with a colonial mindset—a view also shared by the Bolivian archaeologists I interviewed. Although I have no means of confirming this other than through informal conversations I have had with archaeologists from other parts of the world, I suspect that this view is not unusual. Only in a few countries—the US, certain European countries such as the UK, and Japan—is it common for archaeologists to work abroad. I.e., only in a few countries is 'the field' as a conceptual space explicitly associated with foreign travel. It is not so unusual, therefore, that Chileans orientate themselves towards their own society, rather than towards an imagined international or supranational audience.

In discussing problems facing their discipline, the task of producing expert knowledge that has relevance for Chilean society is framed against pressure to do the opposite. To orientate one's work towards producing students who can be flexible workers; to frame knowledge as something students pay to acquire from professors who depend on student patronage; to reduce the relevance of archaeology to the rescue of artifacts; to measure success by the number of articles published in high ranking non-Chilean journals; and to replace their own standards of professional membership with a PhD. This pressure, as the discussion of PhDs and the North American Field School has demonstrated, is seen as coming from outside the discipline. The Global Model is understood to be that which is purportedly universal but actually originates in the US. That Chilean archaeologists are thus positioned on the side of society framed in terms of the nation, and in opposition to the State framed as a callous and cynical outsider in collusion with The Global, suggests a more complicated understanding of the use of the terms 'Imperialist' and ‘Nationalist’.

US archaeologists tend to be unsurprisingly uncomfortable with the idea that they are colonial or imperialist. The definition of the US as a colonial power demands some consideration in this respect, however, because of the entangled and often conflicting nature of *the US* as an entity, in these kinds of conversations. Is “the US” understood as a State and in terms of its foreign policy—the same foreign policy that supported the Pinochet dictatorship, and now supports and staffs organizations like the OECD? What relationship does *this* US have with the groups of US archaeologists who interact with Chilean and Bolivian archaeologists in spaces like the Congreso or Tiwanaku?

In Chile, at least, this collapsing of the US onto neoliberalism and both onto the Chilean State suggests additional complications that reveal the problem of national sovereignty in a time when trans-state organizations that have strong connections to the US do indeed have a neocolonial flavor. International financial institutions like the WTO, World Bank, IMF, and OECD are indeed predominantly involved in exporting US corporate values in a manner that is consciously or unconsciously part of neocolonial missionizing (Graeber 2010). Looking more broadly at the dissemination of neoliberalism through corporations, van Elteren (2009) argues that it is predominantly US citizens who are in positions of power in institutions like the WTO and IMF. In addition, a combination of the US business culture taught in business schools around the world and economic models taught in US universities that attract foreign students, the influence of US think tanks on media discourse, and speaking tours or other visits to foreign countries by so-called “business gurus”, has helped spread a very specific US corporate culture around the world that is indistinguishable from neoliberalism, with all its inherent contradictions and breakable rules (van Elteren 2009).

But again, what is the relationship between US archaeologists who work in places like Chile and *this* US/neoliberal agent? Although I have not had space to discuss the academic culture of US and Canadian archaeologists, I would argue that the imposition of neoliberal educational culture is as unwelcome to North American academics as it is to Chileans. The distinction is that in the US, the shift in the last thirty years has been less easy to identify: it has not come suddenly, accompanied by a violent dictatorship, announcing itself with manifestos, newspaper debates, and memorable names like 'The Chicago Boys' and 'Shock Therapy'. In a sense, the recent gradual shift in discourse in Chile (e.g., the normalization of the PhD) during the democratic period is closer to what has been happening in the US in the last thirty years, as expectations reflect a reorientation of the *terms of conversation* around education and the role of universities as sites of knowledge production—to the point that it becomes inconceivable to talk of a situation *other* than that where students pay fees, academics are considered to be less 'efficient' than those working in the private sector, and the first role of the university is to prepare young people for careers rather than to generate knowledge and critical reflection that is of use to society writ large. That this latter concept appears, frankly, to be nostalgic and perhaps naive in the US context shows how closed the conversation has become. Likewise it is interesting to note that since the 2008 depression in the US there has been increasing attention paid to fact that a college education is just, if not more, likely to result in crippling debt than it is in a job that requires a degree; but the response has not been a demand for free education. Instead there are campaigns for cheaper loans. It remains inconceivable that US high-school and undergraduate students would, like their Chilean contemporaries still occupying the streets in 2013, demand "Neither Scholarships Nor Loans! Education For Free!".[10]

10 This is a slogan from a banner I saw in a reports and photographs on the ongoing protests in Chile during 2013.

Thus US universities appear (comparatively) to have normalized the neoliberal model of knowledge as depoliticized and decontextualized, with paying students at the center.[11] On hearing about my research, it was common for South American academics who had worked in the US (and not just social scientists) to volunteer the observation that the US academy is *remarkably* depoliticized and disengaged from social debate in comparison to their home countries. Partly I would suggest this stems from Porter (1995) and Jasanoff's (2005) observations, that expertise in the US is trustworthy to the extent that it can present itself as entirely disinterested and depoliticized. But US social scientists, I would argue, have also internalized the idea that arguments in favor of supporting the social sciences are most successful when framed in terms of broadening the minds of undergraduates. Archaeologists have no serious expectation that their academic knowledge and professional expertise will be welcomed by society at large as a useful and valued contribution to social or policy debates on a *national* level. If the US is the perpetrator of a neocolonial mission to spread neoliberalism, one could argue that the US academy has been auto-colonized. In this sense the US is as much a local space that is unable to resist the Global

(http://www.biobiochile.cl/2013/06/13/marcha-estudiantil-en-temuco-congrega-a-cerca-de-1-500-estudiantes.shtml) Relating directly to the question of why this is inconceivable in the US, in June 2013 I found myself in conversation with a current Chicago Public School teacher about exactly this contrast. This was a few days after 50 public schools had been closed in city, despite enormous protests by the teachers unions (Joravsky 2013). She was interested in the idea of high-school students in Chile demanding better quality education, and I asked her what her students thought about their current situation, amid the ongoing decimation of Chicago's school system. She offered the insight that her students have no conceptualization of themselves as living in poverty, and thus no way of making the connection between the quality of their education and the socio-economic position within the broader society. They have TVs, they live in warm homes, they are not hungry: they just can't understand themselves as *poor*. At least, right up until the moment they have to apply for college, and suddenly they are confronted with unfathomable amounts of money. It can be something as simple as having to put down a $300 deposit for a college dorm room. There is no possible way that $300 can exist for them, and right in that moment the whole idea of going to college is gone. Sighing, she said that it was only at that moment they realized what they are up against.

11 Interestingly in the last two years since conducting this research the conversation appears to have shifted, to the extent that there has been an increasing number of relatively mainstream media articles discussing the neoliberalization of the US university, and suggesting alternatives. For example: Deresiewicz (2011), Junct Rebellion (2012), Schultz (2012), Thomas (2012), Barkawi (2013), n.a. 2013 ("An Open Letter to Professor Michael Sandel From the Philosophy Department at San Jose State U."), Blouw (2013), and Webster (2013).

Model as Chile is, but with the additional disadvantage that US academics are less able to identify the cause of their discontent (just as Chileans have trouble pinpointing the exact agent of the rush to get PhDs). Moreover they do not have the recent traditions of resistance or the popular support that Chilean academics and students are able to draw on.

On the other hand, however, we cannot ignore the disparity in power between academics from the Global North and Global South. It is indeed the case that US and European academics tend to see themselves as the norm, and expect the rest of the world to be following the same standards. Some (but not all) North American archaeologists who worked in Chile assumed as Ethan did that their Chilean colleagues were doing inferior work and had lower academic qualifications, because their research motivations and criteria for value were not the same and they lacked a PhD. My conversations with North American archaeologists working in both Bolivia and Chile reinforced the impression that it had simply not occurred to many of them that other countries might have different academic and educational cultures that arise out of different historical and cultural contexts. Erika Evasdottir has made the same observation in the case of Euro-American archaeologists in China. Asking why it is that they take little time to understand the local academic culture and thus judge Chinese research according to simplistic stereotypes and dismiss their research because of shortcomings in adhering to a US criteria of "scientific rigor", she comments that:

> Anglo-North Americans, with their frontier traditions of rugged resistance and antigovernmental libertarianism, are particularly prone to insisting that 'those' archaeologists 'over there' are nationalist communists with ulterior motives. … It is particularly odd given that Euro-American archaeologists pride themselves on their ability to reconstruct lifeways and meanings in the past. Euro-American archaeologists are painfully aware of the importance of context and combination in the creation of meaning—if not from their readings of Wittgenstein himself,

> then at least through his interpreters. In addition, Euro-American archaeologists have access to several ethnographies of academic disciplines since Bruno Latour and Steve Woolgar led the way with *Laboratory Life: The Social Construction of Scientific Facts.* Nevertheless, Euro-American archaeologists habitually assign motivations to behaviour without adequately grasping the different implications of that behavior given a widely distinct political, historical, economic and social context. (Evasdottir 2005: 8)

Which leaves us with no simple solution to the problem of comparison and the assumption of commensurability. Understanding that universities and scientific communities in both the Global North and Global South are subject to pressures to normalize neoliberal values (including those of universalism/globalism), does not mean that both are commensurable. The act of comparison is ultimately one that brings to the fore differences in power. Claims to disinterestedness can only be made by outsiders with no local stakes, no local affiliation: by foreigners who can claim to be from no-where. Chileans are able to see that the act of comparison will always be rigged because the scale is not their own.

Chapter Eight

Conclusions

The multiple epistemic cultures of archaeology

Following Knorr Cetina (1999), the question that has guided this is research has been: how does the historical and current social context of archaeology affect the production of archaeological knowledge? What institutions, practices, and objects constitute its culture of creating and warranting knowledge: its epistemic culture? Archaeology lacks many of the features classically associated with 'science'—replicable experiments that allow multiple witness to attest to the security of matters-of-fact; laboratories that enhance, purify, or model nature; complex, blackboxable technology that can standardize, mediate, or purify vision (see, e.g. Latour and Woolgar 1979, Shapin and Schaffer 1985, Daston and Galison 2007). These 'lacks' have historically led to fraught debates within the discipline about the security of its own knowledge claims and frequent calls over the years for archaeology to model its practices more closely on 'real' science (e.g., Droop 1915, Taylor 1948, Wheeler 1954: 53-63, Childe 1954: 169-70, Binford 1962, Watson, LeBlanc and Redman 1984, and for the present day see the recent debate about the American Anthropological Society dropping 'science' from its description [e.g. Wade 2010]), with authors often advocating the use of the philosophy of science as a kind of 'guidebook' for how to do so (Salmon 1982: 35, Dunnell 1982: 1).

The relatively limited success of such scientistic projects no doubt lies in the fact that, as social and historical studies of science have shown over the last few decades, there is a difference between the idealized image of how 'science' works and the reality of its practice: or as Harding (1991) put it so well, "'Physics' is a bad model for physics". Mechanical objectivity is not a productive strategy for warranting scientific knowledge (Daston and Galison 2007). This aside, however, I have shown in this dissertation that there *is* something that makes archaeology quite different from other laboratory and natural sciences—and even from other field sciences. Understanding the means through which archaeology's knowledge is created and warranted provides an example of a science that, despite these differences, has remained not just authoritative, but well respected by the public at a time of growing 'anti-intellectualism' and distrust of scientists, and among academic circles normally hostile to the social sciences (Holtorf 2006).

Taking, for instance, the replicability of knowledge claims, sociologists of science have shown that in practice the *replication* of results is actually not as important an issue as might be assumed (Collins 2001). But *reproducibility* is considered significant and, as is also the case in forensic science, is absent in archaeology because methodologies are not standardized and each field site is unique.

> Repeatability refers to repeating the same analysis on the same materials by the same researchers, in the same laboratory. Reproducibility refers to reproducing the original researcher's experimental or analytic setup and procedures by other researchers, with different materials, in another laboratory. Reproducibility is necessary to ensure the results are 'universal' or 'general' and not somehow explained by some peculiarity of the original researchers, materials, or laboratory. (Coles 2013: 39)

As I have shown in chapter two, archaeologists working in different countries, disciplinary communities, periods/regions, or even just on different sites have very different methodological practices and thus different means of creating their objects of knowledge. While ensuring reproducibility has been a problem for forensic scientists because it invites potential challenges to their knowledge claims in law courts (Coles 2013), archaeologists' knowledge claims are less commonly challenged by groups outside the discipline and thus non-reproducibility has not been a major concern (see Chapter three). In terms of the non-reproducability that comes from the uniqueness of field-sites, archaeological research takes place in field-sites that are unbounded (in the sense that they are not exclusive spaces), are always simultaneously other kinds of site (e.g., a corn field or a tourist site), and have the ability to stop being field-sites once the archaeologists' activities cease. Knowledge is created and warranted in these kinds of sites through the way in which the field is nevertheless created as a scientific lifeworld through the practice of *archaeologists*. In Chapter four I described how each individual field site becomes another instantiation of 'The Field'—a space that is defined and bounded through the archaeological practices that occur within it, and thus each individual instantiation becomes conceptually comparable with every other Field once archaeologists and their knowledge return 'home'.

In this dissertation, however, I have also argued that while the culture of the field is a key component of authorizing knowledge, the field's location within larger colonial processes means that Fields (and the knowledge produced within them) are not always equal. I have sought to understand why this is so by looking at structures of inequality, rather than the intentions or prejudices of individuals. Looking to the larger structure and practice of the discipline at a national and transnational level, the culture of education and day-to-day life in institutions shapes how knowledge is produced—both directly and through resistance. For example, as Chilean archaeologists both conform to and push back against neoliberal conceptualizations of knowledge in commercial *impacto* archaeology and in the university. Thus through the examples of Bolivian, North American, and Chilean archaeologists who work in Bolivia and Chile, I have traced the practices that produce and warrant knowledge in the field, in universities, and in conferences.

I have paid particular attention to the way members of these archaeological communities are made into experts—or, conversely, how their expertise is misunderstood, elided, or silenced (see Chapter three). I have argued that expertise is particularly important in archaeology because knowledge production relies almost entirely on people's bodies, rather than non-human technology. Whether it relies on a single archaeologist working relatively autonomously as an expert-tool (as, for instance, was the case for British archaeology discussed in chapter two and in Chilean archaeology); or on highly structured and hierarchical arrangements of many individual archaeologists and archaeological workers (as was the case in the Andean 'well oil machine' described in chapters two and three): archaeological knowledge is produced directly through the tactile and social expertise of human bodies.

However, while building the argument that there are different ways of practicing archaeology in each of the countries studied and thus that archaeology has multiple epistemic cultures—each with subtly different understandings of what their objects of knowledge are, how they can be known/created, what kind of person can know/create them, and how knowledge is warranted; I have simultaneously argued that this variation is blackboxed. There is an assumption of universalism—of transnational commensurability between practices, knowledge objects, means of warranting and means of recognizing expertise. Moreover, when commensurability is uncritically expected within transnational North-South collaborations, preexisting inequalities of power ensure that the knowledge production practices and products of Southern colleagues are devalued. When North American archaeologists assume that they share a single epistemic cultures with their Southern colleagues, they invariably assume that Chilean or Bolivian archaeology is, or ought to be, commensurable with the US; that there *is* a single accepted standard of measurement, but it happens to be the one that comes from the US. At the same time this standard is assumed to be disinterested and thus more rational and authoritative because it speaks from and to an imagined archaeological public that is over and above 'local' or national spheres. I have termed this *supranationalism*, particularly when it is understood in opposition to an insufficiently disinterested 'nationalist' archaeology.

To conclude this dissertation, I will briefly discuss some implications of this concept of supranationalism in relation to the now growing field of postcolonial science studies.

Science, social science, and the social studies of science from the South

Looking at the experience of Indian physicists in the early twentieth century, Deepanwita Dasgupta examines how scientists working in the 'periphery' experience "persistent asymmetry in intellectual authority" and must rely on the metropolitan centers to authorize their knowledge claims (2009: 156). But this, she argues, goes against a view that still dominates in the social studies of science—one that "maintains that an equality of intellectual authority is the norm in scientific communication among different research communities." (ibid). Celia Lowe (2004) makes the same argument through her discussion of primatology research in Indonesia, when she shows how Indonesian scientists are dependent on the foreign recognition of their knowledge claims. As a field science primatology shares many similarities with archaeology, and as such it is worth paying attention to Lowe's observation that, even when collaborations between individual

North American/European and Indonesian primateologists were respectful, the agenda of the discipline is defined by North Americans/Europeans and this asymmetry of power inevitably influences even the most well-intentioned collaborations in the field. Specifically, Indonesians were not able to define what counts as an appropriate research question nor an authoritative solution:

> The terms for what can be considered ‘good’ science are often set somewhere else, and Indonesians are frequently expected to contribute only data, rather than theory. Furthermore, Euro- Americans can still take for granted that Indonesia is a ‘problem space’ within an entire world available for their investigations, whereas Indonesian scholars tend to perceive the nation as their most pressing area of concern. (Lowe 2004: 502)

When there are differences of opinion between Indonesian and foreign scientists, these are likely to be understood as *cultural*, rather than *intellectual* differences.

> Whereas in a U.S. academic setting, difference among scholars is often understood as paradigm conflict, between Indonesian scientists and their foreign collaborators, theoretical disagreements could produce tensions over the neocolonial relations of scientific collaboration itself. ... In the context of transnational collaboration within the Togean project, North-South aspects of conflict were frequently emphasized over the abstraction of intellectual difference, and many tensions among foreign and Indonesian scientists were readily (and often mistakenly) viewed as ‘national’ difference. (Lowe 204: 501-2)

Similarly, looking to biomedical research trials, Patricia Kingori (2013) discusses how different framings of bioethics are explained as a conflict between ‘Western’ and ‘African cultures,’ rather than being the result of individual data collectors on the ground (whatever their nationality) reinterpreting the abstract ethical guidelines written by physically distant project directors. The experiences of the Bolivian and Chilean archaeologists described in this dissertation are therefore not unique. There is precedent for Northern scientists intentionally or unintentionally framing their Southern colleagues as less abstract, less scientific, and more bound to local, cultural, contexts.

Rather than see this as a matter of personal prejudice or misunderstanding, in this dissertation I have explored the broader machineries of knowledge production that make this a plausible or inevitable framing. Specifically, I have discussed this tendency in terms of *supranationalism*: an expectation that scientists from the North (and particularly the US) are able to transcend their national and cultural contexts and speak to a decontextualized, global, supranational public sphere more readily and naturally than scientists from the South. (Sandra Harding [2008: 3-4] refers to the belief that European and North American scientists are uniquely able to transcend their cultural as *exceptionalism*.) Additionally I have argued that, invariably, what is assumed to be supranational is in practice just as *local* as anywhere else, but local to the US/Northern Europe. For instance, tacitly unspoken expectations of how expertise is performed during an excavation have their origin in US culture and particularly US ideas of 'appropriate informality' (Chapters two-four); the Liberal Arts BA and PhD as forms of accreditation and socialization into a scientific community are rooted in a specific US university tradition (Chapters five-seven).

Looking beyond archaeology we can find other examples of non-European and non-US scientific communities being characterized as 'failures' or as somehow scientifically lacking because they depart from ('fail to live up to') a European or US model. In the examples Lowe, Kingori, and Dasgupta described, the 'failure' of projects to explicitly transplant US scientific institutions or practices abroad was invariably blamed on the local scientists' inability to transcend their own culture. In documenting attempts to export US models of science to India and

Iran through the establishment of Indian and Iranian outposts of MIT in the twentieth century, Leslie and Kargom (2006) also show that politics and culture were invariably seen by the US scientists as things that needed to be overcome. When the projects 'failed', US collaborators and sponsors blamed their foreign colleagues for being resistance to change:

> Politics, cultural traditions, and social patterns remained obstacles to be overcome, problems to be defined and solved. Lacking perspective on the political and social changes swirling around them, the Americans tended to see only "resistance to [technical] change," rather than alternative paths to technological and national development. (Leslie and Kargon 2006: 130)

Likewise Chris Shepherd (2005) documents how the failure of a Rockefeller Foundation project that aimed to improve agricultural science in Peru was blamed on the irritating "Latin" tendency to mix politics and science, something that ultimately disqualified Peruvians from doing 'real', rational science.

Thus at the core of the supranationalist perspective is an assumption that scientists from North America and Europe are more able to overcome their own culture and politics, and are thus untainted by 'nationalism'. That they occupy a more scientifically pure position—a claim which is, of course, illusionary, because knowledge and the practices that produce it can never be entirely context free (Harding 2008: 25).

I have argued that archaeology (as a science that is also a social science) has to a certain extent internalized this understanding that knowledge is not context or value free. At least so far as relationships with non-archaeological audiences, publics, and stakeholders are concerned, and particularly local indigenous groups, few would argue today that archaeological knowledge is entirely value free. However, as I have shown in this dissertation, this understanding has not extend as far as an engagement with inequalities that exist between archaeologists from different parts of the world working within different epistemic cultures.

In this respect archaeology is not necessarily unique. The same critique has been made for anthropology (Ribeiro and Escobar 2006) and for the social sciences more broadly (Connell 2007), suggesting that the reflexive turns of the past few decades and the attention paid by social theorists to globalization as *research topics* have not resulted in greater attention being paid to hegemonic relationships within academia itself. As these authors argue, if a solution were be found it might not involve simply encouraging or enabling colleagues in the South to become more entwined within Northern networks, conversations, and institutions. It should not just be a matter of allowing more people to be recognized as equally supranational. Increasingly, critics of the hegemonic influence of European and US social science have argued instead that even if a detachment from (national/political/cultural) contexts were possible, it would be a *problem* rather than a virtue. Particularly when seen from Latin America, US social scientists' disavowal of political contextualization of knowledge claims begins to look more like a disavowal of the *relevance* of their research—even more so as it appears that intellectuals on the Right lack such scruples about using academic research to advance their political agendas, as the case of Chile demonstrates so dramatically (Gustafson 2010, see also Narotzky 2006). While emphasizing that research still needs to be *scientific*, some of the Chilean and Bolivian social scientists I spoke with argued that the problem with the US academic work is that it is *too* depoliticized to be relevant: too much like a 'game' that privileged academics play for their own amusement or career advancement, with little serious expectation or consideration for how that knowledge can benefit political or social debates within broader society (c.f. Greenhouse 2011, Besteman and Gusterson 2005). By becoming too abstract and detached, social sciences can also become irrelevant.

More abstractly, there is an argument to be made that knowledge produced by Northern researchers without reference to the theories and experiences of Southern researchers will be flawed, and particularly will be flawed social theory. In *Southern Theory* Raewyn Connell (2007) argues that social theory produced in the North claims to be universal while ignoring both the experiences of the South and the historical and contemporary condition of colonization, and as a result Northern social theory produces an inherently incomplete and inaccurate view of the world. Connell first examines "how modern social science embeds the viewpoints, perspectives and problems of metropolitan society, while presenting itself as universal knowledge" (2007: vii), and then discusses alternative theorizations from parallel intellectual traditions in Africa, Iran, Latin America, and India. Connell's purpose is not just to draw attention to inequalities of opportunity and exposure, but to argue that theory that ignores Southern perspectives or experiences will always be inadequate because it does not fully account for the way colonization and postcolonization have shaped the contemporary world. Taking metropolitan societies as a baseline for universal human nature is a fundamentally flawed methodology for any social or humanistic science. (An argument that is echoed in Sheila Jasanoff's statement that biotech companies developing GM crops in the US for export elsewhere will inevitably encounter "unpleasant surprises" when they rely on simplistic assumptions of social and ecological homogeneity between the metropol and the periphery [2006: 289]). Universalism produces bad science: social or otherwise.

For Sandra Harding, who identifies the same problem within both the sciences and the social studies of science, the solution lies in paying greater attention to non-Western and feminist epistemologies because their different perspectives are more likely to generate 'objective' knowledge. Standpoint theory holds that “Dominant groups cannot understand the nature and causes of their own social situations if they examine such topics only from their own ‘native’ perspectives. It takes the standpoint of the oppressed and disempowered to reveal the objective natures and conditions of dominant groups.” (Harding 2008: 14). This is because disempowered groups need to acquire knowledge of the dominant culture as well as their own subordinate culture, while members of the dominant culture have no *need* to be aware of or knowledgeable about subordinate cultures and are therefore more likely to have a partial perspective.[1] This is something that echoes Ribeiro and Escobar’s definition of *provincial cosmopolitanism* (“the often exhaustive knowledge that people in nonhegemonic sites have of the production of hegemonic centers”) and its opposite: *metropolitan provincialism* (“the ignorance that anthropologists in hegemonic centers have of the knowledge production of practitioners in nonhegemonic sites”) (2006: 13). Those *outside* the hegemonic centers of science/social science have a less partial image of the world than those inside.

Having presented these perspectives, however, I am not entirely certain that I wish to go as far as the conclusion that this debate has implied; namely, arguing that those outside of the hegemonic center will always have a less partial and more complete perspective than those inside. Partly because, as I have suggested at critical points within this dissertation and hope to explore more fully in the future, I do not take ‘the US’ to be a stable or simple category when considering the practice of archaeology or science in general. *Who* or *what* exactly, is ‘the US’ in the context of a collaboration between a group of archaeologists, like the ones I have been

1 Harding is not arguing that the perspective of the disempowered will be perfectly complete, however, because this is an impossible dream of positivism. Rather, “the success of standpoint research requires only a degree of freedom from the dominant understanding, not complete freedom from it.” (2008: 120). Moreover, it is by necessity a group achievement rather than an individual one: only collectively can a fully image of the world be built from many different perspectives.

describing? As the discussion of Chileans' slippage between the Global Model, the US, and Neoliberalism demonstrated in Chapter seven, the agent of (neo)colonial oppression is no easy thing to pin down. In my experience, the average Chilean or Bolivian's image of the US was by no means more accurate, or less reliant on stereotypes, than that the average North American's image of Chile or Bolivia. While South American archaeologists undoubtedly had *significantly* greater knowledge of US and European archaeological research and theory than their North American colleagues had of archaeological work in the South, South Americans did not necessarily have a more privileged understanding of the realities of daily life, culture, or politics in the US. This became relevant when reflecting on comparable practices (for instance, the Chilean students' inaccurate comparisons of their own educational system and that of elsewhere): practices that, as I have argued, are central to structuring the specific epistemic culture of *both* US and Chilean or Bolivian archaeology.

Particularly when we look more closely at the intersection of different forms of privilege and the multi-layered histories of colonization in the Americas, a more complex picture arises.[2] How do we explain or evaluate the relative advantages of white, upper-class, Chileans as they attempt to build an academic career on the basis of a Chilean *titular professional*—individuals whose private schooling and language tuition, then cheap state university fees, have been paid for by wealthy professional parents—in comparison to a US PhD holder from a lower-middle class family, the first-generation to go to college, who will then take a non-tenured contingent faculty job that will barely make a dent in the $100,000 of college and graduate school debt they owe?

2 To take just one example, consider the experiences of African American writers as they confront their image as "Americans" abroad. Maya Angelou's (1976) description of being an African American in Italy just after the Second World War, and Ta-Nehisi Coates' 2013 series of essays in *The Atlantic* on being an African American in Paris, are two particularly eloquent examples that provoke us, at the very least, to question simple notions of what 'The US' means when seen in inter- or transnational contexts (e.g, Ta-Nehisi Coates,"English Is a Dialect With an Army" *The Atlantic* 2 Aug 2013.).

And what of the Bolivian student who travels to Chile to get a degree and finds herself constituted as an exotic/fearful Other? Or the Chilean archaeologist living in the far north who resents the hegemonic influence of Santiago? There are multiple layers of colonial histories here, as well as intersecting categories of gender, queerness, race, ethnicity, class, and economics that I have not had space to explore fully in this dissertation, but which are all relevant.

I hope to develop this argument further in the future and in the process use the experiences of these archaeologists to open out the question of what 'The US' is, when seen from South America: in effect, to localize that which is unproblematically assumed to be a stable category. In part this will involve developing an argument I began to discuss at the end of Chapter seven: namely, to what extent has the US been 'auto-colonized' through the same processes that, having been applied elsewhere, are understood as 'Americanization'. For example, US universities have been neoliberalized along remarkably similar lines to those in Chile, with if anything more dramatic results and less conscious resistance. If in Chile this is interpreted as a process related to 'US Imperialism', what does it mean if the same thing happens in the US?

In this dissertation I have focused on the pervasive structures of inequality within and between the North and the South: the organization of institutions such as universities, the influence of organizations like the OECD, the reality of economic hardship for both individuals and disciplinary communities, language use, and so on. Although the situation on the ground is too complex to allow a simple critique that splits the world into Northern oppressors who ignore the realities of the world around them, and Southern victims who have a privileged access to truth, to ignore structural inequality—whether of gender, race, colonialism, or class—is in itself a form of violence.

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