

## RESEARCH ARTICLES

## Indigenous Archaeological Field Technicians at Tiwanaku, Bolivia: A Hybrid Form of Scientific Labor

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**ABSTRACT** Archaeology is a science with an intimate investment in the bodies that labor to produce its objects of knowledge. Data comes into being through tactile skills: eyes that see, hands that touch, voices that name and debate. It matters, therefore, who constitutes and controls the labor force; yet little has been written about archaeological workers. Here I outline the relationship between archaeologists and indigenous workers at Tiwanaku, Bolivia, showing that archaeologists did not have direct control over labor on their sites, including who was hired, how much they were paid, and how jobs were defined. These decisions are made by the community's *Mallkus* in active (sometimes protracted) negotiation with the archaeologists. While active and constant, the process of bargaining was not necessarily conflicted; moreover, it led to a form of labor organization and scientific practice that was neither entirely "Aymara/indigenous" nor entirely "archaeological/scientific." It thus forms an intriguing example of a form of hybrid scientific practice that incorporates two very different conceptualizations of labor, both as it relates to specific individuals (who is capable of occupying specific jobs) and how it is valued (what the underlying purpose of scientific work should be). [*postcolonial science studies, Andeanist archaeology, anthropology of work, technicians, field sciences*]

**RESUMEN** La arqueología es una ciencia con una inversión íntima en los cuerpos que trabajan para producir sus objetos de conocimiento. Los datos surgen a través de las destrezas táctiles: ojos que ven, manos que tocan, voces que nombran y debaten. Importa, por lo tanto, quién constituye y controla la fuerza de trabajo; sin embargo, poco ha sido escrito acerca de los trabajadores arqueológicos. Aquí, bosquejo la relación entre arqueólogos y trabajadores indígenas en Tiwanaku, Bolivia, mostrando que los arqueólogos no tenían control directo sobre el trabajo en sus sitios, incluyendo quién era contratado, cuánto se les pagaba y cuántos trabajos se definieron. Estas decisiones son hechas por los *Mallkus* de la comunidad en activa (algunas veces prolongada) negociación con los arqueólogos. Mientras activo y constante, el proceso de negociación no necesariamente fue conflictivo; además, llevó a una forma de organización del trabajo y práctica científica que no fue ni enteramente "aimara/indígena" ni enteramente "arqueológica/científica". De este modo forma un interesante ejemplo de una forma práctica científica híbrida que incorpora dos muy diferentes conceptualizaciones de trabajo, ambas en la manera que se relaciona a individuos específicos (quién es capaz de ocupar trabajos específicos) y cómo es valorado (cuál debe ser el subyacente propósito del trabajo científico). [*estudios científicos postcoloniales, arqueología andeanista, antropología del trabajo, técnicos, ciencias de campo*]

It's late morning on a large archaeological excavation near the monumental site of Tiwanaku, Bolivia. As work picks back up after the morning break, the soft murmur of conversation hangs in the air. Six of the project's eight excavation teams are working in this part of the site: six archaeologists and 30 men and women from the nearby Aymara community of Wancollo, spread out evenly across two large intersecting squares cut into the ground. Each five-person team is headed by a *maestro*, a skilled technician from Wancollo who carries out much of the careful excavation work under the direction of an archaeologist. The excavation is in a field set back from the road to town and outside the perimeter fence of the monument, so despite the large number of people gathered here, on this still Monday morning nothing much can be heard beyond the low hum of voices and the scrape of metal tools against the dry earth.

Suddenly Don Ronaldo, the most senior *maestro* and also Wancollo's shaman, straightens his back and casts a swift eye about him.<sup>1</sup> He grins, cups his hand to his mouth, and calls out a loud greeting: "Waliki maestros!"<sup>2</sup> The other *maestros* call back a reply from their stations across the unit: "Waliki!"

Don Ronaldo remains in place for a moment, leaning on his shovel. He watches the nearest two *maestros*, Juan and Hector, until they glance back up at him and Juan says something in Aymara that causes all three men to laugh. Hector turns back to the archaeological pit-feature that has been absorbing his attention for the last half hour. He and his assistant are kneeling next to a small round hole in the ground, pushing and pulling the darker earth at the feature's sides with trowels. The assistant scoops the loose soil gently into a bucket. The third person in the team is an elderly, slightly stooped man who bends to watch Hector's careful movements.

Also watching is Rob, a young North American archaeologist, who stands, taps his trowel against his hand, and frowns as he waits to see if Hector will find the feature's edge. When the bucket is finally full, Rob steps aside to let the old man carry it to two teenage girls sitting on the far side of the unit. In their straw hats and black plastic chokers, with soft brown wool shawls pulled tight over their shoulders, these last two members of Hector's team look decidedly bored. The girls get reluctantly to their feet and take the bucket from the old man. As he turns over a numbered card to add this bucket to the day's tally, the girls shake the earth through a large metal screen and begin the processes of picking out artifacts.

A little way off, three Bolivian archaeologists are sitting in a circle on the ground, filling in site journals and a dense wad of forms. While the murmur of Aymara hangs in the dust over the excavation, nothing can be heard from the archaeologists but the rustle and scratch of pencils and paper.

Archaeology is a science with an intimate investment in the bodies that labor to produce its objects of knowledge. Very few machines, technologies, or abstracting devices are used in archaeological excavation (cf. Daston and Galison

2007; Latour and Woolgar 1979; Shapin and Schaffer 1985). Instead, data comes into being through the tacit knowledge and tactile skill of individual bodies: eyes that see, hands that touch, voices that name and debate (Leighton 2015). The archaeological labor force is the primary instrument for producing archaeological data. It matters, therefore, who constitutes and controls labor on site; yet very little academic attention has been paid so far to the manual labor of archaeological workers. Even less work has focused on the labor of indigenous people who, in places like Tiwanaku, have been actively involved in the work of excavation for generations and who outnumber archaeologists on site five to one.

Workers are not ubiquitous on all archaeological projects. Between 2008 and 2011, I conducted a multisited ethnography of Bolivian, Chilean, US, and Canadian archaeologists working in Bolivia, Chile, and Peru. In Chile, excavations were smaller and carried out entirely by Chilean professional archaeologists or student archaeologists. In contrast, in Bolivia large numbers of workers and a small handful of archaeologists excavate on a much larger scale. Those workers include men like Don Ronaldo and Hector, who have in some cases decades of experience and often greater subtlety and skill at excavating than the graduate student archaeologists. But it also includes temporary workers like the young women, whose bored eyes and hands, skimming over the surface of the artifact screen, belied the apparent rigor of this sampling strategy. The difference, I will argue, between employing workers or not is more than a simple matter of moving more earth in a shorter amount of time. In certain situations, the labor of nonarchaeologists has the potential to create new forms of scientific practice: hybrid forms that structure both the physical and scientific work of the excavation itself.

In this article, I outline the processes of negotiating and bargaining that can go into creating an archaeological labor force in a very particular place: Tiwanaku, Bolivia, and specifically a North American project that worked in 2008 with the Tiwanakeño community of Wancollo. At Tiwanaku, archaeologists do not have direct and complete control over the workforce on their sites, including who is hired, how much they are paid, and the kind of work they will do. Instead these decisions are made by the community with whom they work, in active (and sometimes protracted) negotiation with the archaeologists. I will argue that in 2008 Wancollo had a considerable amount of control over the terms of its own labor and that this control led, through an active and constant process of negotiation with the archaeologists, to a form of labor organization and scientific practice that was both distinctly Aymara (or more specifically, distinct to this part of the Bolivian altiplano) and also distinctly archaeological. It thus forms an intriguing example of a kind of hybrid scientific practice that incorporates two quite different conceptualizations of labor, both as it relates to specific individuals (i.e., how to evaluate who is capable of occupying specific jobs) and how it is valued (i.e., what the underlying purpose of labor on an archaeological excavation is).

### THE INVISIBILITY OF WORKERS IN THE ARCHAEOLOGICAL LITERATURE

Given how common it is for archaeologists to employ local people to work on archaeological sites, it is remarkable how little has been written about such labor. Gavin Lucas (2001:8) pointed out that archaeologists' field manuals in the first half of the 20th century used to include instructions on how to hire, manage, and pay local workers. Such explicit references to economic matters have disappeared in more recent decades and not just because (as Lucas argues for the UK) excavation is now done by archaeologists with university degrees rather than untrained "laborers." A small number of contemporary ethnographies of British archaeologists pay explicit attention to the question of employment, notably the work of Paul Everill (2009) and Matthew Edgeworth (2003), but given the very different organization of archaeology as a profession in the UK, such examples do not help us understand situations elsewhere in the world (Leighton 2015). Beyond the UK, there have been very few detailed ethnographic studies of archaeological workers, with the notable exception of Doris Maldonado's (2011) compelling study of archaeological workers in Honduras.

The relationship between indigenous people and archaeologists has been extensively explored in the literature on community archaeology, postcolonial archaeology, indigenous archaeology, public archaeology, and participatory archaeology (e.g., Ardren 2004; Ayala 2007; Castañeda 2009; Derry and Malloy 2003; Hamilakis 2011; Kojan 2008; Kraemer 2008). The argument put forward is that archaeologists should engage with indigenous people's interpretations of the past, their historiographies, and their ontologies of the material/spiritual world. Such work explicitly recommends including indigenous people in archaeological practice, but inclusion is implicitly or explicitly understood to mean not employing people as workers; rather, inclusion or collaboration should be at the level of the texts and narratives archaeologists produce. This position is explicitly stated in one of the few academic discussions of archaeological workers at Tiwanaku, an article written by three Bolivian archaeologists from the Universidad de San Andrés and an indigenous Bolivian scholar (Copa Mamani et al. 2012). The authors argue that workers at Tiwanaku are silenced because they are employed as manual workers without being given the opportunity to share their own interpretations of the site. By not incorporating indigenous narratives of the past into their archaeological interpretations, the authors argue, archaeologists are engaged in acts of colonial domination that silence indigenous people.

The implication of this focus on narratives and interpretations of the past is that there is something inherently demeaning and exploitative about indigenous people being employed as manual workers on archaeological sites. Scholars in postcolonial science studies have considered collaborations between indigenous people and scientists from various disciplines, including those that involve manual or

technical work, and critiqued the form these collaborations have taken. Examples include situations where scientists make use of the knowledge and expertise of nonscientists or attempt to, without necessarily involving them directly in their day-to-day research practices (e.g., Helen Verran's [2002] study of workshops on fire burning held by indigenous landowners for environmental scientists in Australia). Other scenarios involve situations in which local people became both research subjects and research assistants (e.g., Warwick Anderson's [2008] history of the medical investigation of a brain disease among the Foro people of New Guinea, who were doubly valued for their ability to be transformed into data: first as carriers of unrealized "local knowledge" and second as bodies that could be transformed into samples). The latter cases were often decidedly unequal, if not outright exploitative; in the former cases, collaboration was short lived—never able to transcend the epistemic divide sufficiently to involve day-to-day working together. In other examples in which indigenous people are directly involved in day-to-day practice as laborers (e.g., Soto Laveaga's [2009] example of the indigenous Mexicans who sought out and harvested the yams that were required to produce the synthetic hormones in contraceptive pills), local people were intentionally exploited as a labor force, and had no ability to control or negotiate the terms of their labor. Based on my observations at Tiwanaku, I suggest that another scenario is possible, but it requires a closer examination of the assumption that all indigenous people would prefer to be involved in the interpretative work of archaeology rather than the manual work of excavating a site—or, indeed, that manual work is inherently demeaning or unskilled.

Before I go into the details of the case study, however, it is important to reiterate that this is indeed only a single case study, and given the lack of research on archaeological workers around the world, it is difficult to tell if it is unique. The details of specific case studies matter if we wish to move beyond universalizing statements about "archaeologists and indigenous people" that have a tendency to conflate all communities in every part of the world into one homogeneous, perpetually victimized, and thus entirely passive group (Benavides 2004:173; see also Weismantel 2006). What might be exploitative in terms of archaeological practice in one place may be welcomed elsewhere, and vice versa. To illustrate this, I use the example of Tiwanaku deliberately; I do not hold it up as an example that can stand for all archaeological work, in Bolivia or elsewhere. As I hope to demonstrate, Tiwanaku is a striking case study precisely because of the level of experience and control local people have over archaeological resources and the terms of their own employment. The people hired to work on the Honduran project described by Maldonado (2011:15–16), for instance, were new to archaeology: specifically, they were as new to excavation as she was herself, as a PhD student with little field experience. In contrast, workers at Tiwanaku frequently have a great deal of experience drawn from long careers working with a range of Bolivian and foreign

archaeological projects and are in a position to vigorously negotiate the definition of their own labor.

### WHO CONTROLS ARCHAEOLOGY AT TIWANAKU?

During 2008 I followed the last field season of a three-year excavation project at Tiwanaku, as part of a larger multisited ethnography exploring transnational collaborations between field scientists from the Global North and South (Leighton 2014). Bolivia is one of the most ethnically diverse countries in South America, with 40.7 percent of the population identifying as indigenous in the 2010 census. After decades enduring crippling neoliberal policies imposed by the United States, WTO, and IMF (Shultz and Crane-Draper 2008; Webber 2010; Zunes 2001), in 2005 Bolivian voters elected the first indigenous president in the Americas—the former coca farmer and union organizer Evo Morales—with an explicitly antineoliberal and proindigenous agenda and policies promoting decentralization and “plurinationalism” (Kohl 2010; Postero 2010).

Located in the altiplano region of Bolivia, Tiwanaku (also known as Tiahuanaco or Tiahuanacu) is the name given to a prehistoric monument, the contemporary small town that sits beside it, and the surrounding municipality that includes 23 independent indigenous communities.<sup>3</sup> The archaeological site is the most intensively studied in Bolivia. Foreign and Bolivian archaeologists have worked in and around the monument of Tiwanaku since the end of the 19th century (Kojan and Angelo 2005:385–386; Yates 2010:33), and their research has been regulated to varying degrees since 1909 (Friedman 2008:4; Rhebergen 2012:44). Various North American projects have worked in Tiwanaku since Alan Kolata’s excavations began in 1979.

Much has been written on Tiwanaku’s ideological uses and abuses, making the history of how Tiwanaku has been interpreted over the last 150 years a history of Bolivian nationalism and indigenismo (e.g., Arnold and Yapita 2013; Kojan 2008; Sammells 2012a, 2012b; Yates 2010). Yet apart from occasional references to how archaeologists viewed local people “as little more than a cheap labor force” (Paz Soria, quoted in Roddick 2004), scant attention has been paid to the fact that archaeologists have always been dependent on the people living in Tiwanaku to conduct their research.

The proindigenous and particularly pro-Aymara agenda in Bolivian politics since 2005 has given Tiwanaku an additional layer of significance, starting with Morales holding his inauguration ceremony there in January 2006 (Canessa 2012a; Postero 2010). Funds are now being made available that allow Bolivian archaeologists to direct excavations on a scale previously only accessible to foreigners; furthermore, as part of the decentralization process, permission to grant archaeologists permits has formally shifted into the hands of communities (Yates 2011). In practice, however, communities in Tiwanaku were directly involved in granting access to archaeological sites since at least the 1970s. Working on community land always required local permission, although this power formally lay with the Asociación de Trabajadores

en Arqueología de Tiwanaku (ASTAT [Tiwanaku Association of Archaeological Workers]) and the town’s mayor. The ASTAT, formed around the time of Kolata’s 1970s excavations, was structured similarly to other Bolivian worker-syndicates. It included members of all 23 communities, managed relations with archaeologists, and organized the equal distribution of archaeological employment across all communities. (In 2006 one community, Wancollo, broke from the ASTAT to work independently on the excavation described in this article, as I discuss below.)

Thus, while recent legal changes had made decentralization more explicit, this shift began before Morales. Most dramatically, in August 2000 the community of Tiwanaku wrested control of the monument and museum from the national office of archaeology during an “intervention” (Sammells 2013). The museum and site continue to be locally managed today—and, crucially, revenue from ticket sales now remains in Tiwanaku. Christine Hastorf (2006) reported that other communities in the region with archaeological projects were increasingly seeing Tiwanaku as a regional power center and had demanded the return of artifacts found on their land that had been sent to Tiwanaku’s museum for safekeeping. Ever more rural communities were thus seeing Tiwanaku as a model for chasing elusive tourist dollars through archaeology.

As this background suggests, Tiwanaku’s indigenous communities have been central to the practice of archaeology for a considerable period of time. Substantial numbers of men and women have worked on excavations for generations. Far from being passive bystanders, they see themselves—and are increasingly seen by others—as actively controlling archaeological work and the tourism that results.<sup>4</sup> The level of their involvement in controlling archaeological work becomes even more apparent when we look in detail at how labor was organized on a typical excavation.

### LABOR ROTATION AND THE DEFINITION OF EXPERTISE

The two underlying principles of the labor system on archaeological projects in and around Tiwanaku are that decisions over labor are in the hands of community leaders rather than archaeologists and that employment is evenly distributed through a system of rotating jobs each week (see also Hastorf 2006). The excavation I studied in 2008 involved roughly fifteen archaeologists and, each week, between sixty to eighty people from the community of Wancollo. Wancollo is divided into four *zonas*, or sections, each of which has a leader called a *Mallku*. Andean leadership positions are organized through a cargo system, meaning they are held for a year before the position rotates to the (male) head of a different landowning family (Andolina et al. 2005; Sammells 2013:319; Strobele-Gregor 1996). The Mallkus were responsible for generating weekly archaeological work rosters and ensuring that positions were divided equally among the *zonas*.

The majority of jobs were *peon* positions: roles requiring no specific training and rotated after a week, such as the bucket carrier and the artifact screeners. There were also a limited number of permanent and skilled roles, known as *maestro* positions, that were held by the same individual for the entire season and earned higher wages. The excavation *maestros* (who were always men) managed their teams of *peons* and undertook the skilled excavation work.<sup>5</sup>

This system of labor rotation and two-tiered wages controlled by the *Mallkus* had been in place for decades, not just at Tiwanaku but on all Bolivian and North American projects in the vicinity. Despite being a well-established system, however, each year there was a great deal of negotiation over the definition of permanent/rotating and skilled/unskilled jobs.

About two weeks into the 2008 field season the codirectors, Olivia and Emily, were asked to attend a lunchtime meeting with Wancollo's *Mallkus*. The directors expected a discussion of their sponsorship of the upcoming San Juan festival, for which they were providing beer and dance costumes. Instead the problem was one of the *maestro* positions: the *Mallkus* wanted to replace the lab *maestro*, Don Arturo, with a senior Wancollo official. All of Wancollo's *Mallkus* and officials had nonrotating jobs that year, although many of their jobs were *peon* positions that usually circulated. This particular official lacked a position, so the *Mallkus* requested that he be given Don Arturo's job instead.

As lab *maestro* Don Arturo was responsible for overseeing the artifact lab and processing archaeobotanical samples: a painstaking job that involved identifying charred pre-Hispanic botanical material in soil samples. Don Arturo had been personally trained to do this many years earlier by the archaeoethnobotanist Christine Hastorf. By lucky coincidence, he was a member of Wancollo and thus was available despite the community's break from the Tiwanaku Archaeological Workers Association (see above). If Don Arturo had not been available, the directors would likely have had to hire an archaeology student to do the work instead. In other words, this job required specialist training and experience, and it was only by coincidence that a nonarchaeologist was available.

When the *Mallkus* requested Don Arturo's job be given to the unemployed official, Olivia and Emily insisted that this could not be done—Don Arturo had to occupy that specific position because he was the only person trained to do it. After some negotiation, they suggested the official instead be given a *peon* position marking numbers on ceramic sherds but paid the *maestro* rate and not rotated. To the directors' relief, this alternative was accepted.

Although this incident caused some initial panic, requests to swap roles or create additional positions were common. This case was resolved comparatively quickly because the directors were willing to invent an extra *peon* position with a higher wage, if it meant keeping Don Arturo in his more skilled position. The cost of an extra *maestro*'s wage, Olivia explained, was nothing compared to losing

the only person capable of processing archaeobotanical samples.

While it was resolved quickly, this example points to fundamental differences in how the *Mallkus* and the archaeologists conceptualized archaeological work and specifically what made someone capable of undertaking particular forms of labor. From the *Mallkus*' perspective, it was highly problematic that one official had been left without a well-paid, nonrotating position. The official's claim to the lab *maestro* job stemmed from his position within Wancollo—remembering, of course, that political office is only held for a year before it rotates to a different family and that such positions are generally considered more a burden than a privilege (Ströbele-Gregor 1996). For the archaeologists, however, the only important factor in considering who should occupy this role was whether that specific individual had the necessary scientific expertise.

The situation seemed resolved, but the following week the directors were annoyed to discover that the official had not turned up for work—instead, he had been sending his wife. On the basis of past experience, the directors complained that if he sends his wife to do his job today, he was likely to send one of his children tomorrow. Emily was particularly upset, however, because the original proposal had been that this man—who hadn't even turned up for his first day of work—should replace Don Arturo, someone she considered to be skilled and experienced and a reliable and long-standing member of the project.

The archaeologists' annoyance stemmed from a sense that the *Mallkus* were not being fair either to them or to Don Arturo. Giving jobs to officials based on rank amounted to patronage and thus corrupted the principle of rotating employment to equitably share wages throughout the community. The rotation system was, after all, disruptive to the archaeologists' work—each week new people had to be trained, and it was necessary to devise methodologies that counteracted the lack of continuity and expertise among the *peons* (Leighton 2015). From the archaeologists' perspective, the labor system was morally, rather than practically, desirable: something they approved of because it felt like the ethical thing to do—but only when it appeared (to them) to actually be ethical. For the *Mallkus*, however, overlooking one official when distributing higher-earning positions was inappropriate.

In other cases, compromise could be reached more easily. For instance, in 2008 Wancollo decided that individuals who already held other jobs would not be employed on the archaeological project, but this meant all the men trained to work the archaeobotanical flotation machine in previous years would be excluded because they held other jobs that year. After negotiation, the *Mallkus* allowed one of them to work so he could oversee the new trainees. This process of negotiating and bargaining continued throughout each field season. It was not necessarily onerous enough that we should characterize it as "conflict," but there was certainly the sense among the archaeologists that engaging in such negotiations

was a significant part of the directors' day-to-day job when in the field.

Despite their respect for the principle of the rotation—a system they understood as being “traditional” and reflecting indigenous Andean principles of communal labor reciprocity—the archaeologists primarily understood people's ability to conduct particular types of labor as stemming from their individual expertise rather than from their kinship position within a family, a *zona*, and a community. The directors described, for instance, how they were always on the lookout for “good people” who they would request be re-employed in skilled positions in subsequent years. In addition, they tried on multiple occasions to open maestro positions to women. But their requests were only rarely taken into account, illustrating how members of Wancollo evaluated labor differently. On multiple occasions, the directors had asked that a particular maestro who was habitually absent not be reemployed, that adult men with physically demanding peon positions stop sending their young children or elderly relatives to work in their place, and that jobs requiring writing be given to people who were literate. Many of these requests were politely ignored if they contradicted the Mallkus' own evaluation of the most appropriate distribution of employment. As the case of Don Arturo illustrated, it was difficult for the directors to make a persuasive case for employing a specific individual on the basis of personal expertise, even when, from their perspective, there was only one individual in the entire altiplano qualified to occupy that role.

It might be tempting to read about this disagreement and want to take sides, focusing in on the question of whether or not we think particular individuals were right to be frustrated or demanding. What I find more remarkable is the fact that, despite such different criteria for evaluating who should have which job and the significant amount of negotiation and bargaining involved to keep everything together, the process did work remarkably well. Equally, this incident could be read as a story of the stubbornness and patronage of the Mallkus or the cultural insensitivity and inflexibility of the archaeologists. But again, I would argue that the more significant point is what the reactions of each group reveals about how, together and separately, they came to an understanding of the purpose of their collective work and thus the value of an archaeological excavation.

### THE PURPOSE OF ARCHAEOLOGICAL LABOR AND EXCAVATIONS

Broadly speaking, in Aymara agricultural communities the family, rather than the individual, is the basic economic unit (Canessa 2012b; Swartley 2002). Thus, sending one's wife or child to work on the excavation, rather than the household's primary male adult, was not necessarily considered problematic—peon positions were rotated by household rather than individual, so in effect it was the household that was being employed. Negotiations within Wancollo over who should be employed included appeals to consider the total number of individuals in a family who had already held

positions. For instance, there were complaints that one official had allowed one of his many adult daughters to be employed each week. For the Mallkus, a particular individual's suitability for a job was not measured by his or her archaeological experience and skills. In contrast, for the archaeologists this was the most relevant criteria. This reflects the extent to which labor on archaeological sites is, or is not, connected to individual scientific expertise as opposed to need or social position (i.e., a right to share a limited economic resource). At the same time, it illustrates how community members and archaeologists held different ideas of the underlying purpose of archaeological labor—and how these different ideas were in constant interaction.

Interestingly, the interpretation of the past itself (as opposed to the handling and manipulation of material objects through excavation, washing, sorting, etc.) was not considered by workers to be part of their job. This goes against the grain of recent archaeological literature concerning how archaeologists should “include” indigenous people in archaeology by incorporating indigenous historiographies and interpretations into their academic texts. Clare Sammells (2009) has argued that individuals in and around Tiwanaku know a great deal about the archaeological past as a result of their work on excavations, as tour guides in the monument, and as managers and guards of the museum. However, they are also highly adept at code switching between locally specific histories, nationalist narratives of a glorious pre-Hispanic Aymara kingdom, and archaeological narratives that are suitable for tourists/archaeologists. Which narrative and form of evidence is drawn upon at any moment depends on the context of a conversation, but they do not see their selective use of different historiographic modes as contradictory, because there is no single “authentically Aymara” interpretation of the past that is timeless and invariable, and from which others are an impure transgression (see also Canessa 2012b).<sup>6</sup> Certain histories, moreover, are semisecret knowledge—for instance, stories of the monoliths walking at night are shared only with very specific audiences (Sammells 2009:116–123; cf. Abercrombie 1998:82–125).

My own observations during excavations at Tiwanaku and elsewhere in the altiplano suggested that attempts by archaeologists to involve workers more directly in the interpretative process on site tended to fall flat. On one project, maestros were invited to participate in the interpretative process by cowriting forms or recording video diaries. The results were awkward and discouraging, as the maestros steadfastly avoided questions posed by archaeologists about Aymara oral histories or the materials with which they were working. Simply put, interpreting and writing was seen as the archaeologists' job, not that of the workers—just as excavating and sorting artifacts was seen as the workers' job, not that of the archaeologists.

This points to the highly hierarchical and compartmentalized nature of the excavation project, wherein everyone's job is clearly defined and tasks are segmented, but also to how the labor division throws into relief contrasting ideas

of the purpose of archaeological work. For archaeologists, objects themselves (artifacts like ceramic shards or bones, or soil objects like a pit in the ground or the remains of an adobe wall) are not the end point of excavation. Instead, it is the relationships between material entities (such as artifacts and soil objects) that are recorded, analyzed, and treated as “facts” (Leighton 2015) that can be translated—at a much later point, probably while writing in a university office located in an entirely different country—into knowledge of the past. For archaeologists, therefore, excavation is only the first and most expensive step in a potentially decades-long research project, most of which will be carried out away from the field. The full research process includes years of analysis and artifact conservation; synthesis of written records and databases; and writing dissertations, books, and articles in conjuncture with teaching and other professional commitments. The end results are publications, university courses, and conference presentations that disseminate a new highly specific narrative or argument to a relatively small audience.

In contrast, Wancollo (and particularly the Mallkus and officials with whom the archaeologists negotiated) saw archaeology as synonymous with excavation and excavation as valuable because it generated employment. The end point of a good excavation was a plentiful supply of well-paid jobs, with as many families benefiting financially as possible. To achieve this, the Mallkus and maestros repeatedly pressured the archaeologists to resume excavations for an additional year, and the director’s insistence on the need to analyze and write up the backlog of data already gathered held little sway. This is not to say that Wancollo’s peons and maestros had no interest in archaeology at all.<sup>7</sup> Rather, there was not a lack of history that archaeology needed to fill: narratives or arguments about the past that Tiwanakeños helped archaeologists create would always be a supplement or addition to those they already held (Holtorf 2007:152).

Moreover, Sammells’s body of work (2009, 2012a, 2012b, 2013) and Hastorf’s (2006) discussion of the debates surrounding site museums suggest that the value of archaeological work for community members lies in its ability to generate archaeological tourism through producing objects that can be displayed in a museum or monumental structures that can be toured, or through its ability to create employment opportunities directly on excavations. Far from being disempowering or demeaning, plentiful employment and the economic benefits of tourism were more valuable than the intellectual knowledge that archaeologists could produce through their extensive analysis of databases and forms documenting material relationships already excavated (cf. Dawdy 2009:137).

### **ARCHAEOLOGISTS’ UNDERSTANDING OF INDIGENOUS LABOR**

North American archaeologists are trained first as anthropologists—but anthropologists who study people who are no longer alive, not the contemporary people

they live among and employ when in the field (cf. Meskell 2005). Archaeological workers are valued for qualities other than their cultural knowledge—for instance, maestros are valued for their experience working on many prior excavations, their precision when it comes to distinguishing archaeological loci, and their ability to lead a team. This makes them different from an ethnographer’s paid informants (cf. Schumaker 2001). Thus, while archaeologists are certainly interested in the lives of the people with whom they work, both personally and intellectually, the worker–archaeologist relationship is not exactly the same as the informant–ethnographer relationship. To put it crudely, living indigenous people will never be an archaeologist’s “data” in the way they must ultimately be for an ethnographer. As a result, when archaeologists consider and discuss their economic relationships in the field (i.e., the impact of being the main employer of every family within an entire community for six weeks each year), archaeologists are discussing ethical relationships that are not precisely the same as those discussed by their ethnographer colleagues. Yet because North American archaeologists are also anthropologists, with an academic training that was likely to include a heavy grounding in sociocultural anthropological history and theory, they tend to consider such relationships through the lens of ethical discussions of the indigenous informant–anthropologist relationship, which are primarily drawn from ethnography.

Moreover, it is common for North American Andean archaeologists to work in the same region and potentially within a single community for most of their careers, in the process forming close ties of friendship and kinship with individual families. Many told me that this experience rendered them “practically ethnographers.” As anthropologists, they were familiar with the academic literature (historical, archaeological, and ethnographic) on Andean cultures, as well as the broader social science literature on postcolonialism.

This professional expertise as anthropologists informed their understanding of their own ethical position but also their understanding of the labor rotation system and the extent to which it derived from a uniquely Aymara social, political, and economic structure. To be precise, it shaped their understanding of the economic relationships they formed with Wancollo and the degree to which they felt able to negotiate and bargain—not just practically but also ethically. For instance, was it ethical for the directors to insist that women were also allowed to be maestros if the community’s designated (but all male) Mallkus said this wasn’t acceptable?

As they discussed potential responses to each negotiation over wages and workers, they described the rotating labor system as a “traditional” organization of labor that was related to, if not exactly replicating, forms of reciprocal labor exchange documented in the ethnographic literature. In a sense, this also underlay their frustration when the Mallkus appeared to be “bending the rules” when they did not share

positions out fairly. But what was “traditional” Aymara labor organization in 2008? Lynn Swartley (2002) documented forms of labor in Wancollo during the mid-1990s, as part of an investigation into the “raised fields” experimental archaeology project.<sup>8</sup> The project was initiated by a group of North American archaeologists and NGOs, involved several communities including Wancollo, and aimed to reestablish pre-Hispanic forms of agriculture (Kolata and Ortloff 1989). Raised fields were promoted as a form of lost “traditional” Andean knowledge: a prehistoric agricultural system uniquely suited to the Andean environment that had been “forgotten” during European colonization. The NGOs and archaeologists could not understand why communities that initially were very enthusiastic about the experiment in 1988 had abandoned it by 1994. Swartley argued that it failed in Wancollo because it rested on two assumptions that no longer stood in the 1990s: (1) that communities had an unlimited access to male adult labor and (2) that they regularly engaged in “traditional” nonmonetary forms of reciprocal labor exchange known as *ayni*.

Reciprocal nonmonetary labor exchange had been recorded by ethnographers earlier in the 20th century and was understood to have pre-Hispanic origins (e.g., Altamirano Enciso and Bueno Mendoza 2011; Guillet 1980; Gose 1991:43; Urton 2013; Van Vleet 2002:569; but see also Weismantel 2006:94–95). Swartley’s (2002:131–141) 1996–1997 household survey identified nine different forms of labor exchange in Wancollo and asked people whether or not they participated in them. She argued that although people in Wancollo were aware of *ayni*, it was considered cumbersome and inconvenient; therefore, in 1994 only immediate household labor and labor in exchange for cash or kind were commonly practiced. Moreover, men frequently left home to engage in paid labor elsewhere, leading to a drastic shortage of adult labor to cultivate the raised fields.<sup>9</sup> Thus, Wancollo residents in the mid-1990s were familiar with “traditional” forms of labor exchange as defined by anthropologists, but increasingly preferred to rely only on immediate family members or to undertake nonreciprocal labor in exchange for cash or a share of crops.<sup>10</sup>

Clare Sammells (2009) then examined different forms of economic exchange in Tiwanaku between 2002 and 2004. In addition to the agricultural and migrant labor mentioned by Swartley, Sammells documented an extensive archaeological-tourism economy that included women selling souvenirs, restaurateurs, museum guards, and guides/guards for the monument. Tiwanaku was still primarily a subsistence economy, she argued, but there was a broad range of additional labor forms that included working elsewhere in Bolivia or abroad. Archaeological excavation work was thus one part of a mixed economy that included extensive monetary exchange across different sectors (Sammells 2009:58–59).

The archaeologists working in Tiwanaku in 2008 were aware of this economic diversity. Yet as Swartley’s analysis of the foundational assumptions behind the raised fields

project suggests, the ethnographic and historical literature inevitably became a lens through which contemporary indigenous economics were understood.

### ARCHAEOLOGY AS WORK

The inhabitants of this region of Bolivia do not represent a timeless indigeneity that ultimately resides in an imagined preconquest past—Tiwanaku’s economy is clearly both Aymara and contemporary. The labor associated with archaeological work must be understood through a similar framework. The labor system is indigenous (or more specifically, Tiwanakeño), but it is not reducible to this alone and has come into being through interactions and negotiations with different archaeological projects over several generations. Equally, archaeological labor in Tiwanaku neither looks exactly like other forms of labor organization found in the same region (such as agricultural labor) nor exactly like labor in other archaeological communities (Leighton 2015). The forms of archaeological labor found in these spaces are thus created relationally rather than being either purely “indigenous” or “scientific/archaeological.” Instead, they are a hybrid form of scientific labor that is characterized by the ongoing process of negotiating, bargaining, and compromising.

But what of power—epistemic and economic, as well as the connection between these two? At the end of the day, one group in this scientific relationship was being paid and the other was, quite literally, handing out money. If there is some reluctance to talk about the dynamics of hiring workers in the training manuals written for aspiring field archaeologists, no such squeamishness was allowable on site.

The workers and maestros were paid at the end of each week by the graduate student archaeologists, outside the artifact laboratory when all the tools and artifacts had been stacked away. Wages in 2008 had increased from the previous year to reflect a dramatic increase in food prices. In 2008, per week, maestros were paid 60Bs (US\$8.25); contra-maestros (assistant maestros), 50Bs (US\$6.90); and peons, 45Bs (US\$6.21). The previous year wages had been 45Bs, 40Bs, and 35Bs, respectively. On Fridays the graduate student archaeologists would collect a wad of money from the directors and count out banknotes to each worker in turn while the maestro stood by checking names off in a register. The worker would then sign next to his or her name to confirm receipt. One of the project directors, Olivia, told me that she insisted that the graduate students do this task themselves, even though they sometimes found it uncomfortable. We had this conversation after a long meeting with the Wancollo Mallkus in which the year’s wages had been negotiated. Some of the archaeologists had expressed irritation with the long back and forth and felt they should have been excused from attending the meeting. As Olivia saw it, however, it was part of her job as a mentor to ensure her graduate students did not romanticize the economic and class differences at stake when working in Tiwanaku. She described it as a “good sobering

experience” for students to confront their own discomfort at handing over money to the workers, and she would not allow them to delegate this task to the maestros.

Looking beyond Tiwanaku for a moment, it is true that there is a broader uncomfortableness involved in thinking of academic, intellectual, or scientific work as “work” or “labor.” The archaeologists working at Tiwanaku understood their own work as being more than “just a job”; it was a vocation—something they pursued because of a passionate interest in, and commitment to, the prehistoric past. This is hardly unusual. Scientists are supposed to be motivated by the pursuit of knowledge, not money (Shapin 2008; Traweek 1992:21).

As it happened, some archaeologists at Tiwanaku were actually receiving a weekly wage. The North Americans funded their airfares through grants or loans and received from the project only room and board, but the Bolivian archaeologists were paid each week by the directors. The payment of a wage, however, was implicitly understood to put the Bolivians at a disadvantage. This was underlined during one of my conversations with a Bolivian archaeologist. She was upset because she felt the directors were ignoring her input and sidelining her in favor of a North American. Having described this as unfair, she declared, as if trying to convince herself as much as me, that from now on she was going to treat her work as “just as a job,” nothing more than a way to get paid. Such a sentiment illustrates the extent to which, for the archaeologists at least, payment ought not to be one’s motivation for hard work.

Yet if some of the archaeologists were unpaid, and in fact accruing significant amounts of student debt to be there, this complicates but does not take away from the reality that they had significantly greater economic power than the Tiwanakeño workers.<sup>11</sup> For instance, it has been a point of contention at Tiwanaku that archaeologists are paid more than workers; in the eyes of Tiwanakeños, archaeologists are, or must be, benefiting economically. During my time at Tiwanaku, I could not get a clear idea of the extent to which people in Wancollo saw the archaeologists as benefiting in a manner that was direct and directly comparable to their own wages (in the sense that the Bolivian archaeologists and the workers both received a weekly wage) or if they were referring to the archaeologists benefiting indirectly (in the sense that archaeologists’ careers and therefore presumably their livelihoods eventually depended on the work that was being undertaken). The directors certainly worried that it was difficult to explain the latter situation. For instance, during negotiations over the archaeologists’ sponsorship of local festivals or their obligations to buy supplies for the local school, the directors explained that the project’s funding sources were national and that institutional granting agencies explicitly restricted how money could be spent.

In terms of noneconomic power, however, and specifically the ability to make knowledge about the past and be recognized as an authority, the disparities are less clear cut than might be assumed. The relationship between

archaeologists and workers—particularly the maestros—bears strong similarities to other field sciences studied by ethnographers and historians of science, wherein field scientists depended heavily on local field assistants and recognized their expertise. European anthropologists working at the Rhodes-Livingstone Institute, for instance, relied heavily on their Northern Rhodesian field assistants to collect their data (Schumaker 2001); similarly, European and US primatologists working in Tanzania were dependent on their Tanzanian field assistants (Haraway 1989). In both cases, scientists depended heavily on local field assistants to carry out their research and recognized the assistants’ individual and collective expertise. This echoes how Andean archaeologists worked with maestros. But Haraway and Schumaker both emphasize that the local technicians working with foreign primatologists or ethnographers sought out (with varying degrees of success and encouragement) opportunities to publish, travel abroad, or acquire doctorate degrees, and in doing so to shift their role from that of “assistant” to “colleague.” Field assistants were recognized as skilled, professional “technicians” but were ultimately unsuccessfully in their attempts to be recognized as equal scientists.

Noemi Tousignant (2013) has also described how Senegalese lab technicians sought recognition as toxicologists and chemists. In these cases, the reasons local technicians were unable to become recognized as “scientists” had less to do with explicit opposition on the part of individual European or North American scientists (as was arguable the case for technicians of color working for white scientists in the United States during the early to mid-20th century [Bloom 1993; Timmermans 2003]) and more to do with the broader contexts of colonialism (e.g., differences in economic and epistemic power between scientific institutions situated in the Global North and South).

In the case of the maestros and peons in Tiwanaku, however, indigenous technicians do not appear to be struggling in the same way to be seen as archaeologists, at least to the extent that this means participating in national/international academic communities, inhabiting an archaeological epistemology, or engaging with the narratives of the past that archaeologists produced, even when invited to do so. In this regard, they differ from the other technicians described above, in that economic authority is a more pressing concern than epistemic authority.

In this article, I have worked toward an understanding of why this might be the case. First, the scientific knowledge that archaeologists produce does not necessarily replace, disrupt, or challenge that held by their collaborators (cf. Joyce 2009:65; Salomon 2002). Moreover, indigenous people do not suffer from a lack of histories of their own, and as Cornelius Holtorf (2007) and Shannon Dawdy (2009) have argued from a European and pan-American perspective, well-meaning attempts to engage with broader audiences can be difficult to distinguish from older attempts to impose an elite, academic point of view upon a resistant and “uneducated” public. As I discussed above, Tiwanakeños are

perfectly adept at code switching between multiple historiographies, including those of archaeologists.

Secondly, the labor of workers is visible and valued at the regional level—in contrast to the labor of archaeologists, which only comes to fruition later and in spaces far removed from the field. The work of scientific technicians has been described as “invisible”—in that it is uncredited, unacknowledged, and less prestigious (Barley and Bechky 1994; Russell et al. 2000; Shapin 1989; Tansey 2008; Timmermans 2003). I suggest, however, that the relative (in)visibility of the indigenous technicians at Tiwanaku (both literally and in terms of their relative prestige) is less a problem of sight than a difference in site. In the field, the labor of maestros and peons is very visible. The 15 Bolivian and North American archaeologists working at Tiwanaku in 2008 were outnumbered many times over by between sixty to eighty workers. About half of the archaeologists worked inside the artifact processing lab—a relatively quiet building tucked away behind the museum. In contrast, the excavation units were bustling, open spaces. The workers’ labor was visible and its products easily measured: at the end of each day, several square meters of soil had been moved from one place to another; a train of wheelbarrows filled with bags of artifacts was pushed back to the artifact lab; and rows of washed artifacts lay drying in the sun. In comparison to all this visible and communal digging, sifting, and washing, the archaeologists stood watching, writing, and talking. The products of their labor were compiled databases kept on closed laptops or written forms and notebooks packed up each day into personal backpacks. The archaeologists’ labor was by no means invisible, but it would have been considerably harder for an outside observer to figure out what they were actually doing just by watching them. To invoke the divisions that Jacobo Copa Mamani and colleagues (2012) drew attention to, it is much harder to see “interpreting” than it is to see “laboring.” As such, it is not hard to understand why the manual labor of excavation and the generation of artifacts could be taken as most salient point of archaeology. From the perspective of the field, the manual labor undertaken by workers is the highly visible and public, while the products of archaeologists’ labor only appear years later as articles in scholarly journals that are likely read by fewer people than labored on the original excavation itself.

Whose labor is visible thus depends on where one searches for signs of action and agency—in the countryside surrounding the municipality of Tiwanaku or in the citation records of a journal downloaded from JSTOR. Workers are only invisible if one already assumes the purpose of excavation is to generate texts. Although I find the analogy rather cynical, we could think of texts as the currency of academic work: they add up to tenure and can be assigned in classrooms in exchange for tuition. More generously (and accurately), texts are interpretations of the past that contribute to an ongoing conversation within anthropology about the nature of social life. To see the production of

texts or archaeological narratives of the past as the most significant and obvious end point of archaeological work is to already be observing the process from the vantage point of an academic. This is not necessarily problematic or inaccurate—unless it is the criterion used to judge whether indigenous people who work in field sciences are passive or exploited. In contrast, shifting our attention to what labor on specific sites looks like and means opens up a space for thinking about indigenous technicians as more than silenced victims of archaeologists and thus begins to recognize their work and expertise as a crucial component of the scientific process.

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## NOTES

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1. In this article, individual archaeologists and workers from the 2008 project are referred to by pseudonyms.
2. An Aymara greeting, roughly translatable as “How is it going?,” to which the maestros’ reply is “All is fine!”
3. The term *comunidad* is more commonly used today than the term *ayllu*.
4. In contrast, consider Helaine Silverman’s (2006) discussion of archaeological tourism in Cuzco.
5. This organization was not just for the excavation roles. All other jobs on the project were also organized into teams of peons headed by maestros. There were maestro–peon teams of various kinds in the artifact lab, for archaeobotanical flotation, and to sew the cloth bags used to store artifacts. The only exceptions were the team of cooks, who were all women from outside Wancollo who had long-standing careers working on various North American archaeological projects in the altiplano region over the years. These women were paid some of the highest wages on the project.
6. Andrew Canessa’s detailed discussion of the relationship between historiography and indigeneity in the Bolivian Aymara village of Wila Kjarka further illustrates the problems inherent in trying to specify an “Aymara” or “indigenous” version of the past. Looking not only at the multiple historical narratives that are

employed in different contexts, he also discusses generational differences in understandings of the past. Interestingly, when people in Wila Kjarka talk of their descent from a prehistoric past, they talk of the Inkas and almost never mention the earlier Tiwanaku period (Canessa 2012b:79). This is despite the fact that the Tiwanaku period and the contemporary archaeological site of Tiwanaku have become the origin myths of contemporary Aymara-based nationalism, whereas the Inca are more strongly associated with Peru.

7. It is also important to acknowledge that it is highly likely there were significant differences of opinion within the community of Wancollo itself, particularly between the maestros and the Mallkus, that I was not privy to as a foreign researcher observing only public meetings and the excavation work. For an example of such intracommunity conflicts in the Yucatan, see Breglia 2005.
8. Raised fields agriculture involves creating high soil mounds in long narrow strips with deep ditches on either side.
9. The point is not that men are the only ones who can do agricultural work but that men migrate to undertake other forms of labor elsewhere that are indeed restricted by gender, such as the army or mining (Canessa 2012b), and this creates a shortage of adults back home who can work in the fields.
10. Elsewhere in Bolivia, other ethnographers had recorded different levels of commitment to *ayni* and related forms of reciprocal exchange. Elena Montenegro (2008:168–172) argues that reciprocity and rotational exchange is still an essential feature of community life, even in the city. Karen Lennon (2012:83–84), meanwhile, in her ethnography of a rural municipality in the Chuquisaca region, makes a compelling case for how “barter and complementary cultural practices such as *q’oa* and *ayni* are still practiced widely in rural Bolivia, [but] under increased market influences their vitality is changing or fading.”
11. I see a potential comparison between the extent to which North American graduate students indebt themselves in order to work on field excavations and Rheana Salazar Parreñas’s (2012) concept of “custodial labor”—a term she uses to describe the commercial volunteerism involved when women from the Global North pay significant sums of money to travel to Malaysia to engage in backbreaking labor on an orangutan sanctuary.

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