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A Moment in Archaeology: A Reflexive Examination of the Culture of Meaning-Making in Archaeological Fieldwork

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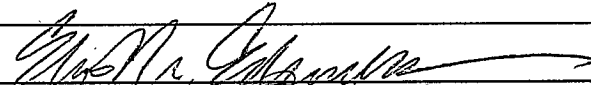
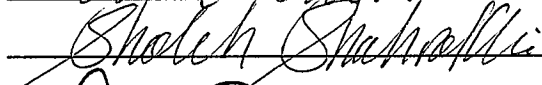
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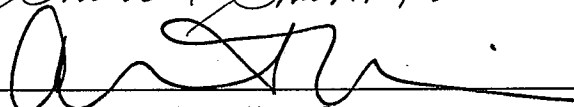
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**A Moment in Archaeology: A Reflexive Examination of the Culture of Meaning-Making in
Archaeological Fieldwork**

A Thesis

Presented to the Department of History and Anthropology

College of Liberal Arts and Sciences

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of

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of the Requirements for Graduation Honors

Jonathan W Irons

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Hamilton County, Indiana 2006:

My boots are stained with moisture from the morning dew; the sun has risen only a few hours before and hasn't had the chance to vaporize these peaceful drops into the stifling humidity that will make my life hell by four o'clock. Having spent the last four weeks learning the science and methods behind geophysical remote sensing in Indiana archaeology, I have designed a research project and am ready to begin excavating. I am looking for a house structure, or rather, a square-ish anomaly that showed up on my data I collected: an oddly placed white line among an otherwise black and grey image.

As the sun rises and the dew evaporates, I begin measuring and establishing the northwest corner of what will soon become my excavation unit. We hammer in a stake and measure its elevation relative to our datum.

From this stake, we hammer in another stake 2 meters due south. From these two we triangulate the third point. After fumbling with my notebook and decaying mathematical abilities, we hammer in the third stake and are able to measure and plant the final stake. We tie a pink string tightly around each metal stake to create a very real and exact boundary.

This two by two meter square of drying grass is hardly distinguishable from the rest of the grass around it. But now it is marked off. Now it corresponds in physical ways to the data I collected over the previous weeks. Now it is an excavation unit.

The sun has risen higher now and the heat is growing. The crew collects around me as this is the first unit to be opened up for the season. Dr. Joe hands me a shovel and tells me how to start. As the black steel cuts through the now-dry grass and soil, I feel a new sense of accomplishment and belonging as I have become an archaeologist.

Mt. Elgon, Kenya 2008:

It is my third season in the field. My excavation technique has improved drastically. Kenya is different from Indiana, but one can only approach two square meters of soil in so many ways. My floor is flat, my walls straight and the subtleties of this level are showing nicely. I have even done a nice job around the large root snaking its way through the center of the unit. As I clean out the last bit of loose dirt a sense of pride begins to fill me. My confidence grows as I peek up over the edge, standing on my tip toes, to check out the scene at the surface.

Six other similar units are being excavated. There is a flurry of activity as orders are called out and questions asked among the group. Kenyan workers haul buckets of brown soil from the units to the great piles of dirt off to the side. Still other Kenyans bring back empty buckets and crouch at the edge of their assigned units to observe the goings on below.

Through some carefully crafted and youthful acrobatics, I managed to extricate myself from the nearly two meter deep square. I glance down, my field of vision totally dominated by this marvelously orchestrated, meticulously excavated vacant square in the earth. I make some haphazard observations of the content of the unit and reach for my notebook.

Just a few feet from the edge of the unit is a large boulder and I sit down on it and begin my notes. I write down the depths of the corners and begin to remark on how the excavation progressed in this level. Who knew how much could be said about 10cm of unchanging soil?

To my right are more excavations. But for a moment I see beyond them. Past the other units is a corn field and an abrupt drop off. Beyond that I see a valley and another hill speckled by the round house structures that characterize much of rural Kenya. Beyond this is the Kenyan countryside, largely flat and decorated by rectangular quadrants of farm fields. The occasional schoolyard reveals some motion among the otherwise static scene.

I am suddenly struck by the vastness of what I am seeing. With a simple muscle movement my vision moves from a confined and controlled empty space to a vast countryside and an infinite blue sky.

I follow an outline of a road off into the distance and can make out our town. Here, for a moment, my vision goes so far that it reverses and whips around inwardly. Suddenly I see Kenya the year before. I see the friends I made there and the unforgettable sights I witnessed. Further, I see back to 2006 and the flat farm field into which I plunged my first shovel.

I blink and turn back to the unit. Suddenly it is dark and dingy compared to the mosaic of colors and images I had witnessed just moments before. Resisting the urge to look out over the landscape again, I continue my notes: “Two small concentrations of charcoal (possibly connected) were noted where the floor meets the west wall in the NW quadrant. Light scraping revealed some darker soil and two significant patches of charcoal. However, a root was pulled and an attempt was made to cut it out of the floor which disrupted this possible feature.”

Reflecting on these experiences it seems that archaeology is not nearly as homogenous as it is often portrayed.

Breaking the Dualities of Archaeology

Control. Science is nothing without control. An experiment with only variables demonstrates nothing of great value or significance. Without a control, you cannot understand the variables.

To many, archaeology is a science. It is not, however, a traditional laboratory science with controls and variables. Its experiments cannot be repeated and its variables cannot be controlled so much as managed and standardized. This regulation in archaeology has, until

recently, attempted to eliminate human error, indeed, the human experience from fieldwork. Though the experiences I had in Indiana and Kenya could not have been more different, the control and regulation in archaeological fieldwork attempts to minimize the importance of those differences.

As I looked up on that Kenyan mountain side I experienced a new vision. It was color vision, one of depth and intricacies. Through it I passed into my past and saw my first experience in excavation. Through it I was able to appreciate my accumulated experience in terms of my excavation unit.

I made a choice at that moment to switch my focus back to the science. I chose to leave the color behind and see only the browns and grays of the soil. But, I began to recognize the presence of that duality between the dynamic and the monochrome, personal and professional. As that day in Kenya progressed I began to see anew within that deep square. Suddenly the buckets were more than vessels for dirt; they became stages upon which the artifacts I excavated were laid. The confines of my unit were no longer controls; they became artifacts of archaeological field work. My excavation notes became full of a beautiful technical language that reflects a way of seeing the world. Even the piles of screened dirt, far from being a burden and future backfill, became enigmatic of priorities and conceptions of data, space and research within the archaeological community.

Once one breaks the duality between subjective and objective within archaeology, an entire new field emerges. A dynamic and penetrating vision reveals a complexity of roles and procedures that have, for the most part, gone unobserved, un-described and uncared for in much archaeological debate. My goal, then, within this paper is to show how I have utilized this subjective, reflexive vision and to illustrate the vast arrays of meaning that transform artifacts

from objects of singular, typological meaning to a palimpsest of complex social spheres (past and present). A reflexive vision inspires a process of examining the archaeological discipline from the inside, working towards understanding not only the product of fieldwork but also the fieldworkers themselves. Through this vision I went from seeing artifacts as stones and bones to being contested and unstable entities that move through and shape archaeological knowledge-making. I reconceived my notion of what field work is. It is not a closed “black box¹,” something either deliberately un-understood or too well understood to deserve discussion in site reports. It is more than a description of methods, funding and an acknowledgement of excavators. It is the foundation of the identity of the discipline and a supreme example of knowledge (as Bruno Latour might put it) *in action*.

The dynamics and complexities within field research that I have observed reveal a discipline in crisis, working out its role as scientific and humanistic enterprise. I intend to contest the very notion of objectivity within archaeology and encourage an embracing of subjectivity in fieldwork to facilitate this. I am not alone or unique in this endeavor, but my perspective as a site photographer and student will illuminate a unique vision of field school and a new reflection on archaeological knowledge.

I will begin part one of this paper with a brief discussion of the transition in archaeology from processual to postprocessual theoretical paradigms. This transition is one that epitomizes seeing archaeology in new dimensions, most notably through reflexive observations. I will invoke Bruno Latour’s idea of a black box in the construction of scientific fact (1987) and move on to open the “black box” of archaeology.

¹ I refer here to the conceptual black box that Latour (1987) presents to understand how scientific truth is derived. I will develop this more in my theoretical perspective.

In part two, I will describe my experience in my field school on Mt. Elgon, Kenya. After establishing an ethnographic context I will describe a series of moments (illustrated by photos) which illuminate the convergence of present social life and artifacts.

Finally, in part three, I will describe how the moments of field school open the Latourian black box. I will discuss the implications that recognizing archaeological fieldwork as a creative, meaning-making process have on ideas of objectivity in archaeological practice.

This ethnography of archaeology will demonstrate how, from Indiana to Kenya, the subtleties of fieldwork produce a research experience that has, to a large extent, been overlooked in archaeological literature.

Part I The Origins of My Theoretical Approach

This paper takes archaeology as its subject, recognizing archaeologists as important actors in the pursuit and production of knowledge about the past. Though archaeological publications overwhelmingly deal with theoretical conceptions of quantified data (i.e. artifacts, features, contexts) and how past cultures order their lives through material things, I do not concern myself with the *things* that came out of the ground. I deal almost exclusively with the stories surrounding moments in archaeological fieldwork. Artifacts, and the cultural aspects that they represent, are often involved in these stories, but only as slates upon which archaeologists write their stories.

In addition, my research dealt exclusively with the collection portions of the archaeological process. As such, the interpretation of artifacts (and theoretical implications of those interpretations) was outside my experience. In my experiences at the site I study here, interpretations were simple and limited; we worked with artifacts knowing that their *real* - that is archaeological - significance would be determined later in lab spaces. Thus, the majority of specific theoretical debate in archaeology lies outside the scope of my project.

Some might wonder if this paper is archaeological if it leaves out typological debates, physical description of data, discussions of geology and discussion about ancient cultural groups. To this objection I would argue that archaeological debate may focus on discussions of the past, but the practice of archaeology is grounded as much in present experience than it is in the material past. Surely, if the practice of excavation and fieldwork is archaeological, then research performed by a practitioner of such practices about such practices should be considered archaeological.

While my work cannot be considered a comprehensive understanding of the site I worked on, it is nonetheless a crucial factor of a comprehensive understanding and should be thought of as archaeological and not supplemental to truer archaeological work.

Finally, I offer two more arguments that should validate my work as archaeological. The first is that, as I will explain in the introduction to part two, I am inspired by the excavation process in a metaphorical sense. I found a site of interest (the field) and excavated, layer by layer (through photos), to determine the stratigraphy of the site and how these layers can be understood to represent a story of cultural occupation (the field season). Second, though my work was not grounded in any particular theoretical discussion of archaeology, its inspiration was. The debate surrounding the (ongoing) transition from processual to postprocessual ideologies engendered a curiosity in me regarding how artifacts (objects so pivotal to the conflicting styles of interpretation) entered into the archaeological record. I will offer a brief discussion of the battle between Lewis Binford and Ian Hodder and how I became more intrigued by archaeologists than by artifacts.

Of Processualists and Postprocessualists

The history of professional anthropology and archaeology goes back into the 19th century. Though major trends of thought tend to develop in relation and response to one another, we must find a point of entry, abrupt as it may be. For the purposes of this paper, the new ideas developing in the United States in the 1950s is as good as any.

In light of the large volume of data and interpretations being collected by early American anthropologist, in the 1950s a movement developed which sought to make sense of cultural variety. Julian Steward developed an idea known as cultural ecology in which biological ideas of

environment, adaptation and evolution were used to account for similarities and differences in cultural change (Erickson and Murphy 2008:144). Leslie White borrowed the idea of thermodynamic systems, and developed an approach which was focused on the availability, acquisition and use of energy to explain cultural evolution (Trigger 2007:388). This trend is known as the Neo-Evolutionist movement and would produce an important character in the story of modern archaeology.

Lewis Binford was a student of Leslie White and began to take the formulaic neo-evolutionary ideas and apply them to cultures of the past. Upset with the lack of systematic procedures for understanding the archaeological record, Binford developed the 'New Archaeology;' a revolution of the discipline which embraced scientific and positivist approaches to regulate the interpretation of archaeological data (Trigger 2007). At the core of the neo-evolution movement was an idea that human behavior was patterned and could be predicted if one knew the nature of pressures placed upon them. Moreover, given the ecological nature of human culture-making, behavior could be fit into different types of systems and processes. Thus, the New Archaeology would take on the name 'Processual' and came to focus on the quantifiable processes of culture that could be empirically determined from the archaeological record.

Along with processual archaeology came the development of rigorous data collection that characterizes most common understandings of archaeology. It was generally assumed that one must have a clean and uncomplicated data set in order to begin to fit the interpretations of artifacts into the equations.

In hindsight, through which modern textbooks are written, it is easy to see the flaws of this deterministic approach. Indeed, as the development of culture was at the mercy of

environmental, external factors, the agency of human actors within cultural expression was minimized. Though neo-evolutionary and ecological ideas were beginning to explain how things changed and got to the way they were found in the early 20th century, it was only a partial story. The rejection of neo-evolution was at the heart of the new movement in archaeology. The movement was named by Ian Hodder in the mid 1980s as Postprocessualism (Trigger 2007:444).

Inspired in part by movements in cultural anthropology of France and the United States as well as the rise of postmodernism in the humanities, postprocessual archaeology would embrace the human factor in the story of culture (Trigger 2007). Using a critical approach, a postmodern refusal to accept absolute determinants (social or otherwise) as patterning culture, characterizes postprocessual archaeologies.

The nitty-gritty arguments about the differences between processual and postprocessual archaeology lie outside the scope and intent of this section. I think that the basic contradiction emerges from two wit-filled book reviews, the first written by Binford second by Hodder reviewing Binford's book. Binford writes about *Reading the Past: Current Approaches to Interpretation in Archaeology* that, "Hodder is involved in a power play, seeking domination for his value-laden ideas" (Binford 1988:876). This comes as a result of criticizing Hodder's suggestion that subordinate claims have been silenced by archaeology. Binford seems to think that the political use of the past in the present lies outside the scope of archaeological interpretation. By ignoring his own cultural presence, Binford could not connect his own sympathies with, for instance, the plight of Native American reservation communities with his professional pursuits.

Reviewing Binford's *Debating Archaeology*, Hodder responds directly to the previous review. Hodder asserts that one of the main points in the book is that, "Binford does not need to

talk to people because he already knows that beliefs... of the past participants could not have been relevant to macrotemporal system change” (Hodder 1991b:385). The tone of the review is one that attempts to coerce Binford to accept that science is “culture bound” and to accept agency of social actors past and present.

This argument is over the primacy of systems or humanity. Is it structural, predictable change or change at local levels that alters culture? The debate continues, though with increasing attention and acceptance paid to postmodernism in socio-cultural anthropology, Hodder’s conception is winning favor.

I found, as I became more and more familiar with different theoretical arguments, that practice still seemed outside this realm. Indeed, it is hard to commit the practice of archaeology to formally written publications; learning through field school, through practice, seems to be the means of theoretical transmission in terms of practice. It was when I found an article published by Hodder establishing an “interpretive archaeology” that I began to really conceptualize what I wanted to learn about archaeology.

Interpretive Archaeology and Guarded Objectivity

The 1991 article by Hodder comes at the head of the postprocessual movement, though he offers a critique of both processual and postprocessual practitioners in this piece. While many archaeologists at that time had become critical of processualists’ theoretical ineptitude (their adherence to deterministic and systemic understandings of change), Hodder criticizes the postprocessualists for focusing too much on self-referencing theory that made, in his opinion, archaeology inaccessible to many populations. He emphasizes three points which define an “interpretive archaeology;” a guide for grounding poststructural theoretical ideas in practice.

One of these points was the idea of “guarded objectivity,” (Hodder 1991a:10) an attempt to reclaim contextual authority of data. A guarded objectivity maintains that there are elements of archaeological data which are objectively understandable; the law of superposition, for instance, could give absolute evidence for one cultural group occupying a place before or after another. Poststructural and postprocessual archaeology had instituted a level of relativism the likes of which some referred to as cognitive anarchy (Binford 1989:34). The result was that disenfranchised claims about the past could be dismissed as lacking the authority that all claims to knowledge lacked. Through guarded objectivity, Hodder reserved the right to empirically state that certain interpretations of culture history were wrong.

Despite this rather absolute claim about the nature of archaeological data Hodder recognized that “[he and the data] bring each other into existence dialectically” (Hodder 1991a:15), that the importance lies in the experience of the data. It is curious, then, that he chose to retain the notion of objectivity in the way that he did.

If the rise of postprocessualism is a reaction against the determinist ideology of culture change proponed and perpetuated by Lewis Binford, one that is dependent on empirical objectivity in method, (and, as Wylie [2001] suggests, if Binford’s notion of *relative* objectivity (1989) that was not different from a *guarded* one) why would it figure so prominently in Hodder’s three steps to interpretive archaeology? The notion that authority and power of subordinate groups can only be maintained by accepting objective realities is unconvincing. I saw the notion of guarded objectivity as being more intertwined with archaeology’s maintenance of a scientific guise than with empowering the other (though Hodder’s work with various minority groups in and around his sites clearly indicates a real concern with empowerment).

After reading *Interpretive Archaeology and Its Role*, it seemed to me that Hodder was suggesting that reflexivity in archaeology should begin arbitrarily where the objectivity of the data ends. That is, reflexivity should occur during the archaeologist's experience with the data, the interpretation. Indeed, in his brief discussions of reflexivity in his 1991 article, no mention is made of fieldwork. Hodder's (2001) volume about the reflexive method in action at Catalhoyuk defines reflexivity as "the effects of archaeological assumptions and actions on the various communities involved in an archaeological process... (pp. 9). The recent volume incorporates the process of excavation within the broad realm of reflexivity. In it ethnographic methods are applied throughout all realms of the project, including diary entries kept by excavators. The resulting catalogue is an immense database of written and video interviews with archaeologists and local people which is cross listed with the artifact database.

Still, he establishes a division between endeavors that are *anthropological* and those that are *archaeological* (Hodder 2001:8) keeping sacred (and safe from the nihilistic postmodern socio-cultural anthropologists) guarded objectivity, and thereby, "the authority to be able to say that a particular interpretation does not fit the data" (Hodder 1991a:16). It seems to me that guarded objectivity sets limits on the depth to which reflexivity can pervade archaeological knowledge making.

Now, an argument can be made that the limit on reflexivity that is set by guarded objectivity is not arbitrary but based on the realities of the scientific method. Indeed, it is hard to dispute the invocation of the law of superposition in terms of objectivity in archaeology. To counter this, I consulted studies of science in which the very notion of fact is questioned.

The Creation of 'Fact' in the 'Hard' Sciences

Archaeology as a type of science under sociological and anthropological investigation is still in its infancy. Though Hodder included socio-cultural theoretical debate in his discussion of reflexivity (Hodder 2001) and Shanks and Tilley called for a “sociology of archaeology” (Shanks and Tilley 1992), I have found few volumes of research on the archaeological process and the conclusions from what few studies have been done are still absent from many contemporary textbooks.

However, lab sciences have been opened up to sociological and philosophical investigation. Bruno Latour, in his collaborative work with Steve Woolgar and his independent investigations into the social construction of fact, offers one of the strongest frameworks. These two sociologists have devoted their studies to the field of science. Latour’s book *Science in Action* (1987) and their collaborative work, *Laboratory Life: The Social Construction of Scientific Facts*, (1979) are some of the most acclaimed works in the field of sociology of science.

The Black Box

In *Science in Action*, Latour attempts to develop of method of investigating science by studying science in action, as it is made through the social worlds surrounding the dissemination of facts. As Latour begins his voyage of “dissent” into science he says that we should modify the words of Dante and, “abandon all knowledge about knowledge all ye who enter” (Latour 1987:7). To abandon knowledge of knowledge is to begin by exploring the controversies that surround the events in which facts are “discovered.” Indeed, Latour suggests that facts are not discovered but made, for there is no exact moment when a fact appears: is it at the moment the

data indicate it or through the repetition of the experiment or the corroboration and support of colleagues?

Latour uses the concept of a “black box”, a conceptual tool used by technicians who cannot perfectly understand the workings of a mechanism (or do not need to), but only concern themselves with the input and the output (Latour 1987:3). The makings of facts, the high-running emotions, budget concerns and professional drama, take place within the black box. And, as the fact nears its solidifying (through corroboration by other practitioners or reiteration and reinforcement in further work) the box closes. Latour introduces two-faced Janus to represent the duality that blurs the moment that the box is closed: “When things are true they hold/When things hold they start becoming true” (Latour 1987:12). In the instance of the discovery of the double-helix DNA structure, we can see that the simple and “pretty” concept of Watson and Crick was not enough to make a fact. They needed others to confirm the pairings of enzymes and the appearance of the structure in the X-Rays. So, confirmation and acceptance in the scientific community of the double-helix came with ease because of the success of its structure, but was nonetheless necessary to supplement the truthiness of the structure. Finding the exact moment of closing is difficult and always leads down more and more labyrinthine paths. Latour uses this paradoxical condition of “science in action” as his principal method and outlet into the science powerhouse.

What Latour effectively does is to deconstruct the notion of scientific fact and demonstrate that the emotional, erratic and enigmatic (those characters blackboxed and ignored in the discourse of science) are integral to and inseparable from the construction of scientific knowledge. This realization is phenomenally insightful and critical to any sociologist/anthropologist/archaeologist of science who is trying to wrap his or her mind around

the objective and subjective duality. Consider for a moment that James Watson's narrative tale of the discovery of the double helix was published in 1968, over ten years after the discovery was made (Watson 1968). The fact, the truth of the structure of DNA had floated around the scientific community accumulating prizes whilst its 220+ pages of emotional baggage went untold. The black box in this case was powerful in its ability to maintain this separation between articles in *Nature* and personal, politically charged accounts of pivotal discovery.

The Delicate Web of Scientific Authority

Latour and Wooglar offer one final and critical insight into my study of archaeology. In *Laboratory Life* the two are some of the first to open the professional and cultural space of the science lab to investigation and understanding. In this early work they tell of the concern with order versus disorder and that lab spaces and scientific publications are means to control the "natural", disorderly condition of scientific information (Latour and Wooglar 1979:245-246). One senses a sort of paranoia coming through these accounts of science. Indeed, at the end of *Science in Action* Latour has built such an intricate picture of science, one influenced by a near-infinite amount of controlled and haphazard stimulations and responses.

Scientific fact, beyond emerging out of paradoxical social situations is also a communal endeavor. This delicate "network" establishes scientific authority by grounding knowledge and claims in a recognizable metrological world. Metrology is the act of fabricating an outside world inside which facts can survive (Latour 1987: 251). The most profound aspect of the scientific metrology is its collective and artifactual nature. It is, paradoxically, made authoritative by its support and respect throughout the scientific and laymen communities, whilst its immensity and success are grounds for the support by both communities.

The network, the web of scientific significance and authority is delicate and not all-encompassing. Failures arise, and Latour claims these failures can be traced back to punctures in the network in which “out-thereeness” is encountered (Latour 1987:249). Out-thereeness is the scientific wilderness, untouched, and, more importantly, uncontrolled by the authoritative web of science. Like a doctor without a medicine cabinet or a geographer without a map (who become effectively powerless without their manifestations of the scientific web and authority), Latour suggests that the scientific networks were collective creations, and as such, are permeable and have limits.

The conceptual black box and network characterize truth and authority as not absolute, but rather immense, social and imperfect. Though archaeology is easily separated from other lab sciences, it has found a niche for itself inside this Latour-envisioned network of objective authority; a network which, viewed from inside alone, tempts the user to accept some things as indisputable facts. Ideally, science is all about questioning and retesting (so that no facts are simply accepted as true), but only a carefully managed questioning and re-investigation can maintain the basic structure of the idea, the safety of the network and its comprehensibility in scientific discourse. The use of a guarded objectivity effectively limits the scope of dissent and preserves the very roots of the network as hallowed ground. Though Hodder claims this is to protect the subordinate voice, the result is ultimately that archaeologists can have a final, authoritative say as wielders of the delicate web of scientific authority.

Latour and Wooglar’s work is an outsider’s journey into the heart of science. Yet, it establishes a roadmap of dissent into any science and for any person. An archaeologist embracing reflexivity could adopt these means to recognize his or her own position in

archaeology and begin to understand the discipline from the inside. Working from the inside can be scary though, for destabilizing the web could allow for one to drop through into the irreconcilable world of out-there-ness. But, I think the leap is necessary and not detrimental. To reflect we must map every fiber of the archaeological web to its limit, which means accepting the completely artifactual nature of its fibers and imagining a world in which they do not exist or are not alone in their authority. Maintaining a notion of objective truth is to maintain a blockade against dissent, to blind one's self of certain fibers. And, even if one falls through the web, the web will mend and survive. The trick, for the insider, is to get back into the web; for much sociology and ethnography of science remains ostracized as something separate from the purer research at hand.

Section two of this paper represents a style of dissent into archaeology, a means of feeling out the various fibers of the archaeological web of meaning. In section three, the conclusion, we fall through the web, aided by the outsider's (Latourian) perspective, while at the same time reintegrating, indeed, maintaining an archaeological identity.

Part II

Snapshots of Mt. Elgon, Kenya: Meaning Within Archaeological Sites of Knowledge

The 2007 and 2008 Field Seasons

I was fortunate to earn the opportunity to work with Chap Kusimba of the Field Museum in Chicago as part of his ethnoarchaeological project in the Mt. Elgon region of Kenya. For several years, he has conducted both ethnographic and archaeological research there in an attempt to put together a comprehensive understanding of the local culture, past and present. I went with Chap and a team of students in the summer of 2007 for six weeks and was involved in the excavation portion, especially the sorting and cataloging stages, as well as the photo-documentation of the project. My individual project was to document the archaeological process; to collect photographs from each of the steps. At that time, my involvement in the excavation and cataloging stages made this almost impossible and I dedicated myself more to the archaeological project.

I was left unsatisfied with the amount of photos I had taken and the small amount of thought I had devoted to this project. The idea of photo-documenting the archaeological process had been growing in my mind for months before the 2007 season and never diminished. Indeed, it served as the inspiration for this project. Dr. Kusimba afforded me another opportunity to return with him to the very same region and work as part of another field school.² In the following, I will attempt to establish the basic premise and nature of Chap's project.

² Other members from the 2007 season returned again in 2008, but most of the participants were new. As such, the environment of the 2008 season was unique, despite the factors that had remained constant from the previous year; those constants being Chap himself, the returning members, the location, certain informants and the archaeological research itself.

Archaeology on Mt. Elgon

The 2008 season included of a myriad of individual projects orbiting Chap's main research in the area. Activities during the heart of the field school were either archaeological or ethnographic work. For the most part, the group was wholly dedicated to one or the other at any given moment, but towards the end of the season time constraints on individual research forced cooperation and indeed, co-mingling of the two aims. As such, the field school was an unpredictable mix of ethnographic and archaeological experiences and goals; it was impossible to approach either the excavation or the observation without giving thought to the other.

Ideas of what constitute ethnographic and archaeological work vary and it is important to state explicitly the nature of the research project and our locale. Chap's work is an integration of these traditionally separated sub-disciplines of anthropology. On the archaeological side, Chap targets rock shelters and cave sites for excavation where human occupation dates back some 2,000 years into the Pastoral Neolithic. These sites yield an abundance of ceramic material from both recent and early times. The dedicated archaeological work (that is, excavation procedure) was as typical and thorough as any other similar project. In 2007, we opened four 2 meter by 2 meter units, at a rock-shelter (Chepkarai) site, sampling both inside and outside of the drip line. A long trench was also excavated and carefully mapped for a more detailed and comprehensive understanding of the site stratigraphy in its entirety. In 2008, we excavated at two adjacent cave sites (Chemai I and II), not far from the rock shelter of the previous year. At Chemai I we opened 8 individual 2x2 units outside of the cave. At Chemai II we opened only 4 units in an attempt to survey three distinct areas outside of the cave. All excavations from both years (with the exception of the large trench at Chepkarai) were conducted in 10cm arbitrary levels until bedrock was exposed.

Archaeological work, as it were, continued beyond systematic excavation. We were responsible for a fair amount of processing of the archaeological data. In cooperation with heritage laws we, as a foreign team, were not allowed to remove substantial amounts of material from the country. The entirety of the artifacts was the property of the University of Nairobi. The most “important” artifacts were, in effect, loaned to the Field Museum of Chicago and we were charged with determining what material would be brought home for further study. To manage this we were responsible for cleaning, counting, sorting and conducting primary analysis (through a catalogue of brief description) while in the field. This task was of great importance during the 2008 season.

Chap and His Collection of Pots

Ceramics in the archaeological record are the best way to differentiate between cultural characters (especially in the Mt. Elgon region where lithic traditions are limited by a lack of available material). Ironically, though ceramic traditions are highly variable, constantly shared and expanded upon, their persistence has remained a constant trait from the Neolithic past to the information age. As a way of illuminating both the past and the present, Chap works with local potter guilds to understand the culture, economy and politics of ceramic traditions. In the 2007 and 2008 seasons he documented (through video, photos, interviews and participant observation) the entire process of producing a ceramic vessel from procurement of clay to the day in which it is sold at market. The archaeologist within him shows through his methodology and representative sample. In the 2007 season he collected over 250 vessels from 3 guilds and shipped them to the Field Museum.

Our ethnographic research in 2007 focused almost solely on the potters. Students pursued work with the potter guilds and took detailed notes on each stage of the process. These notes were then collected and compiled in order to gain multiple perspectives into the process. I was charged with photographing the ethnographic adventures we took; though to me this also meant photographing the anthropologists as well as the informants. In 2008, Chap introduced several other variables. Several students busied themselves with understanding the agriculture and subsistence economy of the area. So, while we worked with a small family of potters, our focus was not entirely on the ceramics but also the spatial organization of the homestead and distribution, description, and maintenance of crops. One student in particular extended this sort of survey to the various farms around the Chemai caves.

These ethnographic portions of the research were not simple supplements to the archaeological work; they emerged as entangled with and borrowing from the archaeological work. In one of the few formal conversations I had with Chap, his vision of anthropology showed in his inspirational and ideological language. For him, anthropology can only be known as a holistic discipline. He recognizes the methodological and theoretical constraints that result in the division of sub-disciplines in practice. However, in his conception of his research and “legacy” at the museum he does not so easily separate ethnography from archaeology from museology. He is at once acting as an archaeologist working as a representative of a museum of natural history within a local rural culture. Efficiency in collection and time best describes his approach in a utilitarian sense. To make the most of what precious little time he has each season nothing is off limits for collection.

What this meant overall was that our intrusion into the daily life of the Mt. Elgon Kenyans could not be ignored in any sense. Indeed, we made the most of it. This region of Mt.

Elgon allowed us to gaze into that uniquely African reconciliation of the imposition of modernity and deep histories. We did this by conducting a series of intertwined projects; moving from one project's questions to another's depending on the specific situation. Although our schedules dedicated times for archaeology and ethnography, the questions in our minds would always change and mix.

The Origins of My Project

The description above demonstrates the character of the project and range of activities I was involved in throughout both seasons. My role as photographer within Chap's diverse project was critical to my understanding of the archaeological process there. I will bring this to the fore in a description of one of my own excursions as the project photographer which I will give in detail later. For now, I will only say that the diversity and unpredictability of day to day work in the field schools meant that my camera was never far away. As a result, the boundary between the archaeological process and ethnographic documentation became diffuse. My photos from the 2007 season show that my concerns more structured as distinct; I shot very little of the archaeology and my best shots were of the ethnographic days. In 2008, though, with a more directed project and goal, my eye that produced images was not limited to only the ethnographic. I managed my time better and, while fully devoted to the archaeological research at hand, I did my best to play the part of participant observer as well. A project that presents you with the task of documenting an aesthetic creative process one day and places you in a 2x2 meter square the next lends itself to a change in perception. This is why I think my role as photographer was so important.

In the following portion of the paper I will tell a series of stories illustrated and inspired by individual photos. These photos are the result of blurring the transition between documenting the hand-crafting of a pot and documenting a day of excavation. The effect is two-fold. Not unlike Chap, I see the contemporary potting process in an archaeological way; orderly, systematic, and segmented similar to the way excavations are. However, though Chap blurs the line between his sub-disciplines, he did not worry himself with ethnographically documenting his archaeology. The other effect of being involved in his project was that the archaeological process became more open to interpretation and indeed, more aesthetic than I had previously known it.

The Research Settings on Mt. Elgon

Places: The Site

Having established the basic premises of the field school that I worked in, let me now set a more specific contextual premise. I will begin by establishing the settings and the characters that emerge within the field school with the hope of making the snapshots of the field school less disembodied.

There were three main settings of the field school; Chemai (the site), the compound and Robai's. Excavations were most intense at Chemai I, so when I refer to the site I mean this specific cave. Chemai I was a cave set on a steep slope of Mt. Elgon. The rock face climbed steeply upward to the roads above. At the mouth of the cave was a small clearing dotted by a few shrubs, boulders, and one large fig tree. This clearing was also fairly steep, perhaps a 35 or 40 degree incline, but steeper in some places. Beyond the end of the clearing was an even steeper drop off upon which maize was planted. The view from the site, then, was one looking off into

the distance. Directly across from the hillside was another mountain (the two were connected, but a deep valley had separated them to a great extent). Squares of varying shades of green checkered the landscape all the way to the horizon beyond the second mountain. It was upon this

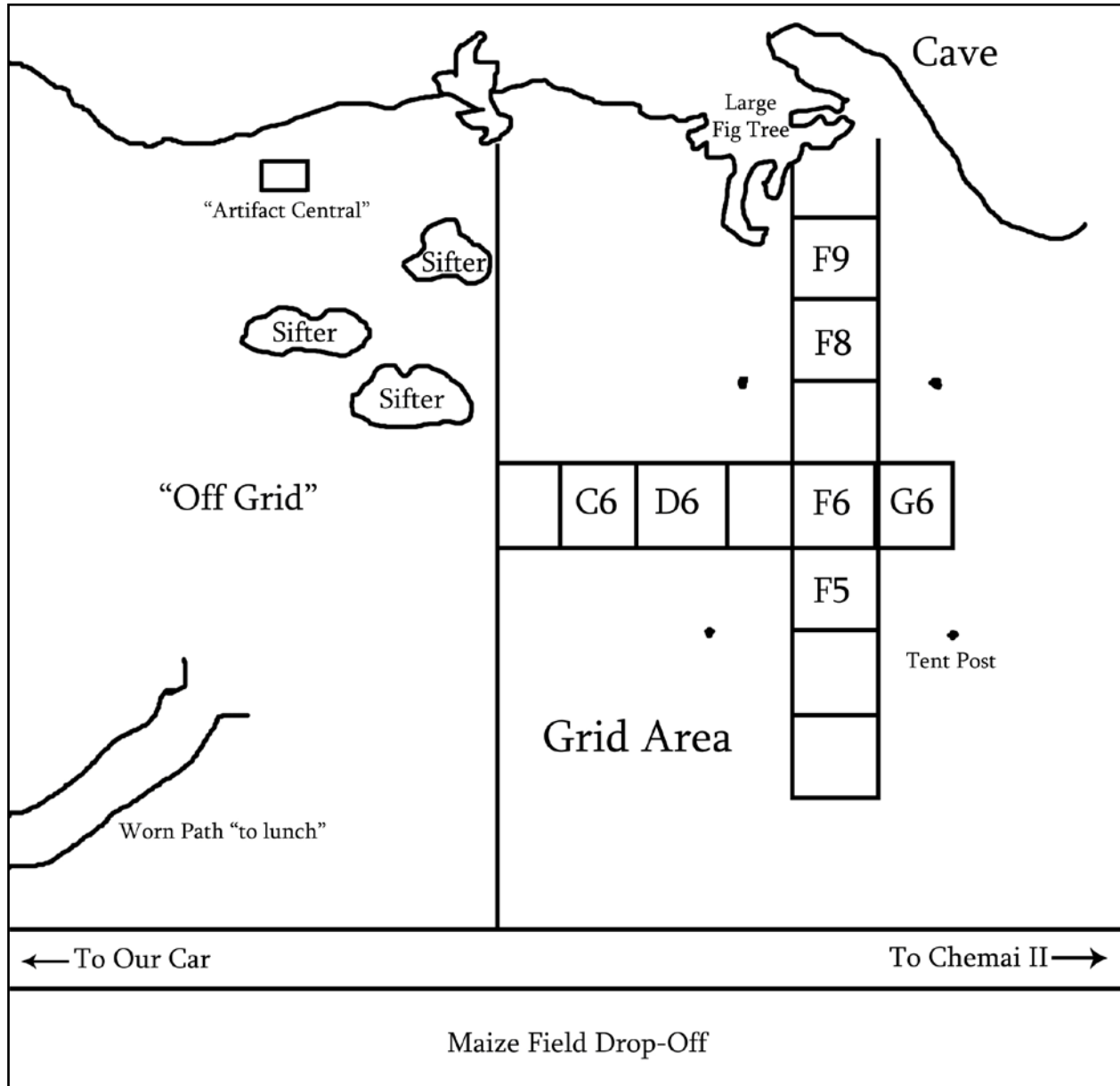


Figure 2.1, Chemai I

decidedly scenic and beautiful hillside that the archaeological portion of the field school was located. We would park our van near the local family homestead and walk a short ways uphill to

the site. On a portion of the site we laid a grid out of string oriented in line with magnetic north. On any given day we would have some twelve students and twice as many Kenyan workers moving around the grid and focusing on various 2x2 meter squares. Just beyond the southern extent of the grid, towards where we parked our car, there were three screening areas set up. These were later identified by three massive piles of fine, sifted soil with screens set atop. Just a bit beyond the screening areas was what I call “artifact central.” Here all the artifacts from the different units were brought to be bagged accordingly and often examined and identified. Figure 2.1 is a digital version of a map I drew of the site on midway through the excavation period.

Places: The Compound

The next scene was the compound, or the gated homestead where we lived. My description of our archaeological homestead starts on a typical Kenyan morning. It is cool and my breath fogs as I step out onto the drab red cement of the front porch. Somewhere a dog barks and just across the compound a rooster crows. I sit atop the wall and extend my legs out along it. Enjoying my mango juice (from concentrate) I fight the cold by sitting in a ray of sunshine shining through one of the many tall, yet young, trees that dot the compound.

Here was where the entirety of our off-site archaeology took place. Owned by Chap, this compound was made up of 5 separate structures. The largest was the main house within which Chap and other assorted students and teachers resided. Two round structures served as meeting places and make-shift labs (and also a party center). A large dormitory had also been built that contained 6 suites of two single bedrooms and one common bathroom. The last structure housed both our permanent lab (where equipment and artifacts were stored) and a small home where some of Chap’s family lived.

All of the buildings were made of cement with various gaudy paint-jobs. The dormitory was red with some sort of sky-like blue trim. This blue was one common color tying the entire compound together. One of the round structures (or bandas) was bare cement with a tin roof, the other painted as if it were made of bricks with a thatch roof. The lab and small house were painted another red (blasted by the orange Kenyan earth) but was later painted a bright orange on a whim. The main house looked almost Mediterranean, with several balconies and a mix of reds and oranges and blues along the outside.

In 2007, most of our free time was spent in the thatch banda (all meals were taken there except for breakfast which was taken in the main house). In 2008, the main house was the gathering place. The bandas were intermittently used for laboratory space and were, as such, considered off limits to many.

Inside the main house we would gather in a living room to talk and eat during meals. Upstairs were several bedrooms and one plain backroom. It was in this tiny, square, cement room that most of the cataloging was done. The washing was done on the small cement patio or porch in front of the main house. A screen was set up near to this patio for drying.

Places: Robai's

The final setting was Robai's homestead. Robai is the leader of the small family potting guild that we worked with in 2008. Her land was not too far from Chap's compound, some 10 minutes by matatu (an African van) or an exhilarating 20 by boda-boda (or bike taxi). Well off the main road, Robai's home was the main structure on the land; a square house with two main rooms made of mud and clay and a tin roof. The houses of her 3 co-wives rested in a semi-circular pattern about 100 meters from the main house. Not far from the main house a fence

extended that separated the farm area and a maize field had been planted adjacent to Robai's home. It was in the shade of the house and a large fruit tree that Robai and her partners molded their pots. I have seen them work twice and the soft, dramatic lighting creates a mystifying scene of hands and subtle voices working with the utmost skill and ease. It was hypnotic and I found myself on occasions neglecting my photography duties and just witnessing this process.

It was in those three settings that we found ourselves, in the 2008 season, most often. For the most part, the entirety of a day was devoted to a single scene (usually either Robai's or Chemai) with the compound serving as our living quarters and hub.

The Characters

To ease the exploration of the snapshots to come, I will also describe a cast of characters in brief; for many of their tendencies and quirks will emerge in the different encounters.

Chap. Chap is the easiest to start with. Being the leader of the project he commanded a great deal of authority. Any decisions about the work ahead would pass through him and indeed, he was always the ultimate designer of the day's itinerary. He is tall with soft, yet resonating voice. His Kenyan accent, though thick at times, gave a rhythm and timbre to his lectures and monologues that I would find typical of other Kenyan academics and storytellers. Partnering with his wife Sibel, the two have conducted research in Kenya for some time. The project in the northwest of the country is the newest, yet has been ongoing since 2001. Their other projects focus on an area near Mombasa, on the Indian coast of Kenya.

The Mwalimu. The Mwalimu was our direct connection to the University of Nairobi. Mwalimu is the respectful Kenyan term for a teacher or "wise one" and even Chap used this term, for the Mwalimu was Chap's mentor during his Kenyan studies. In the 2007 season I found

his demeanor to be quiet and reflective, but a whole other side to him burst forth from the moment we were re-united in the 2008 season. As he arrived to our homestead, his eyes lit up and he grasped my hand with a loud and forceful slap (true to the Kenyan handshake tradition). This very friendly greeting would transform into a more casual relationship; one of jokes, goofy looks and occasional confession. The Mwalimu could instantly switch from a wise calm to a sudden chuckle with little or no explanation. I have the utmost respect for the man and found his switches endearing and reasonable (for no one can be all work all the time). He took more time to interact with the students, especially the two high school students that came along, and truly relished the opportunity to conduct this research. I must admit, though, that I fell in favorably with the Mwalimu (likely because I was returning) and that those who did not fall in so favorably found his contrasting personalities to undermine his authority. Ultimately, the respect Chap paid to the Mwalimu and his expertise in the archaeology of the area made his opinion revered in the field school, this account recognizes this level of authority and the authority I conferred to him as a friend.

The Grad Student. In his late 20s, the Graduate Student was pursuing his Master's under the direction of Chap's wife and colleague, Sibel. He had come to Kenya to contribute to the growing record of artifacts that had emerged from the husband and wife team's ongoing project in the area; the data which he would be basing his thesis on. His connection to the project was one of great responsibility as Chap would often defer to him and the Mwalimu to guide specifics on the day-to-day research. The data we were recovering were *his* and he was careful to keep track of them and have a strong hand in their processing. To say he was staking the entirety of his professional career on this project might be an overstatement, but I can say definitively that

the success of this field season would reflect directly on his standing as an archaeologist and his relationship with Chap.

The High Schoolers. In both the 2007 and 2008 seasons, two high school students competed in and won an essay contest in their Chicago high school. One male and female winner were selected and given the opportunity to travel with Chap and his field school funded by the museum. These students were young and untrained and we were told that we would be mentors to them; teaching them what we knew about archaeology and serving as examples of success in higher education. For all four students I met, the trip to Kenya was their first time out of the country and out of the state. During excavation and research they were to soak it all in as well serve as a kind of labor. Fieldwork often comes down to needing bodies to work at times and though the high schoolers were unskilled, they could be placed strategically to dig or free up other experienced excavators from less important jobs. In another sense, the high schoolers bring a new and vibrant color to the compound; whether it was making the relatively young college students feel out of touch with youth or sharing jokes and gossip about their schoolmates and hopes for the future.

The Leaders. Though the pool of students was very small in both seasons, there is nonetheless a stratification of abilities and dedication that emerged in the compound. The Leaders are those students with previous field experience, both those returning and those bringing experience from elsewhere. Age need not be a factor as in 2007 when I, one of the youngest students (high schoolers not withstanding), still had field experience and served alongside the 2007 grad student as a leader. Leaders were given responsibility for their own excavation units and to instruct the newer students in field techniques. On the downside, Leaders often clash with each other regarding how to go about doing things. Often arrogance comes with

increased responsibility and a higher favor in the eye of Chap. Nevertheless, we serve an important bureaucratic responsibility to the project in our simplest sense by maintaining the individual units and the notes.

The Newcomers. The newcomers are the general student population of the field school. Many were studying archaeology and breaking into the field school for the first time. Others were merely along for the curiosity. As a result, some newcomers take on roles not unlike the high schoolers and others quickly rise to leadership positions. It is among the newcomers where the individual projects are assigned. As undergraduates, their status as researchers is much more important than that of the high schoolers, and they are given the responsibility of maintaining their own projects, if they choose, alongside the larger research project. The diversity of interests among this group, archaeological and otherwise, is what characterizes many field schools, especially ones with volunteer populations. This particular school was not volunteer (save for the Kenyan labor at the site) and those admitted were granted funding through the National Science Foundation's Research Experience of Undergraduates or through other means. That said, the success of a field school depends, in part, on the dedication of these newcomers and not necessarily their skills.

The Kenyans. We hired a group of approximately 15 local Kenyan workers (some were part of the family living on the same land as Chemai I). Their labor was critical to our timely completion of the excavations. Most of the Kenyans attended the screens at one time or another, sifting through the excavated soil under the watch of the Mwalimu. A select few aided in excavation with basic troweling. Though I worked with a few of these workers on a regular basis, language barriers ultimately prevented me from inquiring about their perspective. Their

stories and interpretations could comprise an entirely separate work and I have chosen to exclude their presence for the majority of this paper.

The leaders and newcomers comprised about two-thirds of the population in the compound, about 10 of 15 in the 2008 season. I generalize here, but will later refer to individuals in specific when the need arises.

In the three different field schools I have been involved in this is an applicable model. Generally speaking there is a project leader and a close team of colleagues that surround them. Next come the student populations of various kinds and finally the volunteers. Individuals at various times cross these boundaries; newcomers sometimes emerge as dedicated and quick-learning leaders and some of the colleagues or graduate students mingle more closely with the other students.

Ultimately, in the descriptions that will follow it is the individual character and tendencies of the people interacting with objects and systems that is intriguing. Those are the idiosyncrasies that I wish to tease out in relation to archaeological research. I do, however, find it useful to be able to place these individuals in a specific time and place relative to this field school while learning of their individual roles. I have, however, singled out three individuals in specific because of their stable roles as “archaeologists.” In the cases of Chap and the Mwalimu, degrees and respect in the discipline define this. For the Grad Student, his commitment to acting out the research experience that defines an archaeologist cements his role. In contrast to this, while the general student population of the school (and I include myself in this category) may be no less dedicated to the project in question and archaeology in general, our lack of experience and permanent connection to the immediate research project place us on a different level.

A Theoretical Frame

The particular combination of theoretical perspectives I've discussed here are the result of my undergraduate emphasis on socio-cultural anthropology. However, my experience with Chap and his ideas of holism in anthropology showed me that socio-cultural anthropological and archaeological theory need not remain exclusive. Being at a site where archaeological and ethnographic investigation was staggered, if not concurrent, fully blurred theoretical lines. Thus, I am attempting here to borrow from the reflexive turn in socio-cultural anthropology and reflect on the processes in the field seasons I've outlined here.

The following portion will describe a series of snapshots in which the interactions of the characters (both their individual tendencies and their roles above) and the settings will come to the fore in the moments I captured on camera. These are an outgrowth of a photo-documentary eye focused on the discipline of archaeology; a series of stories through photos. They reveal the creative processes of meaning-making in archaeology as I have found them. From the drying screen to the shade behind Robai's house, these stories will expose the sites of knowledge production that I experienced during this particular research season. Each site will be excavated (in the sense of peeling back layers to expose the story of meaning held within) from an individual photograph, with a description of the numerous strata of meaning that make up my interpretation of the moment. I will show how something like washing artifacts, when seen through the viewfinder, becomes an entirely new element in archaeology; far more than a functional, utilitarian stage. Finally, in what I call the "photography metaphor," I will tie together the ideas of meaning-making explored in the post-processualist movement with the moments in time I've gathered here and a description of the physical workings of a camera.

Scene I: Washing

The Necessity of Washing

As I mentioned earlier, the particular constraints regarding the cultural material we dealt with in Kenya required that we take certain steps in the field that are normally reserved for labs. Washing is one of those steps. To maximize time, many archaeological projects will devote all the time in the field to collecting artifacts. Any catalogue is very preliminary and artifacts are sorted generally by provenience. However, because we were only able to bring back the diagnostic artifacts we were required to determine what was and wasn't diagnostic in the field. The nature of Kenyan ceramics is such that the decorations are often fine and shallowly impressed into the clay. Even ceramic sherds that have been placed in water and rinsed may still hide decoration. Thorough washing with toothbrushes was required to ensure that the artifacts we brought back to the United States were the "best" examples.

Ceramics were the predominant artifact type that emerged from Chemai I and II. Bones were also abundant and the situation was same with the bone. I worked more intimately with the ceramics in this field school, though I spent many hours washing all types of material. Anything that was not bone or ceramic (iron fragments, beads, other metals and lithics) was also brought back with the team. Iron, however could not undergo washing and beads and lithics were often separated out early because of their rarity at the site.

What made a "diagnostic" ceramic then was one of two qualities. Decoration is the easiest trait. The ceramic traditions of northwest Kenya are impressed decoration by an array of roulettes. A roulette can range from a piece of string to knotted fibers or craved pieces of wood. After washing, the specific design of roulette used can be identified on each sherd. The second diagnostic ceramic quality was whether one could identify the specific shapes, types and sizes of

the vessel from which it came. Rim, neck and base sherds are examples which, using certain formulas, can indicate the type of vessel (be it pot or bowl) and even the diameter of the mouth. A diagnostic sherd need not be decorated to be considered of import, though those decorated diagnostic sherds were treated as the gems of the collection where small undecorated rim sherds were catalogued almost begrudgingly as, “undecorated, rim, rounded lip, small, red in color.” Uttered in a rehearsed and repetitive monotone there was clear differentiation between the “prettiest” pieces and the lesser ones.

As seen in my description above, the actual material being washed quickly overshadows the actual process itself. Washing itself is a pretty self-explanatory process and takes no real skill to complete. It is largely disregarded as a means to an end, a necessary step to diagnose detail on ceramic sherds. Often it is relegated to menial laboratory work and not part of the contested experiences in archaeological field research. However, exploring a photo of washing as an archaeological site reveals something far more profound than a utilitarian (and often dirty) process.

Around the Washing Basin

It is a typical Thursday very much like the three or four Thursdays prior. After driving to the site at 8 in the morning, spending the morning and early afternoon scraping at gravelly, root-filled soils, we finally arrive back at the compound. The matatu ride is a brief respite and as the van parks in front of the house we are once again back to work unloading buckets of artifact bags, survey equipment and half-filled water jugs. Without stopping even to remove our dusty, sap-covered clothes, we move on to the next assigned task, washing artifacts. The quotas changed from day to day, sometimes only one bag per person, at other moments (when free time

was ample) we might set out to complete five bags a piece. That day was a standard 3 bags per person. To ease this task, we would often share the burden and share basins. Because proveniences could not be mixed in the basin; we would wash one bag at a time, meaning that a basin of two people would be responsible for 6 bags. We spent the waning hours of the afternoon completing the task. Some of the more savvy newcomers chose bags of ceramics (especially with large sherds) which took less time. Inevitably, different people would be responsible for the enormous bags of bones that numbered over 500 individual fragments with some fragments no bigger than a grain of rice.

On this particular Thursday is towards the end of our collection period. We are collecting fewer and fewer artifacts each day as the units go deeper into simpler occupations. As a result, we are slowly catching up with the washing, having nearly all the material washed one day before collecting new material on the next day. Slowly, individuals finish up their quotas and move on to other tasks like making dinner, or (more importantly) napping and showering. As we near the end of our quotas we realize that there are only five more bags left to catch up completely. A small group comes to this realization; a group consisting of the Grad Student, Ange, Chap and two newcomers. Coming to one additional bag per person they agree to work as an unprecedented team of five to complete the task quickly. A large wok is filled with water and the first bag of bones poured in. The sun is waning and setting behind the lab while a dedicated group of 5 plunge their hands into the now brown water and find their first fragments to wash. The largest come out first; complete portions of limb bones, ribs, joints and mandibles. Brushes work furiously to rid these of soil. On one occasion the Grad student uses the handle of the brush



to slowly rid the center of a femur of compacted soil. The result is comical in a scatological sense and the group gets a laugh.

It is not a silent scene. Laughter and music come from within the house where the others have retreated to unwind. Outside, the individuals mumble possible identifications of the fragments and others nod or comment in agreement. The mood is not solemn and to quell the monotony Ange and Chap share stories of previous excavations. Chap sometimes will launch into a discussion of the cultural history of the area and the newcomers listen intently to absorb as much as they can from this man. On this occasion, Chap launches into what seems like an academic-minded story which ends with him and his brothers stealing liquor from their father's room as children. He throws his head back and laughs his labored and raspy laugh while he spouts incomprehensible words in a heavy, laugh-laden accent.

Every once in a while, one of the washers comes across a rock. Given the poor materials available for stone tools on the mountain, lithic traditions are hardly as neat and pretty as they are in North America and the Rift Valley. It is difficult to tell rocks from lithics. This particular rock is passed to Chap who glances at it and tosses it off the porch without skipping a beat. The result is surprising for the student who passed the rock and expected an explanation or analysis.

They discuss the state of excavations and the progress of individual units. The brushes continually sound and eventually the bags of completed and drying artifacts pile up in the corner as there is no further room on the drying screen. Towards the end of the task, as the business side of conversation winds up, gossip is shared about those inside unwinding. One of the students washed only one bag and retreated quietly to his room. "Just let it go," says Ange, "at this point there's no reason to make a fuss about it." One of the returners refuses to wash with the group

and does a poor job by simply rinsing the pieces. “The Washing Machine Method” as it comes to be called is discussed some.

Such is the washing process as seen in this photograph. This is a unique situation including an unusual number of people and Chap himself. Normally, there would be no more than three people to a basin and there would be as many as 5 basins set up around the porch. The same themes of business, jokes, gossip and identification play out on smaller levels simultaneously during typical washing sessions.

Pruned Fingers, Cold Water, Tired Feet, and Monotony

What is usually characterized as a simple, utilitarian step is actually a complicated process in its own right. Robots could wash artifacts, but robots do not wash artifacts. People do. Friendships are made, explored and broken around washing basins. Personalities emerge when music is played and moments of dancing and singing erupt. Knowledge is shared between the returners, the Grad Student and the newcomers (in no particular hierarchical order mind you).

Through teaching and primary identification roles are played out through artifacts. A particular ceramic sherd becomes a teaching aid for the Grad Student to show how to identify knotted versus twisted roulettes. For my part, in displays of my familiarity with the material and knowledge about lithics in particular, I would engage in “porch chucking” (as the Grad Student so aptly referred to it) much as Chap had. This display cemented an amount of authority as I was taking direct action to exclude something from the archaeological record. Artifacts change hands a lot during washing, especially when interesting pieces are found. A mandible might show up in a complete condition that allows for a previous conversation to be recapitulated with new visual evidence.

Washing occupies a special place for me. Indeed, it emerged as a great source of tension during the 2008 season. Despite the funny and illuminating moments, washing remains a monotonous and draining task. Pruned fingers, cold water, tired feet and bored minds do not an enjoyable task make.

Now, towards the end of excavation in 2008 we were not only washing but also cataloging the material. The catalogue was a series of brief descriptions of diagnostic ceramic sherds and counts and weights of the undiagnostic pieces. This task was such that it required a fair amount of knowledge to identify different decoration types. It also became very much like an assembly line moving from bag of unsorted artifacts to Excel spreadsheet. The Grad Student headed up this project and kept two others, myself and one of the newcomers (ascended to leader). Instead of washing after excavation, we would retreat to the confines of an upstairs room of the house to catalogue.

The result was resentment that we three were not completing our end of the washing bargain. Here, hierarchies (and randomly assigned tasks) are played out as those with less knowledge complete more menial tasks. Not to say that the cataloging task was privileged one (indeed it was just as monotonous and draining as washing), but it was selective and excused us from washing. The artifacts become the material manifestation of a despised chore in this sense, whereas the washed ones carry with them a privilege played out through special and physically (as we catalogued on the second floor) elevated tasks.

Washing as a utilitarian task, then, is only so simple on the surface. An archaeological report might not address it as a step. This “black boxing” avoids the rather complicated unveiling above and dismisses the ideas and roles and dramas played out during this process. It is easy to see how the most important part of this task is that the artifacts were washed and sorted within

the allotted time. But opening this black box of washing reveals something of a more profound *experience* that produces these cleansed artifacts. An experience versus a step or task implies a more complicated situation that is not a simple in and out process. Taking account of this experience requires that archaeologists critically reflect on what factors produced data.

A Hierarchical Anomaly

To clarify the nature of the hierarchical categories I must elaborate on an individual who I find anomalous in this schema. Ange was a returning member from the 2007 season. She has worked with Chap closely at the museum and is pursuing a career in archaeology, though she has not advanced past a bachelor's degree. Most anomalous of Ange is that she is older than Chap and has children that are older than many of the general students. Her role in the compound was very motherly; she maintained order within the dramatic disputes among the students and did her best to care for the ill and ensure that the folly of the youthful mind did not go unchecked. Many of the students respected her in this sense and this respect carried over to the archaeological side of things. Ange was excluded from actual excavation and took charge of "artifact central" and carrying out the primary processing of the artifacts. Despite her lack of the same academic standing that Chap and the Mwalimu have, she was very much in their tier during decision making. While Ange has the traits of my definition of a leader she moves in the circle surrounding the project director. In addition her relationship with the general students and the high schoolers in particular is very unlike that of what would be acceptable for Chap or the Mwalimu (though Chap made it no secret that he cared a lot about our experience).

Ange's very presence at the compound created tension at times and exposed the fabric of the hierarchical model that characterizes field school populations. To see her move through

multiple roles exposes the roles themselves in ways that would otherwise go unexplored. If we were to specifically follow Ange's role in different stages of the archaeological process, we would expose tension within roles that have a very strong hand in the creation of archaeological knowledge.

Scene II: The Excavation Unit

Systematic Excavation

The archaeology site is supposed to be the systematized period of collection (or experimentation) in the research process. As will be discussed later in this section, an otherwise unassuming or natural piece of mountain side is objectified with coordinates and squares that ensure the replicability of the work.

Actual excavation is a "technique" that can be taught. This is what field schools are about. The tools of the trade are trowels and levels and shovels. One has to learn how to trowel, to keep one's floors and wall flat and intact, to not dig too deep, and to identify subtle changes in soil color that represent features.

In Kenya, sampling was arbitrary and based on the director's interpretation of the uses of caves. We excavated two by two meter squares in 10cm arbitrary levels, screening through ¼ inch mesh. Excavators took notes throughout excavation of the levels and filled out paperwork after each level was complete. Unit forms included descriptions of artifact material and densities, soil descriptions and lists of associated artifact bags. Almost no artifacts were mapped *in situ* but, the units were mapped in plan after every 10cm level. Units were excavated until bedrock was reached and units were photographed and mapped in profile after excavation was complete. Soil

samples were taken from the profile walls during mapping, but no Munsell colors were described in the field.

A description of excavation in this technical sense reveals the methodological choices of the head archaeologist and gives other scholars grounds for evaluating the quality of an archaeologist's work. If another archaeologist was to revisit the site they might follow the example of previous work or change it to accord with modern standards.

Often, in field reports, descriptions of excavation are the only mention of the actual collection process. Illusions are made regarding how sites were selected and sampled and how material was catalogued. Other than that, the sites are often discussed only in ecological senses and in terms of previous archaeological work at the same site region³.

The more thorough an archaeologist is, the longer the methodological section of their report will be. Yet little more is said than what I have described above from Kenya. The elements of excavation might be described in great detail and more jargon used, but the point is to show that excavations were as objective as possible; to show that artifacts can be associated with a regimented collection process, that one can work backwards and place the discovery beneath the earth, in time and elude to significance.

However, the act that I call the "handoff" reveals a different picture of excavation, one not so concerned with systematics, but with strata of knowledge and description.

The Handoff

Around 11am each day at the site tedium began to run high across all the units. Our excitement with continuing excavations had worn off, or worse yet, our dread of continuing had

³ Archaeology is slowly changing this respect. Hodder's work in Turkey is but one example of how archaeologists are already starting to consider the site experience in more comprehensive ways.

reached its peak. What's more, lunch was looming ever so distant on the horizon. We could almost taste the fresh, juicy pineapple which made the final few centimeters of the level last forever. In stark contrast, towards the end of the day, the oncoming storms never left enough time to finish properly.

At any particular moment, discoveries of unusual things were rare. A few ground-stone tools had emerged up near the cave mouth and a fair amount of iron as well. Most of the excavation consisted of troweling through rocky, tightly-compacted soils with layers of nothing or too much bone to count.

I worked alone for the most part but, when we cleared out the massive fig tree roots that crawled through the floor there was space for another. Paris was the name of the woman who helped me. She spoke English well, but we spent most time digging and not talking.

The unit next to me was another matter all together. The two students working there talked and talked about everything from boys to the towns that their schools were in. Looking to the east I could see heads poking up from time to time and buckets, overflowing with soil being passed to workers, who would in turn send empty buckets in.

Chap and the Mwalimu wandered about the site poking their heads in from time to time. Chap brought with him a video camera and would conduct brief interviews with the chief excavators about progress and discoveries. These two were the only ones to move outside of the rhythm of the site. The scraping of trowels, the soil falling through the mesh of the screen, the sounds of pages being turned and charcoal being wrapped in foil created a rhythmic element to excavation. Conversations came and went, with exchanges between the units few and far between. On some sites music is played to break the monotony of the rhythm or to contribute to it. In my unit I would find Paris humming tunes occasionally.



On occasion, though, the rhythm was broken with discoveries of relatively high magnitude. On one occasion early on in excavations I came across an oddly shaped rock. Instead of a cracked and flaked, rough surface this one was smooth to the touch and bulbous. As I lifted it up I noticed its density and presumed it to be iron slag, a leftover from the smelting process. I had seen such slag before, but the piece I had found was well over an inch in diameter. I showed it to Paris and when I said iron she immediately recognized it and told me how she had seen iron made. I showed it to my fellow students to the west, one of which was particularly impressed by its girth. I caught Chap's attention and showed him the piece. He went into detail of the smelting process and the different types of slag. He wondered if we might be near to the place where all the iron from this site was processed.

Other students from the other units were drawn in as Chap spoke. The misshapen hunk of iron was passed around from student to student, worker to worker. Eventually it landed in the hands of Mwalimu who brought it off site to Ange to be placed with the other artifacts from the same level.

Scenes like this occurred for every unit. Sometimes the object in question was an artifact, other times it was a bone. I remember especially well two instances. One was a large femur fragment, of a size unprecedented for the field school. One of the returning students, a graduate student familiar with the ecology of the area, described it as buffalo. On another occasion a large hammer stone was found. This stone was, at its broadest end, almost too large to fit in the palm of my hand. It tapered to a blunt point where tell-tale fractures revealed how the tool was used.

I remember vividly the cool feel of the stone and, as I brushed more and more soil off of it, the smoothness of the side that rested in the hand. It was almost blue in color and heavy in the hand. This tactile experience made the object more real to me as I could feel how the shape of

the stone fit fairly perfectly with the anatomy of the human hand. This tactile sharing experience is the handoff. In the picture above a nondescript pot sherd is passed from the excavator who found it to the Graduate Student who might initially describe it.

The handoff is a break in the rhythm where rules of space and work are violated for moments of learning and tactile experience. These moments, these handoffs, do well to illuminate the less systematized elements of the research process occurring on the site itself.

Touch and Archaeologists

For the most part, excavation gets into a rut or rhythm. I did my best to illustrate the monotony and type of work going on at the site and how it fits into the day. Interruptions like the handoff might easily be dismissed as unimportant, just like conversation within the pits and the brief exchanges and interviews with Chap.

However, I think that the handoff shows the active learning process that is going on during excavation. It also shows that description and conclusions are drawn up and revised throughout the research process. Indeed, the handoff only occurs if there is information being attributed to an object or coming as a result of the discovery. The majority of artifacts are casually examined and thrown quietly into a bag or bucket, if they don't remain concealed by soil and head to the screen.

Ad hoc conclusions are constant throughout the process. We examine possible artifacts to determine if they really *might* be of interest. I emphasize "might" because there is a consensus that it is better to be safe rather than sorry. It is better to assume that something is an artifact and let the truth be sorted out in a lab after cleaning rather than being too hasty in the field.

Further, the tactile experience of artifacts is drastically important to learning and archaeological identity. Since there is significant importance given to the *stuff*, the ability to reach out and touch is seized. We can feel the smoothness of grinding stones and the sharpness of thousand-year-old blades. We can learn to feel the different weights of stone versus iron and the natural and unnatural contours of bone.

Often, when excavators found noteworthy pieces they were congratulated with “nice job” and “good find.” Here is the first place where a student may feel like an archaeologist; upon their first success of contributing in a meaningful and recognized way to the archaeological record. A sense of pride might even be associated with certain artifacts. We might recount the recent history of discovering the piece in question. The story almost always involves something that caught an eye briefly, something that glimmered in the sun or a sense of unfinished business that caused an excavator to revisit a particular corner. It is through the handoff that the excavation process is romanticized with relation to certain uncommon artifacts.

Moments like the handoff are brief. They might easily be misconstrued as merely impediments to the efficiency of the excavation. Most certainly they are not considered, unless in telling the tale of a truly magnificent piece, after the fact.

Yet this is a break with the understood methodology. Nowhere does it state that, upon finding a piece of note, that the archaeologist shall be called over to give praise, that students shall gather ‘round the magnificent unit and gaze upon the fine work of the excavator. Nowhere does it say that identificational meanings that are given to the artifacts in those moments shall come to bear significantly on the conclusions and typologies drawn from the site. There is no rubric, no objective process that I know of which accounts for the situations surrounding the handoff.

Just as politics and social relations come into play around the wash basin, so do they come into play during the handoff. It is Chap or the Graduate Student who get to describe the piece and tell its significance. The moment of finding and handing-off might even cement an individual's position as being experienced and revered during the field school. Other students, upon seeing the piece being passed around might recall something similar they saw but did not identify. Tensions and jealousies come into play here, alongside pride and success.

These moments dot the excavation process; through handoffs and breaks meanings are explored actively and tactilely during the collection process. Technical and methodological description cannot do these moments justice. Indeed, they purposefully exclude moments like the handoff as unimportant to the larger cultural and archaeological significance of the research.

It cannot be ignored, however, that through touching and learning and gathering on the site that archaeological identity is born and cultivated. The handoff, though fleeting, nonetheless reveals the very subjective play occurring throughout the collection process that affects the excavators (in how they might feel about and approach their work) as well as the artifacts themselves (in how meanings determined here tend to stick with artifacts).

Scene III: The Drying Screen

Origins of the Screen

In the 2007 season we were not as laborious with our responsibilities after excavation. The last week of the field project was a frantic race to wash and account for all the objects we had collected. Washing became a nightmare and problems abounded. Chickens were turning bags of faunal material into nests and, worse, pecking at the bones. Humid climes made the drying process slow and daily rainstorms drenched the slowly drying objects. Artifacts would not

dry in the bags and, when placed atop their corresponding bags, inevitably became mixed and lost.

Of great concern in the 2008 season was to address these logistic problems right away. Soon after excavation began, Chap had a carpenter build a large screen for drying. The screen was elevated some four and a half feet off the ground; safe from the dogs and fowl. It had 18 separated compartments so that bags could be contained. Finally, when staring a thunderstorm in the face we would water-proof the screen with an ad hoc roof of grimy tarps and discarded rocks. The tarp was lifted to expose the wet objects to sun and applied overnight or during storms.

This awkward wooden screen resided just off the front porch of the main house, near the lab. Washing often took place on the porch, but was centered around the drying screen. After pieces were washed and roughly sorted, they would be counted and laid on the screen to dry. Within the individual compartments on the screen, artifacts would be further separated between bones, rocks and ceramics with improvised barriers of sticks and leftover sugar cane. The corresponding bag would lie on the screen, often with drying treasures atop it, so that each compartment could be easily associated with a bag. Sometimes, single levels produced dizzying quantities of both bone and ceramic material which might fill an entire compartment. This is to say that there were occasional obstacles to overcome which left some space for contamination and confusion.

Overall, this screen was a great achievement and greatly contributed to the success and smooth running of the 2008 season. However, its utilitarian purpose overshadowed our reverence towards this creation. Indeed, this space was of little importance for many participants and I doubt it has retained any significance in the research of the Grad Student. Still, as with the other

instantaneous and utilitarian moments discussed here, the screen was grounds for contention and vital to the play of politics among all involved at the site.

Orderly Disorder on the Screen

Though Ange was very involved in washing, through some unspoken general consensus, she became responsible for the coming and going of artifacts on the drying screen. She kept a special notebook full of various checklists and catalogues of bags collected at the site, unwashed bags, washed bags, uncatalogued bags, and catalogued bags. Through this notebook, later in the season, we could track individual bags and find at what point they “disappeared” and often track them down.

We often raced the daily rainstorms at the site; frantically scribbling notes, grabbing supplies and, quite literally, running to the car as it began driving away. (Wet mountain roads meant a stranded archaeology team.) But, just as often, we raced the rain after excavation and during washing. These times were less frantic but nonetheless stressful.

On one such occasion we could see the rain clouds coming, but they were unusually slow. They were just slow enough that we could continue work and comfortably estimate how much longer until we would have to cover the screen. Washing proceeded as normal, each pair or trio slowly working through their assigned number of bags. Counting work was split up, but recorded in the same notebook. Then artifacts were brought the screen where Ange would check them in and assign a new bag to be washed.

Most of us were washing on the porch, though one of the more reclusive returners had hid away in the dim lab. The Grad Student was off working with some of the washed bags starting the catalogue. The high schoolers were inside the house doing some preliminary and

“dirty” sorting of the unwashed material to ensure that no iron or special artifacts were washed. As the late afternoon wore on tensions rose as the politics of washing played out. Some were just getting cranky.

It all started with one question, Ange asked one of the pairs what bag they were washing. They said that they were washing bag F5 05-02. Ange marked it down, but one of the girls realized she had mistakenly looked at the wrong bag. They were actually working on bag G6 05-01. Where was bag F5 05-02? The bag was on the ground near the basin, but its contents were on the screen. The offending pair and Ange then worked to sort out where the bag should be placed; it was a fairly easy task of finding a bagless compartment.

While they searched for the separated material, they noticed that some writing had come off one of the bags. Ange called for one of the high schoolers to come out and remark some of the bags that have faded. The high schooler grabbed a marker and some new bags and got to work.

During the brief search, my basin and another had finished with one bag; we had already counted and were holding these important numbers in our heads. With Ange’s attention once again for the taking, we assailed her with a series of bag numbers and artifact counts. She furiously flipped through the pages of her notebook to keep track, telling us to slow down more than once. We then brought our washed artifacts to an empty portion on the screen. While pouring the cleansed material we spilled a few pieces into a neighboring compartment. We ask Ange to help us sort out our material from the other compartment.

While we sorted it out, another group finished and took Ange’s attention away to record those numbers. They also asked if they had to do any more bags. The storm was drawing nearer and they couldn’t possibly finish another before the rain, unless it was a small bag of ceramics.



As Ange turned back to decontaminating, the Mwalimu walked about and began some informal sorting. He tossed plain rocks and sorted out pottery-like rocks from the actual pottery. While doing this he also examined some of the more elaborately decorated ceramics and took the opportunity to show the high schooler about the decoration types.

My group finished sorting our spill and moved on to another bag. Ange then noticed that the Mwalimu had mixed up some of the compartments and put the piece he had use to teach the high schooler in the wrong compartment. As she explained how the screen is organized to the Mwalimu, the Grad Student came down from cataloguing and asked if she would help him find bag F9 02-01. Ange agreed, but simply handed the notebook to the Grad Student to sort through himself. She continued to work with the Mwalimu. During this time, the returner that had secluded himself in the lab had placed more artifacts on the screen. As he walked away he told Ange that he placed a bag on the screen. She asked which one and whether he counted it.

As the returner begrudgingly rambled off a series of numbers Ange raised her voice and told him to wait. She could not find her notebook. The Grad Student had wandered off with it.

Ange threw her head in her hands and moaned. My group had finished another bag and had counts to report. The other high schooler came outside and wondered why he was the only one working inside. The cook yelled that it was almost time for dinner. The high schooler who was re-marking bags finished and the Mwalimu was making jokes. Ange, increasingly frustrated still was without her notebook and told us to stop telling her numbers. She called for the Grad Student to return the notebook. He still was unable find bag F9 02-01. The reclusive returner came out yet again and placed another bag of artifacts, unaccounted for, on the screen.

A bright flash stunned all the parties involved and with the crash of thunder began the downpour.

The House of Cards: Imposition of Order

The account above is a dramatized version of several similar moments. Unfortunately, Ange often bore the brunt of this type of confusion and frantic episodes. And, it was often that we washed and worked until the rain poured down.

This story, though a complex and confusing mix of events, is still simplified in many senses. It does not take account of the individual conversations during this time, conversations like that of the washing basin only add to this madness.

I find it ironic that this space was often the source of so much tension and confusion. Indeed, we developed the screen for the purposes of easing the confusion we experienced in the 2007 season. In a perfunctory sense, this was accomplished. Imagine, for a moment, this scene playing out with bags strewn about on the ground outside the lab, the ground inside the lab, the tables of the lab and the porch.

In 2008 we upped the organization and created an object through which we could work efficiently. However, this drying screen only made this super-organized chaos all the more distinct. On the drying screen we have individual compartments; within those compartments are individual bags which represent individual levels of individual units of excavation. Those bags are then sorted between bone, ceramic and other material and counted. The notebook accompanies the screen as the master list accounting for all the material collected on a day to day basis. All this control creates a house of cards, a series of catalogues and organizations stacked

carefully atop one another on the brink of collapse. All it takes is one breath or one foreign element to through the scene into panic.

The house of cards analogy goes further. Scenes created around the screen and the structures of catalogues that create the potential chaos are unique and change from day to day. As each set of 18 compartments is recycled, the elements and contributions change the structure. Unique errors occur from different individuals and teams that force the order on the screen to be tweaked and altered so that it makes sense.

As more and more order is imposed upon the artifacts – which emerge from the ground in irregular and unique forms – the structure of the card house is built higher and higher, ever more fragile. We must not forget, too, that it is not only the systems of organization that contribute to the structure of the card house. Indeed, individuals and the relationships between them contribute to it. Though Ange was usually heading the screen operation, both the Grad Student and I filled in from time to time. Our individual contributions alter both the form of the organization but also alter the interactions between ourselves and the other groups reporting bag numbers and counts. If someone preferred not to deal with me directly, they might pass their information along, like a game of telephone, through which it could get misconstrued. Others might be eager to interact informally with the Grad Student, in his less involved position of accounting for bags, and work in haste.

Though the creation and use of the screen were founded in perfunctory and utilitarian senses, the actual playing out of the screen is not so easily synthesized and understood. What emerges is a unique and fleeting situation of cataloguing and ad hoc organization that is in continual flux with a variety of logistic and personal issues. Though artifacts are left idle to dry on this screen, the emotions and interactions that surround them reach a height the likes of which

is not seen anywhere else in the field school. This super-organized chaos, this contradictory orderly disorder, is spontaneous and, unless documented at the time, utterly lost into a series of memories.

Perhaps even more ironic is the fact that the only thing that brings relative peace to the artifacts above is a powerful thunderstorm.

Scene IV: A Little Room on the Second Floor

The Catalogue

Once we had accumulated a fair amount of artifact material, and had washed and sorted it, we began the task of creating a catalogue of all the ceramic, artifact and bone pieces amassed. I was not involved much with the bone material, but I did spend a large amount of time with the ceramics. When I speak of the catalogue I am referring to the ceramics catalogue.

Conception of the catalogue began before excavation. One night the Grad Student, Ange, and a few of the returners sat down and thought long and hard about how we would organize the material we collected. When we bagged artifacts on the site, we would need some way of organizing and keeping track of the bags so that we could put the artifacts in an understandable order. The bag numbering system included site number, unit number, level number, and another number indicating if the bag was part of a series. For example, the first bag of my unit was labeled as follows: CII F8 01-01. CII refers to the site Chemai II. Unit numbers were determined by their x and y location on the grid. With the southwest corner of the grid representing the origin; we counted numbers going east and letters going north. As I said above, units were excavated in 10cm arbitrary levels, so level one is from 0-10cm below the surface, level two 10-20cm and so on. The final number became a problem. Depending on how artifacts were sorted in

the field and who made the bags and remade bags (as artifacts were sometimes sorted again during washing) we found we had no way of telling how many bags might be included for an individual level.

After material had been washed, the bags were placed in the lab and ordered. Each unit had a different section of the lab and the bags were placed neatly on the tables going from shallowest level to the deepest in a line. We had two lab spaces; one devoted to ordered bags of ceramics and the other bones. From these rank and file storage spaces we would grab series of bags and begin the cataloging process.

The Grad Student headed up the catalogue and recruited a team of others. At first, I was merely photographing the diagnostic pieces after they were catalogued, but soon I was part of a team of three including Jane, one of the newcomers who had shown a dedication and competency that separated her from the other rookies.

As I have discussed in brief above, we were responsible for determining the diagnostic properties of the massive ceramic collections. Through brief examinations and description we determined which pieces would be valuable to bring back to Chicago for further examination and inclusion in the research at hand.

We would begin by dumping a bag of washed ceramics on the table. From there we would sort out the decorated and diagnostic pieces from the other undecorated body sherds. I would help sort and then count the mass of uninteresting body sherds. Jane would go on to describe the types of decorations exhibited and the primary conclusions from the diagnostic pieces. We would then count the amount of decorated pieces, diagnostic pieces. For instance, we might have 2 twisted roulettes, one carved and 3 knotted roulettes totaling 6 decorated pieces. Then, we might have 2 rims (1 with decoration) and a handle fragment for a total of 3 diagnostic

pieces. As Jane rambled off these counts and descriptions, The Grad Student would enter the information into a spreadsheet on the computer. Entries were made for each individual bag of each unit. Another catalogue was created for longer descriptions of the diagnostic pieces.

After we had counted the pieces, Jane would begin to take measurements of the thickness and decoration motifs of the diagnostic pieces. Each one would then be given a number based on the site it came from and simple sequence; CII001, CII002, CII003 and so on. Jane would, after measuring, mark the actual pieces with a permanent marker. I would apply a coat of clear nail polish over this number so that it would not rub off. I had also, during the measuring and description, created bags for the decorated undiagnostic and diagnostic pieces. These bags would have the same numbers as the larger level bag, but with specific titles and, in the case of the diagnostic bags, a list of the catalogued pieces held within.

We would then move on to the next bag and so on until we had grown weary, stir-crazy or the power went out. It is true, though, that we spent some time on the edge of sleep, giggling incessantly, cataloging by flashlight in order to get the thing done.

All told we spent nearly three weeks cataloguing with a week and a half devoted solely to the catalogue. We saw nearly 20,000 individual ceramic sherds from both sites twice (we went through the entirety of the collection again before we left to make sure nothing was left). We retained several thousand decorated pieces and individually described, numbered and photographed nearly 500 diagnostic sherds.

Yet, though this process is so easy to describe in a practical sense (as with the moments described above) the experience is not so simple.

The Assembly Line

We called ourselves the Triceratops Crew. The name was derived from the close relationship between the three of us and the naming of the cat that lived in the compound.

Each day, as excavations were wrapping up, we triceratops would grab our buckets and sharpies and bags of artifacts and retreat to the upper floor of the main house. There, at the end of a hallway of bedrooms was a small room with one table and three chairs. The room led out to one of the balconies on the main house and overlooked the courtyard to the front of the structure.



The room was yellow with a blue, metal door and a window (barred with blue-painted iron). The table, for the most part was covered in various notebooks, out-dated electronic equipment, empty bags and dust. Here, we would set our set of bags in order, plug in the computer and, after turning on our favorite Credence Clearwater Revival album, begin.

I dumped out a bag and the ceramic sherds spilled across the table. Jane and I would quickly begin fingering through the pile, our eyes looking carefully for hints of decorations, strange fractures and rounded lips. The large rims and handle fragments would stand out like sore thumbs, but occasionally we would come across a piece not 2 cm across with just an edge of decoration on it.

Without missing a beat, we began counting and present our completed number. Jane would then spout off the relevant information. Her dry cadence indicated the routine that we had fallen into. We were never routine, however. Moments of ridiculous and random jokes would find us exploding into laughter that carried throughout the house. The Grad Student, from time to time, would speak to the different pieces; sometimes complimenting the big, decorated pieces or consoling the forgotten, plain, and tiny rim sherds.

As we moved through this routine and rhythmic system, I would sometimes come across missing bags. The Grad Student would sometimes shrug it off, sometimes grab his head or, on occasion decide that, clearly, the historic individuals at the site decided not to deposit anything on that particular square for that period of time. Regardless of his initial reaction, we would soon call Ange to examine her notebook and decide if the bag was really missing or where it was last spotted.

We continued, thinking it better to get through what we could before the daily storm knocked out the power. Soon, one of the high schoolers popped her head in. She wondered where we three and Ange had got off to. She wondered if we had any work for her. We passed off a few menial tasks on her and even taught her to identify the different decoration types. Still, we had a particular rhythm which enabled efficiency and security in our production. Ange understood that we needed to get this done and found another job for the high schooler.

The Mwalimu, having woken from his nap, came in and examined the pile of unsorted sherds on the table. He threw a few out the window and moved the pieces that we had sorted around. His actions, though extremely helpful, still became the grounds for our inside jokes. He, as with the high schooler, upset our system and rhythm.

As we finished our initial batch we decide we could get a few more bags done before dinner was ready. I gathered up the finished bags and headed down to the lab to fetch another set. As I went, I passed the porch where most everyone else was washing. It was deathly quiet and I could have sworn I saw a few evil eyes directed toward me. They had heard our laughter and wondered what all the fun was. Most importantly, they wondered why we three were not down there washing.

As I came back with the new bags, I ran into the Mwalimu. He joked with me about the Grad Student's catalogue and his knowledge of ceramics. I laugh courteously, but cannot interpret the degree of sincerity in his soft voice. The relationship between the two is a complex one of support, instruction and ridicule.

When I came back to the yellow room, the Grad Student and Jane were sorting out a few problems with the numbering system. The Grad Student was slightly frustrated, not with us, he said, but with himself and with the excavations. He sent me back down to retrieve one of the completed bags so we could recount and possibly renumber.

When I went back down, the storm had forced all the washers inside. They asked what I was doing going back and forth. I explain the situation and a few ask if they can help upstairs or maybe catalogue next time. I said that we would love to teach some new people and have some help tomorrow, but that tonight we were almost done.

Finally, when I returned with the contentious bag, Jane and the Grad Student had begun going through the other material. We set the contentious bag aside and got down to business, making a note to have another look at the old bag. The smells of dinner finally floated to our end of the house and before we could even finish one bag our growling stomachs were greeted with the call for dinner.

Respect

The catalogue combines aspects from the wash basin and the drying screen. Not only do politics and gossip surround the task, it is another place where order is imposed on artifacts.

The separate physical location of cataloguing gets at a more specific aspect of the politics within field school. While the wash basin demonstrates how they are informally negotiated in the daily regimented tasks of the project, the catalogue reveals a more hierarchical order. I hinted at it earlier as I outlined the cast of characters involved. While the status of Chap and the Mwalimu were never questioned, we were left to sort out the rest of the roles for ourselves.

Conversations throughout the field school allowed for us to state our levels of education and amount of field experience. Generally, then, the graduate students were given higher levels of respect initially. Quickly, though, it became clear that this was not ascribed, but rather achieved respect. The graduate students worked alongside the undergraduates and built informal relationships with them. As such, they were subject to similar kinds of gossip, ridicule and respect-negotiation as are the general undergraduates.

By respect-negotiation I refer to informal evaluations of the competency and social standing of individuals. Things like arrogance, pride, reclusiveness that were demonstrated in places like around the dinner table come to bear in these evaluations. For instance, we might

have considered whether an individual took too many of the delicious japati (a type of fried, buttery Kenyan flatbread) or if they complied with the dishwashing schedule we created. Breaches of these unspoken rules would mean a loss of respect in the more professional contexts like excavations. All this is to say that there was not such a clear divide from the professional and personal archaeologist or student.

These social roles and social respects were tentative and changed throughout the season. In addition, these were not the only types of roles in the field school. There were also the roles decided in relation to Chap and the Mwalimu. Though personality and general likeability figured into these roles, abilities figured more prominently. Responsibility and trust were important in these types of roles. Certain individuals were given leadership positions at the site based on experience; these positions were either overseeing a unit of excavation or being responsible for teaching the inexperienced students. Similarly, something like the catalogue could only be trusted to more responsible students. And, as discussed briefly above, there was a need to be familiar with the ceramic types and archaeology of the area.

Thus, though our selection for the catalogue was based on an evaluation made by Chap (based on experience and our good standing with him) were still subject to the informal hierarchy established among the newcomers and leaders. This explains the tensions that arose when our cataloguing duties overshadowed our washing duties. It might have been jealousy on the part of the others who understood our jobs as a relief from the tedious washing duties. It might also have been a response to the camaraderie that we built whilst cataloging. Being part of the “Triceratops Crew” meant being in on many inside jokes and becoming a nearly inseparable trio.

We catalogued both in that yellow upstairs room and one of the bandas in the compound. Thus, on certain occasions we were very near to, and on the same level as, the washers. On other days we were physically above them in the house (away from the cold and rain or sun). Though our physical place changed, our evaluation by the students and Chap were not exclusive. We moved simultaneously through different social and political conditions; cataloguing only highlights two of the basic conditions.

Finally, our selection by Chap and the Mwalimu influenced our evaluation by the rest of the students. I believe that this evaluation, which was often negative because of our exemption from washing, only enhanced our clique. This close bond ultimately influenced our catalogue. We could work seamlessly and efficiently through the ceramics. It led us to keep the same three cataloguers, partly because we enjoyed being together and partly because we functioned well. Changes in this clique and our evaluation by Chap and the students would have led to a change in the catalogue. This change could have been shallow (as in different, or more legible, writing on the bags) or deeper (as more pieces may have been overlooked if we brought new eyes to the table during each session).

The Case of Jimmy Hoffa and the No-Talent Potters

I offer one further note on cataloguing. While The Graduate Student, Jane and I went through all the ceramic pieces we came across a rather unusual piece. It was clearly ceramic but exhibited traits unlike anything we'd seen in over 5,000 pieces. It had fractured in the shape of a circle. It was curved, as if it had come from the neck of a vessel. It was decorated with a knotted roulette pattern on the concave side. Finally, and most unusual, it had a small hole punctured square in the middle.

This piece defied all the classifications that we had seen. Its shape also defied the knowledge we had of ceramics. The only rounded pieces we observed were either carefully smoothed and shaped rims or bulky handles. Most ceramics fracture without a discernable pattern. At times these particular pieces fracture along the coils, or the flattened vestiges of the snake-like coils, wrapped around and around, which line the body of the pot. These pieces are long and rectangular in shape. Base sherds fracture in rounded manners but are also massive and thick. These sherds are also warped, but balance when placed on a flat surface just as the rounded pots would balance if they were not so top-heavy.

The unusual piece we found did not fit in with any of these parameters and, because the Mwalimu was away, we decided to set it aside and wait for his decision on it. We made no entry for it in the catalogue, but made an informal note. We also had some concern that we might not be able to find it again if we did nothing to separate it out. As I prepared a bag for the piece we joked about it. We thought maybe the potters had made it just to confuse us. We also joked about the likelihood that, when the Mwalimu returned, we would be unable to find the piece and come off as foolish. We named it Jimmy Hoffa after the famous union leader who disappeared and whose final resting place is the stuff of urban legend. "Jimmy Hoffa," was written on its bag and we moved on.

A few bags later we discovered another piece of similar description, though it was larger overall. We named it Papa Hoffa. When we consulted the Mwalimu he described it as decorated neck sherd, based on the curvature of the piece. The curve does fit with neck pieces but the round shape of the piece and the hole in the center remained unexplained.

Fears Lived Through the Catalogue

Jimmy Hoffa demonstrates the personification of artifacts and the relationships that archaeologists have with them. I briefly described above that at times the Grad Student would talk to the pieces of ceramic. A similar behavior is the way we describe ceramic vessels. They have necks and lips and shoulders and bodies. Artifacts are given human characteristics and personalities as archaeologists work with them. Jimmy Hoffa is this behavior writ large and, well, comically.

In the case of Jimmy I think we were expressing our fear, distrust and experience with the classification system we had developed. We had seen pieces go missing. (I can remember several times of noticing unique artifacts while washing and then never coming across them again in the field school.) We had seen how bags had been lost and found and that, without constant note-taking and organization, it was almost impossible to keep track of all these ceramic sherds. That archaeologists live on occasion out their frustrations with their personal and professional experiences through artifacts is significant.

Living through artifacts also has another intriguing effect. From time to time, during cataloging, we came across particularly ghastly pieces of pottery. Extensively decorated pieces of complete rim sherds were uncommon. These pieces would be considered beautiful and fine examples that would fit well as illustrations in research. The majority of the ceramics were randomly fractured, unadorned body sherds. The ghastly ones were body sherds with malformations on them, due to mistakes in craftsmanship or processes in the ground. One body sherd had what can only be described as tumors covering both sides. Another piece had lost both the smoothed inside and outside surfaces and was little more than a squarish mass of clay and temper. Still other ghastly pieces were decorated, but with mistakes like pressing too hard or not

keeping a straight line. When we came across pieces like this we would joke about the talent of the potters. On brutal occasions we questioned the mental capabilities and stabilities of the potters, suggesting that they might have been the outcasts or lowest achieving workers.

I believe that in the sense of the no-talent potters, archaeologists are working out their relationship with the past. Artifacts become vessels that not only allow us to explore the past, but also the people of the past. Using artifacts we can tell stories and the individual characteristics and quirks of the people. These stories, though, are inseparable from the archaeologist's own experience. Not all archaeologists would have interpreted the malformed pieces as the result of poor potting skill; perhaps allergies caused a sneeze or someone interrupted. There are untold interpretations.

These reactions are significant because they do affect the interpretations and scholarly work. Initially, I considered dismissing these incidents as jokes and meaningless moments that occurred in a state near exhaustion. However, these emotional and personal reactions could cause particular pieces and stories to come up more frequently. Indeed, Jimmy Hoffa remains as one of the strongest memories I have of cataloguing. A researcher might choose pieces to include in images within a paper based not only on the beauty of the piece, but also on the memories that he or she had of all the different pieces. I remember a particularly striking rim sherd that represented nearly 40% of the rim, showed a significant amount of neck as well as fine decoration. So some memories are based on characteristics of the piece and others based on stories and experiences lived through the pieces.

That archaeologists live experiences through artifacts is seen in the identity of archaeologists. It is fieldwork and hands on experience with excavation or analysis of collections that define archaeologists in the scholarly community. I think that the artifact is more than just an

object involved here, it is as critical to the identity of the archaeologist as the archaeologist is to the artifact. Artifacts are great visual aids for explaining our discipline to others. Rare and exciting finds might become the sole focus of an archaeological career. Through Jimmy Hoffa and the no-talent potters, I observed a group of aspiring archaeologists building personal relationships with the artifacts in which they explored and criticized their own work (with the system of organization) but also their interpretations of the past. We make them familiar to our human condition and our professional condition. Often, though, this most personal imposition of order and familiarity is dismissed. Artifacts are merely moving through systematic processes so that they can be analyzed with ease back in the scholarly real world. However, far from being objects awaiting study, in cases like cataloguing artifacts become dynamic canvasses reflecting and absorbing many different personal and professional relationships and idiosyncrasies.

Scene V: Chemai I

Finding and Building a Site

Chap and Sibel had been working in Kenya for a long time before I came to their field school. They understand the cultural geography of the region and are able to target excavations with a great deal of skill and experience. Often, in archaeology, locating and securing the site is a major obstacle. Limited funds, labor and time limit what archaeologists can accomplish during a research season. Large scale test phases and excavations are often not feasible. In addition, when working on publicly or privately own land, compensation is a necessity for acquiring rights to excavate and remove material. These were of limited concern for Chap and Sibel's work in Kenya.

The archaeology in the area has shown that rock shelters were popular living places of Pastoral Neolithic peoples. Thus, after 2007, Chap located three additional rock shelters (including Chemai I and II) which would be the focus of the 2008 season. And, while we excavated in 2008, we went on several excursions to locate other possible rock shelter sites. Experience and the advice of the Mwalimu lead to selection of suitable caves. The presence of middens, the interior of the cave and the amount of artifact material on the surface are all indicators. Thus, methodical sampling techniques are not required.

Once a place is selected to become a site several things must happen. The archaeologist must decide the extent of investigation, boundaries must be set up. This is done by setting up a grid. Using a total station, corners are outlined (usually with magnetic north guiding the orientation) and posts pounded into the ground to mark them. From there, depending on the type of investigation, the grid is built in metered sections; in the case of Chemai I and II the grid squares were two meters squared.

Once the grid is set up, test pits and excavations can begin. Using experience and archaeological evidence, Chap and Mwalimu were able to target excavations based on how they thought the site was used. This is an example of arbitrary sampling in which the data set is statistically biased by the archaeologist. We performed no test pits in 2008 and excavation units were placed arbitrarily. Chap and the Mwalimu wanted a representative sample from the site, but focused the majority of the units on the slope coming down from the cave mouth. From here the process moves into the realm of the story of the handoff.

Until this point, the organization of space is what defines the process. The study of special organization is not unusual to archaeology, but it is usually the space of prehistoric

households that are the subjects. Subjecting an archaeological site to a similar analysis should reveal similar plays of power and community that come to define cultures.

The Grid: Space and Buckets

Unlike the trowel, a bucket is not an iconic tool of archaeologists. Nevertheless, its presence on the site is pivotal. There is no better way to move soil. But what if buckets could talk? Would they live their lives as tools or would they present a different understanding of the site. A day in the life of a bucket is a perspective far from the minds of archaeologists. What could it reveal?

The morning started off with loud bangs and clanks as I bounced around the back of the matatu. Several of my colleagues and I were stacked upon one another so the jolts during the ride were all the more disruptive. Finally, we reached relative peace for a second as the sun shone through the back window of the car. Without warning, this relative peace ended as the hatch was flung open and weary-eyed students began grabbing us and the other tools from the car.

Up the site we went, still stacked, but now full of trowels and knee pads and brushes, all the tools of their trade. I heard someone yelling out a sort of roll call for tools. It seemed we were all accounted for.

When we finally made it up the hill we were unceremoniously tossed on the grass near the massive piles of soil just outside of the ropes laid out on the site. One by one, the students came by and separated me and my brethren. I saw two students fight over one of my close friends. Apparently, one of the sections of this site, G6, had two large buckets and another section did not. It seems that large buckets like my friend are in great demand and highly

appreciated. The losing side of that argument reluctantly stumbled up the hill to me and tossed their tools in me. They mumbled something about being so far away from the great piles of soil that the carriers hardly came over to their section. In order to be as efficient as possible they needed to have bigger buckets for regular removal.

After this excitement played out things slowed down for a while. I was once again roughly plopped down upon the site, this time in a very dusty section near to one of their holes. Due to the carelessness, I tipped over and tumbled into the hole, bring tools and dust from outside the hole with me. This upset a few of the students. I believe they wrote it down in their notes and continued writing and looking into the hole for a while. Eventually, we got down to business.

Almost all of the students got to work in their holes, scraping away portions of dirt and filling me and other buckets with the dirt. While some dirt was unimportant enough to tread upon and toss into great piles, this dirt was saved in its entirety. It is odd, though; I suppose this sacred dirt eventually made its way into those great piles.

Eventually, because I am no great bucket like my dear friend, I was filled with soil. I was full to the brim and one of the students heaved me up from this hole onto the ground outside. They looked around to the other sections. There was a commotion around one of those sections and the student near me went over to it. After a brief time, she came back with one of the Kenyans. The Kenyan in turn took me up in his hand and walked me over the site. I saw the other sections had holes. All were defined by string squares. We walked past five other sections. Other Kenyans carried other buckets, some two at a time. I noticed my large friend was so heavy and full of sacred soil that the person carrying him needed two hands.



Once we were full of the sacred soil we were treated a bit more gingerly, that is, relative to how most buckets are treated. We were not flung down some assembly line, but carried individually without spillage across the site. When we made it to the large piles of soil, which was our sole destination, our contents were spilled out upon a wood-framed screen and then shaken about. We ended up tossed aside, only to be picked up by another Kenyan and brought back to the gridded portion of the site. Sometimes it was the same Kenyan that brought us to the piles, sometimes it was a different. Our return destination was always an unknown too, though more often than not, we made it back to the same section and hole.

This was how I spent most of my day at the site. During the students' lunch break I sat patiently in a hole half-filled with soil. There, as I waited, I reflected briefly on my time at the site. As a bucket I had seen of lot of things and been pivotal to many jobs. I have carried rocks and food and water and tools. Nowhere, before I began working with the students, had I seen so much concern placed on order. In most places I am thrown around and carried hither and thither. I see an unpredictable mix of things thrown in me. Here, at this site, the kind of soil put in me is incredibly specific and I have seen arguments ensue about the origins of the soil. One time, a small amount of soil spilled out of me and onto one of the grassy parts of the site. There was great concern regarding whether or not some of the rocks within that soil fell out along with it. I remembered feeling honored at one point when some of the students spoke of their work in terms of how many times I had been filled. Overall, these students are very strange; while their goal is to get everything done in a timely fashion, they seem to take all possible steps to impede this.

After my reflective break, work continued as usual. It became frantic towards the end of the day and I ended up being filled with plastic bags of rocks and hurried down the hill and into the car. That was a time when my contents were mixed without problem, speed and efficiency

became the name of the game for those few minutes. These trends were only brief and before I knew it we were carefully being placed and stacked inside the van instead of being tossed carelessly.

Chemai I: Bound, Ordered, and Owned

The bucket's life is a strange way to look at the use of the site. Buckets are not humans and "experience" only a use-sided bias of the site; they cannot become part of the social elements of work on the site. It witnesses a very small and soil-centered portion of work on the site. However, in another twist of archaeological irony, its limited perspective illustrates a wider trend on the site. The perspective is three-fold.

When I first drew the map of the site that I included in the introduction to part two of this work, I included a series of lines that represented the paths that the buckets took on the site. At that time we had only opened five units. I drew five separate lines which showed the paths that the bucket carriers took to carry the soil to the screeners; out of habit, most passed through a similar portion of the site. I also noticed during this time the importance of the gridded portion of the site and the open portion of the site where the screeners and great piles of soil were found. This was my first introduction to the idea of imposing order that I explored in the story of the drying screen.

The grid on the site represents one of the first and most defining actions of imposing order in the archaeological process. Until an archaeologist "discovers" and decides that a piece of mountain side is a site, the objects within the soil are unknown. Occasionally they turn up in tilled soil and might play the role of impediments to farming. However, once the archaeologist defines the site, the objects are presumed to exist beneath the ground and become artifacts; that

is, they become special pieces of material culture which must be excavated and dealt with in highly regulated ways. This is the defining line between the objective realities that objects have while deposited within soil and the archaeological objectivity of the metered grid. Whilst underground, a pot sherd is at the whims of geological processes and at the risk of being turned up by humans. They exist under no predetermined order and are unknown to archaeologists. Once the site is created, archaeologists assume their presence and begin to apply ways in which to record the positions they found them in. Suddenly, soil become sectioned out and the objects within belong to specific sections and names. This occurs not only from the grid, but the archaeological concept of stratigraphy in which each artifact while correspond to X, Y, and Z, variables (that is, northing, easting, and depth).

This does not create the tenuous situation of a house of cards like on the screen, but it is the beginning of this process which separates “natural” objectivity from archaeological and scientific notions of objectivity⁴.

The bucket’s perception of work is very limited. This is because work on the site is so ordered that to gain a perspective of the site in its entirety is almost impossible. It must always be related to the work at hand or the individual working. I alluded to this point in the story of the handoff.

Image Making and the Production of Meaning

I cannot emphasize enough the importance that the blurring of ethnographic and archaeological work in this field school has had. The following story will perhaps give it a new level of clarity.

⁴ Ironically, the initial conception of separating of Chemai was done with arbitrary sampling. Thus, whatever initial basis that the house of cards had to stand on was arbitrary and recognized as such by archaeological terminology.

If not, then it will demonstrate why I have chosen photography as my vehicle to explore archaeology.

Three-Stoned Hearths, Past and Present

Throughout excavation, Chap would occasionally pull me aside and point to one of the mountains in the distance. Beneath an outcropping, one could just barely make out a wooden, fence-like structure. Chap explained that this was to trap porcupines, to prevent unwanted encounters and protect family gardens. More importantly, he expressed a desire that I walk to that place, and on the way photograph the countryside, the people and (this was added after we unearthed three enormous stones arranged together with ash in the middle) the family hearths.

On one of the last days at the site, Chap finally arranged for my excursion and I was led by the manager of the Kenyan workers on the site. The hillside that Chemai I was on and the hillside that the trap was one were actually connected. An ancient river had carved a deep valley, but only a small creek remained. In my reflections on the walk I said that I made some fine images and completed Chap's mission. I had made several family portraits and photographed different fireplaces in the houses. Though my ethnographic data were purely photographs, I nonetheless began to understand a little more of what drives ethnoarchaeology. I began to see the Kenya that Chap knew so well. It is a country rich with cultural histories and lifestyles and technologies that have persisted through the years. Yet it is a country distinctly modern and recognizable to a lost muzungu such as myself.

Of all the photographs I took, one stood out among the rest. It was nicely composed, but nothing to write home about (The lighting inside of modern rural homestead is a nightmare for a photographer!). This was a particularly dramatic scene of a hearth. A single, misshapen window



illuminates the scene with bright sunlight. On the bumpy, earthy floor, the three stones are imbedded in a ring of mud. The shadow creates a drama that depicts a complicated scene of nature and artifice.

The resemblance to the three stones we unearthed on the bedrock of Chemai I is striking, indeed they vary only in size. I wrote, initially that the otherwise lifeless feature came to life in the photo bringing a brilliance and narrative to the archaeological process that I had heretofore rarely ascertained. These three stones were not simply recognizably arranged, but held within them the same drama that I had seen on my excursion.

I began to clearly see that the stories within archaeology begin deep in the past; they are punctuated by a period in which artifacts lie beneath the surface, are re-written numerous times in the present, and occur in poignant moments that I could and had captured in my camera. I had always approached my project as one of photo-documenting the series of procedures in the field. But, it was through this photo that I realized how poetic and dense the isolated instances and things could be. The photo, inseparable from the experience I had hiking through the mountains, showed that these rocks are never separated from the life of rural Kenya surrounding them. They showed that they bring with them (through the geological processes of their deposition) social baggage which should prevent archaeologists from treating them as something to be read. If the three stones we unearthed at Chemai were ever like the ones in the photo here, their story cannot be known and recorded, but only rewritten.

Before I had read the theory, my experiences in Kenya showed me the creative elements of archaeology. The stories I've listed above proved to me that there is no uncreative element in the research season. To make the most sense of this, I will use the creative process I know best to illustrate, photography.

The Photography Metaphor

At some point in my exploration into the world of photojournalism, I noticed a matter of verbiage that intrigued me. These days, photographers refer to “making” images instead of taking them. It sounded awkward to me (and still does) but after a brief moment of thought it made total sense. I thought of the early days of anthropology when photos were perceived as undeniable proof of ethnographic authority. Not only did they show that the photographer was there, but that these strange events described in the literature were actually happening. Then I thought of a film I saw about Franz Boas. In it, a photo he had produced of a Pacific native working on some craft was shown as it was displayed in his work. The slow Ken Burns style pan showed that Boas had cropped the photo and the original scene was completely fabricated. Indeed, just outside the cropped frame and behind the dark backdrop were fellow natives dressed in contemporary garb and going about their business. This scene Boas cropped was completely alien to the contemporary world of other natives and anthropologists. We can only assume, then, that this natural and native scene had to be drawn up, likely with great coercion and creative control on the part of Boas.

Composition is one of the most obvious sources of creativity that photographers have and it has been the best site to criticize the sort of truths supported by photographs. But, it is not the only source of creativity. A photograph is essentially the result of light reacting with some sort of medium in the camera like film, or a CCD sensor in today’s digital cameras. This light begins at the sun or the flash or the studio light. It bounces off the subject (in a way decided by composition choices) and begins its journey into the camera. The light passes through a lens which is an intricate series of concave and convex pieces of glass, meticulously arranged to bend the light and flip the image. Different arrangements of the glass elements allow for a different

amount of the scene to be captured. Thus, a short simple lens captures a wide part of the scene because it bends the light less. The long lenses at sporting events bend the light through many more elements (resulting in their Freudian lengths) and capture smaller sections of scenes, producing images we perceive as being zoomed-in.

After the light is bent and flipped (for the human eye flips images and non-flipped images would appear upside-down) it passes through the aperture of the lens. This opening expands and contracts controlling how precisely the light will hit the sensor. Wider apertures result in more diffuse light and more of the image appears out of focus.

After the light leaves the lens and enters the camera proper, it must pass through the shutter before it can strike the sensor. The length of time that the shutter is open controls how long the light hits the sensor. If there is little light, the shutter needs to be open longer so that the light can accumulate into a recognizable image.

Finally, the sensor can vary in its sensitivity. Sometimes referred to as “film speed,” one can choose specific types of film which are more or less sensitive to light. In digital cameras this appears as an ISO number. Higher sensitivity means that light needs to strike the sensor for less time to produce the same effect as light hitting a lower sensitivity.

Of course, there are editing and printing choices that affect the resulting images, but for my purposes here this is enough explanation. In professional cameras, every single aspect I described here is controlled manually. Photographers choose different lenses for different occasions and different lens makers depending on personal preference. Varying the aperture and shutter speed is called setting exposure and has drastic effects on what the final product can be. What is more, each element described here was not predestined in any way. The complexity of professional cameras and lenses is such that they are not rolled off assembly lines but more

meticulously manufactured. Thus each piece of glass in a lens has its own story of creation and installment.

Though the photographer begins with light, the moment that the light enters the lens ends any sort of objectivity in the process. And, in the case of studio photographs and many journalistic shots, the light sources are controlled by photographers as well, so the color and intensity of light is altered. In these cases, it is difficult to find any element which the photographer does not control; even the light itself is manufactured. Still, we must admit that in most photographs, natural light exists as an objective and uncontrollable element.

But, how does this relate to archaeology? The light in archaeology is the artifact. The artifact in its original matrix is out of the control of archaeologists. It is at the mercy of geological forces occurring since its original deposition in the ground. Eventually, an archaeologist comes along and begins to compose a research project which immediately begins to control the movement and understanding that the artifact undergoes. It begins to move through the lenses of archaeology as we ask certain types of questions, design excavation procedure and layout sites. Then, and as the excavation process begins in earnest (that is, when artifacts are finally removed from the ground) the artifact passes through a series of meticulously placed, calculated (yet unpredictable) and socially dense moments.

As light is bent and flipped in the glass elements of photographic lenses, the processing steps in the archaeological process (like washing and drying) bend the way the artifact is understood by reducing it to data. Cleaning allows us to see and sorting begins the description process of data creation.

The catalogue may function as a sort of aperture. Catalogues can vary in their complexity. Our ad hoc catalogue in the 2008 season is like a wide aperture leaving much unsaid

and out of focus, whereas Hodder's catalogue he describes at Catalhoyuk is nearly as narrow as an aperture can get, condensing vast amounts of information into something accessible and clear. I can offer further analogy, but it is speculative because I was not able to work with this team in their lab spaces.

Light entering the camera proper is analogous to artifact entering the lab. Different individuals have different amounts of time dedicated to research in the lab just as the shutter controls how long light hits the sensor. Finally, whether students or PhDs, all the actors involved in processing and studying the material in the lab have different levels of training and expertise which are comparable to the sensitivities of different types of film.

All this is to say that, if a lone photojournalist working on the streets of, say, Olympic Beijing is *creating* instead of capturing (understanding that he is working with natural light) then archaeology *must* deal with some comparable amount of creativity given the population of people working towards a single goal in an archaeological project, not to mention the amount of contingencies that come to bear on sites.

The Photographer and the Storyteller

Shanks and Tilley described the archaeologist as a storyteller. This is a stark change from considering the archaeologist as a steward of the past, one who unearths and protects *the* past. Shanks and Tilley emphasized the narrative aspect of archaeology and, as each archaeologist is a different story teller, suggested that archaeologists tell stories about *a version* of the past. Implicit in this conception is the work of anthropologists like Kirin Narayan⁵ (1989), whose

⁵ Narayan's ethnographic work dealt with Hindu storytelling in India. A student of Clifford Geertz, she advanced his definition of religion as a web of significance. Taking account of how her principal informant altered stories depending on his listener's needs, she saw how religion could be adapted.

work on storytelling demonstrates the fluid and adaptive nature through which stories transmit and disseminate histories and traditions.

To think of the archaeologist as a photographer, then, is to expand upon that previous analogy. A storyteller needs some sort of memory in order to remember the rudiments of the tales. A photograph might do well. If the publication and dissemination of archaeological knowledge is comparable to storytelling (instead of fact spouting) then the research and excavation process is like the photography which triggers the memories behind the stories. The photography metaphor brings the beginning of creativity in archaeology to the field, indeed to the very conception of a project. Thus, the storyteller is not only telling a version of the truth due to his or her subjective present position; he or she only had a contingent, if not manufactured, memory dataset to begin with.

Photography began to blur the lines I knew between pasts and presents, archaeology and ethnography. In this analysis it has even gone as far as to suggest the inability to grab hold of any sort of baggage-less fact from the past. As I begin to conclude my paper, keep in mind the confusing, hectic and intricate series of stories that I told. What ramifications does this have for the idea of rigorous and objective method in archaeology? What effect could social drama surrounding the wash basin have on archaeological reports? Is it feasible to incorporate this kind of reflexive method into every project, and if so, what is the best way? If nothing else, it is clear that these stories and questions cannot be ignored outright, we must emphasize the importance of facing these complications borne out of postprocessualist method head on.

Part III

Falling Through the Web: The State of Objectivity in Archaeology

I have, throughout this paper, used the excavation process as a metaphor to frame my work. I consulted theoretical perspectives to establish grounds for investigation, borrowing from archaeological and sociological perspectives in order to bring up new questions. What was behind Hodder's use of guarded objectivity and could I move past this barrier of dissent?

From there I found a suitable site to collect data from and work out answers to my questions. Being put in the position of site photographer (and bringing an ethnographic perspective to fieldwork) I laid a grid down on the site and began to make sense of the hectic events of fieldwork and data processing.

I established excavation units, selectively sampling intriguing moments that I encountered through my photo-documentation and participation alongside the team. Using photos and notes from the day's events, I began to peel back layers within individual events, searching for idiosyncratic occurrences. I took notes on my excavations and began to piece together the relationships at play surrounding artifacts.

Finally, I established a sort of catalogue, a metaphorical system for understanding my data points. Through photography I was able to tie all the different moments and stories together. Indeed, borrowing from such a completely different field I was able to feel out the archaeological process to the limits of its intelligibility. Now, the task is to incorporate what I have learned from Latour, from Kenya and from photography back into the discourse of archaeology.

Lessons from the Moments in Archaeology

Photography proved a useful tool in two ways. The first was conceptual. In the photography metaphor I was able to demonstrate the creative aspects of archaeological research. Each moment in the fieldwork was like an element in a lens or setting on camera; highly controlled by the head archaeologist (though not completely) and by no means predetermined or necessary. Photographers and archaeologists could modify their process, removing pieces of glass or moving them around, there is nothing stopping them from this. However, the product would change drastically and perhaps become unrecognizable, like a blurry image. It is, then, the desire to achieve a certain type of aesthetically pleasing image or product that drives the placement and manipulation of the elements.

But what of the elements themselves? The second benefit of photography was to use the photos as sites. It is easy to see the processes in archaeological fieldwork as common sense and simple. Indeed, in almost all archaeology I have been exposed to, the steps of fieldwork are brushed over quickly. In the cases of special method, like remote sensing, there are sections devoted to understanding the tools and their data, but nothing in-depth surrounding the politics of their use. Similarly, these photos from the Kenya project could have been placed within this paper as supplements, works of art demonstrating that real people were in Kenya and were sorting artifacts and washing them. Speaking as a photographer though, I know that photos are not so simple. They often look pretty and tie messages and concepts together nicely but there are always compositional elements contributing to the prettiness.

By excavating the photos I revealed innumerable levels of meaning occurring within these moments. Continually the conflicts between the inexperienced newcomers and those in leadership positions were being fought during tasks like washing and cataloguing. To complicate

matters more, through events like the handoff, newcomers were gaining experience and being promoted, raising tensions between some higher. These negotiations could have occurred without the artifacts being present, but their presence in the situations was not a passive one.

Throughout excavation, washing and especially cataloguing the artifacts were accumulating layers of meaning on themselves. One of my initial conceptions of this project was to photograph single artifacts in the different situations they pass through and to peel back the layers of meaning accumulating on these veritable palimpsests of archaeological identity. Not only are archaeologists giving informal meaning to artifacts, in typological senses, throughout the field season, but they are negotiating identities through them. Archaeological prowess could be gained through “porch-chucking” and other sorts of identification. In the case of Jimmy Hoffa, the paranoia of loss (not unlike the paranoia that arose from Latour and Wooglar’s work) was acted out through a relationship between a group of students and an artifact.

Thus, though these artifacts seem to simply pass through the process (as merely objects in the hands of those arguing over who has the right to dictate the work schedule) they become intertwined, at various stages of the process, with the personalities and identities of those working with them. In the case of the no-talent potters, Hodder’s dialectical relationship with the data is lived out vividly as the student’s ability to identify pot sherds and the ancient potter’s ability to create are brought out through ordinary, drab pieces of ceramic.

A criticism I have often encountered is that stories about the relationships between students in the field have no important effect on the data. At most they could affect the efficiency and thoroughness of the data collection. I think a look at the moments should demonstrate the importance played by the seemingly harmless *personal* interactions taking place during *professional* exercises.

I imagined, as I concluded my fieldwork, creating a diagram of each photo demonstrating the various relationships being played out. My narratives surrounding the moments are but a brief attempt to this end. Indeed, the relationships played out within the moments were not only tied to other moments I described here but also to countless other instances where artifacts were not even involved. I quickly realized that such a diagram would quickly become endless, eventually extending far enough to reattach to itself and become spherical. And, as I learned in my high school days of geometry, there are an infinite number of possible points on a circle.

Thus, there seems to be no self-evident way to separate the relationships within field school from the meanings gathered by artifacts. A strong argument can be made that at a certain point, relational events are so far removed from artifacts that their influence on one another can be ignored. This is true, much of what goes on after work hours during field school remains distinct both formally and informally from the archaeological work proper. But, I think that the infinite arrays of connection within the fabric of the field school reveals that such distinctions are arbitrary and result from an *a priori* assumption that true archaeology is a scientific process and not a personal one. The distinction may be useful, but it does not negate the realities of these connections.

I would like to offer one final lesson learned from the narrative moments in archaeology. The trend of imposing order on archaeological data remained throughout many of the different moments. From imposing order in terms of a grid on a site to the ordering of drying artifacts, it seems that archaeologists are continually creating ways of understanding. This links nicely back to the understanding of archaeology as a creative, photographic process. The order is not inherent in the data, in the pieces of stone emerging from the ground. The systems used to make sense of

them are often developed on site as spur-of-the-moment systems. Creativity abounds throughout the archaeological process and is nowhere more evident than in the processes of excavation and fieldwork.

Revisiting the Black Box

What I hope to have accomplished throughout this work is to open the black box of archaeology. Indeed, prior to my investigations in the summer of 2008 I was aware of no ethnographies of archaeology like the one I was undertaking. After investigating more literature I found some examples. A recent volume edited by Matt Edgeworth, *Ethnographies of Archaeological Practice* (2006) was the most substantial example I found. Within the opening essay, Edgeworth demonstrates the ethnographic perspective relative to fieldwork is not totally new, and was found in certain veins of even processual archaeological work.

A discussion of Catalhoyuk, and other sites excavated by well-known postprocessualists, shows that many archaeological teams are supplemented by ethnographers. And, as Hodder seems to outline in his 2001 volume, much of this work has focused on the impact of archaeologists on the various communities involved at sites (native, political and archaeological). However, there seems to me to be a difference between an ethnography of the effects of archaeological practice and a study of the practice itself.

A few contributions to the volume were comparable to my own; namely an essay by Blythe Roveland about understanding the contextual nature of practice (Edgeworth 2006:56) and an essay by Cornelius Holtorf working out the different kinds of experiences that project members have at a site (pp. 81). Many of the other contributions deal with the effect archaeologists have on communities, especially through culture heritage sites. The latter type of

ethnography seems similar to Hodder's approach to reflexivity. The importance of complicating the authority of archaeological knowledge in contrast to local knowledge is important, but still leaves a great amount of practice going unstudied.

Fieldwork appears to be the archaeological black box through which questions are entered and answer-filled data emerges. Save for a minimal amount of investigation the process lies outside the realm of formal understanding. Archaeology students are expected to accumulate experience under qualified archaeologists to learn how to conduct fieldwork. There is no textbook I know of that details the process of excavating a two by two meter unit. In addition, Edgeworth comments on the fact that most archaeologists bring in ethnographers and do not complete this research themselves (Edgeworth 2006:9). The process is, for the most part, closed to thorough and original archaeological investigation.

This paper was not an attempt to create a guide to excavation, nor do I think that such a guide is necessary. Instead, I think I have opened the black box of archaeology. Like Latour I attempted to encounter archaeological science in action, to make sense of the discipline through the moments in which knowledge is produced. This was not a targeted ethnography of the effect the Kenya project had on the small town we lived in, but rather an ethnography of the archaeologist's experience of culture of meaning-making. This culture was grounded in the social relationships developed throughout the field season and tied together by the work with the artifacts. Within this cultural matrix archaeological data were processed and ultimately made understandable for further analysis. The data were made meaningful in tandem with and as a result of the social experiences of field school.

By opening the black box we reveal tenuous strands that make up the web of archaeological authority. These fieldwork strands are at the very root of the web, for without the

production of understandable archaeological data, there is no archaeology. Not unlike the web Latour revealed in *Science in Action* the archaeological web is social, communal, delicate (if not paranoid) and totally contingent on the expected product of the research.

I think it is important to recognize that fieldwork is just one of several black boxed moments in archaeology. This examination of fieldwork is not enough to demonstrate that the interpretation of archaeological data is affected by the politics of moments. Another entire work could be devoted to an ethnography of archaeologists in the lab months after excavation. Indeed, Latour's work started with fact claims; claims made in publications years after the black boxes had been closed. But the fact that this study has limits does not diminish its importance. Most importantly, it becomes clear that the archaeological record, when it reaches the lab, has already been through a great deal of drama.

A dramatic change in fieldwork (i.e. skipping the washing step) has lasting effects on the archaeological record. If we can recognize major alterations in the elements of our archaeological camera, why are the minute tweaks so hard to recognize? If anything, the moments played out in fieldwork suggests that similar situations are likely to play out in the lab and in interpretation.

What of Objectivity?

If I had accepted Hodder's idea of guarded objectivity I do not think I could have come to understand archaeology through the photography metaphor. It is not that such a perspective forbids this kind of understanding, but, in the least effect, it does not encourage it, and in the greatest effect, impedes it. It was only by embracing an intellectual anarchy that I was able to draw from archaeology, photography and sociology of science in a comprehensible way. My

questions were not guided from within the web of archaeology, but were guided by an understanding that the web was not the sole source of authority in general and, specifically, in archaeology.

I cannot contest the law of superposition, which emerges as a compelling reason to maintain guarded objectivity, and do not wish to. I can only invoke Latour and suggest that such a law was not necessarily self-evident, but was only made true and authoritative by a series of social situations and players.

But a critical stance on objectivity is not about rejecting hard and fast laws of geology. It is a continued embracing the human element of archaeology. Consider a fundamental difference between archaeology and photography. Where some photography deals natural light originating within the chemical reactions on the surface of the sun, the archaeologist as photographer never deals with such a source. The light in archaeology may have traveled through geological process, but originated at the weathered hands of a culturally steeped human being. Given the contingent and subjective nature surrounding the creation of these artifacts, one must ask if the objects were ever free of social influence to begin with. Surely we cannot begin to take account of the artifact in *action*, but does this mean it should be sidelined in order to facilitate archaeological understanding?

The archaeological process is embedded in cultural processes (past and present) before the artifacts come out of the ground. Any attempt to maintain a sense of objectivity is an attempt to arbitrarily cleave the subjective experience of an artifact between that of the past and that of the present. The time during which the artifact lies at the mercy of geological forces is a compelling choice, but it is still just a negotiated choice and not an absolute one.

If we understand the constructed nature of this cleave, then it is not detrimental to dissent. However, I think the dangers that come along with the maintenance of objectivity (namely the assumed primacy of archaeological knowledge and the closure of the black box of fieldwork) should force archaeologists to think twice about it.

Right now I am lucky. As a student brought up in a discourse that has, to a greater or lesser extent, begun to accept the claims of postmodernists, I am able to live easily with the contradictions and multiple truths inherent in the movement. I am able to see through fieldwork the complicated origin of archaeological knowledge and the problems of maintaining objectivity. Yet, I can practice archaeology in a traditional sense of systematic excavation and data processing. I can at once deny absolute objectivity whilst establishing some arbitrary and well-fitting notion of objectivity for the time being.

To use the style of a Latourian paradox, only once one denies all notions of objectivity can one begin to move forward to work out a proper context for objectivity. Thus, the approach is always grounded in uncertainty and relativity instead of absoluteness and truthiness. Conclusions drawn from a momentary objectivity based in an understanding of a denial of objectivity might contribute more to a discussion and debate. The debate may never be resolved, but archaeology has never been concerned with the Indiana Jones type of romantic truth.

Through the moments in archaeology that I became so familiar with over three field seasons of excavation experience, I have developed an idea of the discipline in which I am not constrained by methods or theoretical debate, but instead guided by intrigue and questions. Surely this type of academic endeavor is not threatening, as some seem to feel about this type of

postmodern perspective on archaeology, it can only open up more doors for archaeology in the future.

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