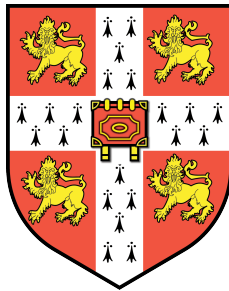


Authority and the Production of Knowledge in Archaeology



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Abstract

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by Tera C. Pruitt

This thesis examines the role of authority in the production of archaeological knowledge. It examines how fluid ideas and observations formed in the field become authoritative, factual, solid archaeological products, like scientific texts, reconstructions or museum displays. It asks, what makes a person, a thing or an account of history something that is authoritative? What makes someone an authority on the past? What is archaeological authority? This thesis deconstructs and exposes authority in archaeological practice. It targets how practitioners of archaeology actively enact, construct and implement authority in the process of producing knowledge. Formal representations of the past rely heavily on an underlying notion of the 'authoritative account'. The entire process of reconstructing the past in archaeology is dependent on individuals and institutions existing as authorities, who actively or passively imply that artefacts, sites and final interpretations are 'authentic' or have 'fidelity' to the past. This study examines how authority and acts of legitimation are employed and distributed through the medium of science, and how they need to be actively performed in order to acquire and maintain status. This thesis not only argues that authority is embedded in every stage of the archaeological process, but importantly, it identifies how this authority manifests through the medium of scientific acts.

This thesis is structured around two comparative case studies: one case of professional archaeology and one case of alternative archaeology. Both are archaeological sites that produce their own 'authoritative' accounts of the past through practices, publications and presentations. The first case is the professional archaeological project of Çatalhöyük in the Republic of Turkey, under the direction of Ian Hodder at Stanford University. This case offers insights about how the processes of inscription, translation and blackboxing establish and maintain authority in archaeological practice. It also addresses how physical and intellectual space, as well as issues of access in localised knowledge-producing social arenas, affect archaeological authority. The second case is the controversial pseudoarchaeological project in Visoko, Bosnia, commonly referred to as the Bosnian Pyramids. This project, under the direction of amateur archaeologist Semir Osmanagić, has successfully created an account of prehistory that has been received by the general Bosnian public as authoritative, despite objections by the professional archaeological community. This case demonstrates how authority can be constructed, mimicked and performed by drawing on academic arenas of scientific practice and by eager public participation. Specifically, this case study highlights the importance of socio-politics, authoritative institutions and performative behaviour in the construction of archaeological authority.

Declaration

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except where specifically indicated in the text.

This thesis does not exceed the limits set by the Faculty of Archaeology and Anthropology, including a 25,000 word extension approved by the Archaeology and Anthropology Degree Committee and the Board of Graduate Studies at the University of Cambridge.

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FOR MY FAMILY

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List of Acronyms

BCE: Before Common Era

BP: Before Present

ICBP: 1st International Scientific Conference of the Bosnian Pyramids

SSK: Sociology of Scientific Knowledge

STS: Science and Technology Studies

UN: United Nations

UNESCO: United Nations Educational, Scientific and Cultural Organisation

CHAPTER ONE:

Introduction: Archaeological Authority and the Mangle of Practice

“If sociology has been marked from the start by the discovery that action was overtaken by other agencies, it has been spurred even more forcefully by the ethical, political, and empirical discovery that there exist hierarchies, asymmetries, and inequalities; that the social world is just as differentiated a landscape as a rugged and mountainous terrain; that no amount of enthusiasm, free will, or ingenuity can make those asymmetries go away; that they all seem to weigh as heavily as the pyramids...that any thinker who denies those inequalities and differences is either gullible or somewhat reactionary; and, finally, that ignoring social asymmetry is as ridiculous as claiming that Newtonian gravitation does not exist.” (Latour 2005: 65)

1.1 Introduction: Authority and Archaeology

1.1.1 Authority and the Production of Knowledge in Archaeology

This thesis asks: what is the role of authority in the production of archaeological knowledge? To explore this core question, this dissertation investigates the complex negotiations, transformations and heterogeneous acts that go into the production of authoritative accounts of the past, such as academic texts, archaeological reconstructions and museum displays. This thesis examines how fluid ideas and observations formed in the field become authoritative, factual, solid accounts about what happened in the past. It asks, what makes a person, a thing or an account of history something that is ‘authoritative’? What makes someone an authority on the past? What makes a professional interpretation ‘more right’ or ‘more expert’ than an amateur one? In cases where amateur or alternative accounts of the past have more authority than professional opinion, then why? Why do some opinions hold more weight than others, within and without the professional discipline? Furthermore, what ethics and accountability lie behind archaeological authority? This thesis seeks to address these questions by candidly deconstructing and exposing authority in the disciplinary practices of archaeology. The aim is to examine how authority is both passively and actively embedded, used, translated, desired or resisted—structurally, conceptually and spatially—in the production of archaeological accounts of the past.

1.1.2 Defining 'Archaeological Accounts of the Past'

Accounts of the past are constructed and narrated by professional archaeologists using material culture, which is acquired through practices like excavation and then interpreted to offer the best judgements about 'what actually happened in the past and why'. This interpreted past is presented to other academics and to the public in the form of publications, museum displays, reconstructions, and in forums such as conferences and seminars. Such formal representations of the past rely heavily on an underlying notion of the 'authoritative account'. The entire process of reconstructing the past in archaeology is dependent on individuals and institutions acting as authorities, actively or passively implying that artefacts, sites and final interpretations are 'authentic' or have 'fidelity' to the past.

Over the past thirty years, many academics have argued that the practice of science is inevitably affected by its social context, and that scientific practice progresses according to academic fashions of the time (Kuhn 1970; Feyerabend 1975). They have argued that scientists work within paradigms of practice and knowledge, and they "attempt to extend and exploit [these paradigms] in a variety of ways" (Kuhn 1970: 91). In the field of Science and Technology Studies (STS), academics have argued that knowledge is acquired, developed, distributed and contested in a social environment (Latour and Woolgar 1986; Latour 1987; Law 2004). The development of a 'fact'—something regarded to be a truth about the natural or social world—is a social, physical and material outcome of people who interact within social networks. Because of the social nature of factual knowledge, represented 'truths' about the world are always relative to, and rely upon, structures of authority—power asymmetries between individuals, institutions, materials and representations.

This dissertation is situated within this general strain of reflexive study of scientific practice, and it focuses on the observation and identification of authoritative structures inherent in decision-making, interpretation and production of knowledge in archaeological practice. This thesis emphasises the production and presentation of 'final product accounts' of what happened in the past, which are arguably the last and most important steps in the archaeological process. This study is operationally based on the idea that contestation and tension in a process allow for its internal complexities to become more transparent, a theory called 'blackboxing' in Science Studies. Bruno Latour (Latour 1999: 304) coined the term 'blackboxing' to define a process or model that runs so smoothly and efficiently that no one stops to question its internal complexities, only its inputs and outputs. Latour argues that processes in science often operate so rigorously and efficiently that scientists rarely question the internal social complexities of their own

routine actions; they only question their data and results. According to this theory, when contention or conflict arises, or when something goes awry, the 'blackboxed' systems of practice become more transparent. In contested practice, people can more thoroughly examine the internal complexities of their own working system, breaking down the walls of a 'blackboxed' system. This is a concept I discuss in depth in Chapters Two and Three (Sections 2.2.7 and 3.3.2).

1.1.2.1 *Defining a 'Final Product' Account of the Past*

For the purposes of this dissertation, a 'final product' account of the past is defined as an explanation of archaeological material that appears in condensed form meant for public consumption. The 'public' in this definition is simply those who receive or consume accounts of the past. This category includes both the general public of lay persons *as well as* specialists or experts in archaeology. Examples of a 'final product' account of the past include archaeological explanations that appear in newspaper reports, television media, websites, as well as academic reports, museum displays and public conference presentations. The reason this dissertation includes archaeological accounts of all consumable varieties—from professional conference presentations to popular science television shows—is because these accounts are all fundamentally based on the same principle: they are acts of summarising, abstracting and stabilising the fluid, mangled and unstable social processes of knowledge production that lie behind their construction. The term 'account' is used because these public explanations are 'accounting for' material culture by describing or explaining the activities of past peoples. The reason this dissertation calls these 'final products' is not because these accounts are meant by their authors to be seen as 'final' in the sense of eternal or unchanging. Rather, 'final product' accounts are interpretations that appear as stabilised explanations in a 'final' form meant for public consumption. They are often meant to be contested and changed if appropriate (especially the products of *scientific* knowledge production); however, they are presented in a 'final' format which is meant to be as faithfully representative to original material or conceptual understanding as possible.

Final accounts of the past are 'front stage' products, which consolidate and 'black box' all of the messy processes that went into the making of the accounts in the 'back stage' social arenas of knowledge production. A published archaeological paper might, for instance, headline the account: "Medieval skeleton shows signs of arthritis". Behind this statement lies all of the archaeological activity that went into the production of this account: the complex history behind why this particular skeleton was chosen to be studied

and accounted for by an archaeologist, why and how the archaeologist became an archaeologist in the first place, why the archaeologist is considered an agent worthy to speak about skeletal pathologies, where this skeleton came from, how it was exhumed or excavated, how and why that skeleton would be diagnosed with arthritis, the complex history of biological anthropological studies that went into the development of a pathology of arthritis in the first place, the use and agency of complex technical apparatuses that turned the skeleton into an representation of 'arthritis', how and why the data and results were finally presented in a textual form—all of these processes become mere *assumptions* that lie behind the 'final product' archaeological account of the past.

Most importantly, *not all accounts are equal*. Some accounts of the past are seen as more authoritative than others, as more or less valid, and a great many factors play into this perceived status of an account. The assumptions behind a single statement comes packaged with *who* is saying the statement, *how* the statement is said or presented, *why* the statement is being presented and used, and *where* the statement is presented. If this statement is presented by a professional archaeologist in a PowerPoint presentation at a major scientific conference on skeletal pathologies, for example, it likely carries a far higher status and burden of validity than if it is typed in a newsflash headline by an alternative journalist for Nexus Magazine. Behind this statement and its presentation lie a number of assumptions about the 'back stage' activities that went into its production.

An account can be called 'authoritative' when people accept its information and explanation as final or valid, and *when people stop seeking alternative knowledge* (Kruglanski 1989; Raviv, Bar-Tal et al. 2003). When knowledge or information becomes identified as 'authoritative', people may take executive action based on that information, which can ultimately affect results, situations and outcomes from the actions that people take based on information they perceive as valid. The concept of authority—both in terms of the immediate 'front stage' authoritative presence of the account, as well as the attached or assumed 'back stage' qualities—plays a major role in how an account is perceived, consumed, reacted to and regarded by the lay and expert public. What makes an account of the past more or less 'authoritative' is the extent to which people accept it as valid.

1.1.3 Major Themes: Blackboxing, Translation and Epistemic Dependence

Three central themes form this dissertation's exploration of the role of authority in the production of archaeological knowledge: blackboxing, translation and epistemic dependence. First, there is the concept of blackboxing in institutional contexts, with idea that contestation can breed transparency. Institutions—customs, laws, hierarchies,

structures of authority—can potentially black box a system, as well as create and sustain epistemic and executive authority of people and things or abstractions. This thesis explores how, in the terms of Latour’s concept of ‘blackboxing’ (1999: 304), archaeologists question inputs and outputs. Archaeologists often question what objects and data they find and manage (inputs) and whether or not their interpretations of those objects are competent or incompetent (outputs); however, they often do not question the actual system and structure of their own established system, asking how and why their system of practice operates like it does in the first place. An underlying concept in this dissertation is the idea that when a successful social system of practice is in place, it can be hard to break out of that system to see what is actually happening below the surface. When contestation arises, the system can break down and become more transparent, because contestation brings focus to the underlying operation of a blackboxed system. This concept of contestation and blackboxing is a central concept of this dissertation study’s methodological approach, and is discussed further in Chapter Three.

A second major theme is that of translation. According to Latour’s ‘translation model’¹ (1986: 267), authority and power are products of social interaction, accumulating in the hands of a multitude of different actors. Underlying this theme is the concept that a web of actors—human as well as material, both tangible and intangible—are interrelated under a system of practice. This thesis explores how authority is only built and sustained through the accumulation of negotiations by many different actors in a network; each actor supports and sustains a given object, narrative, archaeological interpretation and so forth, in order to further and achieve his own goals and aims. In practice, an artefact or account accumulates power over people and practice as its interpretation is translated through each actor’s goals and aims. This concept is discussed further in Chapter Two, and it is extended in Chapters Four and Five.

Finally, this thesis fundamentally rests on the concept of epistemic dependence. ‘Epistemic dependence’ is the “appeal to intellectual authority and the way in which such an appeal constitutes justification for believing and knowing” (Hardwig 1985: 336). In other words, a person may believe many things for which he does not possess direct evidence for, but he relies on the authority of experts who he thinks do possess the necessary evidence. Epistemic dependence is essential to how we interact with knowledge beyond our experiential capabilities. This concept is discussed in detail in Chapter Six. For now, it is useful to point out that epistemic dependence plays a key role in how accounts are constructed and translated, gaining authority and status by politics and performance.

¹ See Section 2.2.3.

These major issues—blackboxing, translation and epistemic dependence—form central themes in this dissertation’s exploration of the role of authority in the production of archaeological knowledge. This thesis is structured around related thematic arguments. (1) Executive and epistemic authority in an academic discipline like archaeology manifest directly in the process of stabilisation, which occurs through processes like inscription and translation.² That is, during the process of ‘producing knowledge’ in archaeology, fluid ideas are actively turned into stable, formal accounts of the past, such as textual representations or museum displays. In the process of knowledge formation, there is a fundamental tipping point between factual knowledge as it is constructed in a fluid development phase, and the knowledge as it appears solidified in a presentable, publishable development phase. It is in this tipping point that mere ideas become solid facts, strengthened and made authoritative by the robustness of a new material and media presence. This argument forms the primary discussion of **Chapter Four**. (2) The power of external socio-politics can affect how readily the general public or scientific community accepts accounts of the past. Many “problems of legitimacy and of extension arise because ‘the speed of politics is faster than the speed of science’” (Collins and Evans 2007: 125). Regardless of the ontological value of archaeological narratives or interpretations, some accounts of the past may be more readily accepted, highly regarded and seen as ‘authoritative’ by the general public because of the social needs they fulfil. (3) The power of using the ‘appropriate performance’ of scientific behaviour can also directly affect the authority of an account of the past. These latter two arguments form the primary discussion of **Chapter Five**. (4) Authority is produced as much as it is consumed. While authority is, in effect, built and accumulated by various actors, it is also consumed in the process of translation. In archaeology, things and narratives are often packaged for consumption, and the processes of inscription, translation and performance are intertwined with how status and authority are received and consumed by the public. **Chapter Six** concludes with a discussion about the implications of the production and consumption of authority in archaeological practice.

This dissertation outlines these arguments using two illustrative case studies, both of which are involved in levels of interpretive contestation. To maximize descriptive value, this research employs one case of professional archaeology and one case of alternative archaeology. Both case studies are archaeological sites that produce their own ‘authoritative’ accounts of the past through their practices, publications and public

² The concepts of executive and epistemic authority are introduced in Section 2.2.2. The concepts of inscription and translation are introduced in Section 2.2.5.1, and further expanded in Sections 3.2.1.1 and 4.4.1.

presentations. The first case study is the professional archaeological project of Çatalhöyük in the Republic of Turkey, under the direction of Ian Hodder of Stanford University. **Chapter Four** examines site's intentional reflexive practice and its professional status as a highly authoritative and prestigious archaeological site. The site of Çatalhöyük produces both authoritative accounts of the past and authoritative accounts of present archaeological methodology, which have been openly contested, by both academic and alternative groups. This case study is employed to offer insights about how the processes of inscription, translation and blackboxing can affect and establish authority in professional archaeological practice. It also addresses how physical and intellectual space, as well as issues of access in localised knowledge-producing social arenas, can affect archaeological authority. The second case study in this dissertation is the controversial alternative archaeology of Visoko, Bosnia commonly referred to as the Bosnian Pyramids. This project, under the direction of the 'amateur archaeologist' Semir Osmanagić, has been very successful at creating an account of prehistory for the general public, which has been received by the general Bosnian public as authoritative, despite objections to the project by the professional archaeological community. **Chapter Five** uses this case study to explore how authority can be built upon, mimicked and performed through drawing on academic arenas of scientific practice and through eager public participation. Specifically, this study highlights the importance of external socio-politics, as well as drawing upon authoritative institutions and performances, in the construction and maintenance of archaeological authority.

1.2 A Crisis of Authority in Archaeology?

1.2.1 The Importance of Addressing Authority in Archaeological Practice

Along with a detailed exploration of how authority operates in archaeological practice and presentation, this thesis also contributes an extensive deconstruction of the term 'authority' in **Chapter Two**. Authority is a conceptual abstraction that directly reflects asymmetrical social power relationships, and it also manifests in material ways: any two things or people, when put in tandem, directly relate to one another in terms of asymmetrical power, influence and status. A major contribution of this thesis is to outline some of the roots and debate about the nature of authority that have emerged in disciplines outside of archaeology, implementing a wider collective understanding of the term for use in archaeological discourse. This is not to say that authority has been

disregarded or neglected in the field of archaeology, or that this dissertation is raising issues about authority and archaeological practice for the first time. In fact, this is quite the opposite: problems of authority as they relate to other major issues within the field have been raised by archaeologists for decades as important and worth our concern—especially in discussions over issues like the impact of personal biases on the material record, the need for multivocality and collaboration with the public, issues of physical access or ownership of archaeological material, all matters that directly rest upon the concept of authority. However, this thesis argues that while the field seems to readily engage with issues of authority and power rights, rarely has the root conceptual understanding of what authority is and how it manifests in the first place ever been explicitly discussed.

Furthermore, in the field of archaeology we have often been quick to address authority by dismissing it. Often authority is referred to as something negative, something to be avoided, something that hinders collaboration and public access. However, authority is an integral and necessary part of any academic endeavour, embedded in the social structures of academia and in the scientific traditions that we have brought down from the Enlightenment. In archaeology, practices such as acquiring credentials, performing or accepting expert testimony, engaging in practices of witnessing and peer review, as well as allying and defending our own interpretations through the performance of appropriate behaviours or by drawing on the appropriate categories of practice, are all systematic social ways to accumulate, negotiate and verify authority. This thesis will address these issues in depth. While such authoritative practice is innate in our professional disciplinary methodology, it is also often discussed as if it were a fundamental ‘Bad Thing’ in the wake of postmodern discourse. In archaeology, theories and new understandings of multivocal interpretations and post-colonial ramifications of ownership have arguably left us in an uncomfortable relationship with our own power and authority.

This thesis argues that it is important to acknowledge the root causes and necessary reliance upon authority in the way we produce knowledge. In the discipline of archaeology, it is not only important to address authority as a side-effect or relational issue in problems of access rights and control of the past, but it is also critical to acknowledge exactly *what authority is* as a root system of practice. We need to address where our authority comes from, how it manifests in our own practice, how disciplinary authority is produced and consumed by members of the public and not just individuals within the profession—and perhaps most importantly—we need to address the impact of our own authority, acknowledged or not, on our own interpretations. In order to address these concerns, this dissertation examines authority in archaeological practice by

ethnographically observing how ‘factual accounts of the past’ are produced through the archaeological process. Issues of authority and scientific practice, and the questions that relate to how we understand and account for our past and present world, are a matter of social interest. Therefore, these are social concerns and matters of social ethics, issues which impact both our social and natural understanding of the world, and important to address in detail.

1.3 Thematic Structure of this Thesis

This thesis is divided into three thematic sections. The first section (**Chapters One, Two and Three**) introduces the relevant theoretical concepts, background and theory behind this study. A detailed deconstruction and discussion of authority and reflexive archaeological practice is integral to this project. **Chapter Two** presents an original deconstruction of the concept of ‘authority’ and identifies its relevance in broader academic literature. This chapter introduces ‘authority’ as both an abstract concept and as a system of practice. The term is conceptually tied to power relationships, implicating who has the legitimate right to exercise power and influence others. This kind of discourse provides a useful baseline for a reflexive study of archaeological practice and the production of authoritative accounts of the past in a contested environment—an approach that is used later this dissertation. **Chapter Three** offers the methodological background of this dissertation’s case studies and ethnographic approach. This chapter introduces the two case study sites—Çatalhöyük and the Bosnian Pyramids—and illustrates the themes, concepts and issues behind the fieldwork and case study-based approach of this study.

The second thematic section of this dissertation (**Chapters Four and Five**) raise the main arguments about the nature of authority in the production of archaeological accounts of the past. These chapters use two case studies to discuss the implications of how authority is manifested, constructed and construed both inside and outside the discipline. First, **Chapter Four** introduces how authority impacts the way archaeological knowledge is produced and consumed. This chapter reintroduces the major issues of inscription, translation and blackboxing in the production of knowledge and explores how authority is accumulated, networked and translated in archaeological practice, outlining the way actual practices are mangled and complicated affairs. This chapter uses themes and issues that arose during my fieldwork at Çatalhöyük, and it uses this archaeological site as a means to illustrate the argument that authority is formed in the process of

stabilising fluid ideas into formal, material representations and accounts of the past. **Chapter Five**, addresses the way external social factors—influences and pressures from socio-politics and the public outside of the core scientific community—can directly translate, accumulate and contribute to the authority of archaeological interpretations. This chapter also addresses the importance of performative behaviours in the creation and sustaining of status and authority in archaeology.

In the third and final section of this thesis, **Chapter Six**, I conclude that authority is built and translated and accumulated by various actors, but it also consumed in the process of translation. In archaeology, things and narratives are packaged for consumption, and the way consumption directly contributes to and implicates authority in archaeology is an important issue that needs to be addressed. This chapter raises the importance of closely linked concepts such as ‘fidelity’ and ‘accountability’. The term ‘fidelity’ comes from the Latin word *fidelitas*, meaning ‘faithfulness’, and it references how accurate a copy or simulation is to an original (OED 1989). The notion of ‘accountability,’ a concept in ethics that (in this situation) demands responsibility for any unethical misuse of authority, opens an important discussion about the ethics of results and consequences of archaeological interpretations, reconstructions and authoritative accounts of the past.

This thesis examines how modes and structures of authority are inextricable from the collection, construction and distribution of archaeological knowledge and material. It seeks to show how practitioners of archaeology actively enact, construct, and implement authority in the process of producing knowledge. It aims to examine how authority and acts of legitimation are actively employed and distributed through the medium of science, and it investigates how these acts are embedded and inextricable from practical archaeological methods and theoretical archaeological interpretations. This thesis not only makes the argument that various modes of structural and epistemic authority are embedded in every stage of the archaeological process, but importantly, it identifies how this authority manifests through the medium of scientific acts.

CHAPTER TWO:

Concepts and Theory: Authority and the Social Construction of Archaeological Knowledge

“Science is widely accepted to be three different things: a method of understanding and of establishing facts about the universe; the facts themselves, the products of that method; and a voice of authority and consequently a locus of cultural power.” (Marks 2009: 5)

2.1 Introduction

2.1.1 Introducing Theory and Concepts

It has become a truism that the past is contested space, that archaeological accounts are not statements of fact, but rather educated interpretations about what ‘might have happened’ in history (Lowenthal 1985; Webb 2002). While theoretical discussions about the socially constructed past have rattled the halls of academia for over thirty years, the profession of archaeology has arguably remained the strongest, most intact and authoritative voice in how the material past is accounted for in public settings, in forums such as museum displays and media productions, and in official publications such as books and articles on the past. *Authority*, the abstract influence and physical force, plays a major role in how and why accounts of the past come to be accepted as correct—as authoritative—by both the professional academe and the interested public. The subject of this thesis is the ‘authoritative account of the past’: how it is produced, why some accounts are treated as more authoritative than others, why some people and materials are regarded more authoritative than others, how authority is embedded in the archaeological process and ultimately manifests in the acceptance or rejection of ‘final product’ authoritative accounts of the past.

Previously, the Introduction of this thesis outlined the problems and structure of this dissertation. This chapter addresses the concepts and theory behind this study. The first section identifies the foundation of this thesis: the argument that knowledge is socially constructed. The second section addresses two related but distinctive concepts—‘power’ and ‘authority’—and pays particular attention to the term ‘authority’, which has long been problematic in social studies. This section offers a framework for thinking about ‘authority’ in the context of the term’s origins, and it identifies the main threads of discussion that traditionally appear in both

social studies and in the field of archaeology. In the third section, this chapter offers a new way of thinking about the term ‘authority’ from the perspective of studies in the sociology of scientific knowledge, arguing that authority is a created and earned outcome of complex social interactions. Rather than being a single quality or characteristic that is ‘possessed’ or ‘not possessed’ by an individual—the traditional approach to defining and thinking about authority—this thesis instead opens the argument that authority is an effect or accumulation of status gained during a complex process of social interactions. This argument will be followed through the remainder of this dissertation, and it is central to the study and discussion of the two case studies in this work. The end of this chapter specifically focuses on authority as it has been discussed in general archaeological theory, and concludes with a call for further deconstruction of the actual processes and mechanisms that constitute authority in archaeological practices.

2.1.2 Introducing Authority and the Social Construction of Knowledge

This thesis is based on the premise that archaeological accounts are socially constructed (Wylie 1989). While this might seem to be an obvious statement—since archaeological accounts are clearly produced by people in the present who study material culture that was also produced by people in the past—there is, however, a general dictum that some accounts of the past are more right or more correct than others. Despite waves of postmodernism thought,³ with arguments touching on relativism and constructivism that heavily impacted archaeological theory (see Lampeter Archaeological Workshop 1997), there is still a strong assumption in the field of archaeology that a form of ‘truth’ about what happened in the past is ‘out there’ waiting to be objectively discovered. This assumption is visible in how the discipline is structured and ordered, and in how archaeologists approach and interpret the past. One of the most fundamental tenets of natural science is the idea that nature is constant, and that scientists can create ‘facts’ through the acts of discovery, observation and analysis of objective data. Data, in this sense, is perceived to be legitimate material from the natural world, independent of any social hierarchy or any socio-organizational form of authority (Marks 2009). Archaeologists, from the inception of archaeology as a professional discipline, have worked under this premise, finding human-made objects as an astronomer would find new stars in the night sky, and interpreting culture and human behaviours based on the idea of discovery, observation and analysis. The most notable change of thought affecting this process in archaeology—occurring with the postprocessual theories of ‘multivocality’ and ‘reflexivity’ (Johnson 1999; Hodder

³ See Section 2.3.2.

2000; Holtrof and Karlsson 2000; Hodder 2001; Hodder 2003; Hodder 2008)—has resulted in a much more complicated way that archaeologists look at and understand archaeological interpretations, even if the basic objectivity-oriented methods in archaeology have changed very little.

Despite the fact that the very notion of objectivity has been deconstructed and fragmented in recent years by postprocessual theory, resulting in a new understanding of the past as a complex, hermeneutical and interpretive space, most actual archaeological practice today still works under the overarching methods of discovery and observation, analysis and ‘producing accounts of the past’. The act of excavating and publishing ‘found data’ still remains intact as the basic way the discipline operates. In the field, we still talk of ‘findings’ and ‘data’, ‘observations’ and ‘analyses’. The interpreted past, which emerges from this process, is then presented to other academics and to the public in the form of publications, museum displays, reconstructions, and in forums such as conferences and seminars. Such formal representations of the past rely heavily on an underlying notion of the ‘authoritative account’. The entire process of reconstructing the past in archaeology is dependent on individuals and institutions existing as authorities, who either actively or passively imply that artefacts, sites and final interpretations are ‘authentic’ or have ‘fidelity’ to the past.

The assumption that some level of objectivity or correctness can be reached through the process of scientific archaeology is perhaps most visible in the authoritative status of individual archaeologists, of archaeological institutions like the university and the museum, and most importantly, in the authority of individual interpretations.⁴ Authority, while often tied into a claim of correctness or authenticity, also appears to be equally tied into the level of public acceptance of accounts of the past. The success of an account of the past can often be tied to the socio-political needs or desires of a social community, or in the prestige or power of a charismatic individual. The case of pseudoarchaeology in Visoko, Bosnia is a primary example of how the authoritative status of an archaeological account is tied into performative behaviours, socio-political needs and charismatic personalities.⁵

This creates an interesting paradox: if archaeological accounts of the past are understood to be socially constructed, then why are some accounts considered more right than others? If knowledge is a socially created enterprise (constructed by people who create and use knowledge for their own purposes and for contextual reasons), then why is there a general sense that some accounts—in the form of museum displays, publications or media—represent a more authoritative form of ‘truth’ or an ‘authentic’ past? I argue that the main ingredient

⁴ From the perspective of social constructivism, individual statements of interpretation and objects of creation like images, once generated by archaeologists, can themselves be imbued with authority.

⁵ See Chapter Five.

sustaining some accounts of the past as correct—that propel other accounts to the popular fore, that condemn even others to a sentence of sudden death or a quiet retirement due to unpopularity—is *authority*. What is authority? How and why is it embedded in the archaeological process? What makes some people authorities on the past and others not, and what makes some accounts and interpretations more authoritative than others? These questions also raise important ethical concerns: how is authority connected to claims of authenticity and correctness, to the concepts of trust and witnessing, to a morality of what is right and wrong about speaking for people who are long dead? The past is in many ways a malleable and unknowable thing—so, who has the authority to speak about the past, and who does not? On the other side of the coin, how and why do some people have the authority to silence alternative, less authoritative views? How and why should some people be granted access to a non-renewable resource—archaeological material—to interpret it as they please, while others should not? Ultimately, this thesis is interested in questioning: what is archaeological authority? How does authority manifest in the archaeological process and affect the acceptance of accounts of the past? And what does authority mean to the discipline?

2.2 Defining Authority and the Social Construction of Knowledge

2.2.1 Defining Authority

2.2.1.1 *The Difference Between Power and Authority*

Authority is intimately related to the concept of power, but it is subtly and critically different. The Oxford English Dictionary (1989) states that power is “authority given or committed”—which identifies the underlying idea that the two concepts are related and interdependent, but distinct. As Barnes relays in his article *On authority and its relationship to power*:

The received view of authority within the sociological tradition is that it is power plus: power plus consent, or power plus legitimacy, or power plus institutionalisation . . . Against this, I shall argue here that authority should be thought of as power minus, that to possess power is more expedient and advantageous than to possess mere authority, and that consent and legitimacy are immaterial to understanding the difference between these two attributes. (1986: 180)

Critically here, Barnes singles out some of the more important points about the relationship of authority to that of 'power'. Barnes relays two different views: on the one hand, authority is most traditionally represented as a 'legitimate' form of power that must rely on consent or institutionalisation in order to exist; authority is a capacity that is only operated or enacted with the exertion of power. Alternatively, Barnes argues that authority can—and should—be seen as a passive power in its own right, something that is less forceful or expedient than straight-up power, something that gives a person passive rights to act without discretion, which may or may not translate into power.

To clarify the latter point, Barnes gives two examples. The first is of a monarch who possesses the authority to sign Acts of Parliament into law. This authority, Barnes argues, does not always represent power: the Queen of England has no practical power to alter or withhold assent to most laws enacted in the country today. Thus, Barnes argues, authority is distinguishable from active power and is more of a passive power or right (Barnes 1986: 183). In another example, Barnes gives the case of an 'authority on' Aristotle. This authority, Barnes says, is empowered by an individual's extensive knowledge of Aristotle, who derives her standing "wholly and entirely from his society", rendering "any actual connection between the authority and Aristotle, or Aristotle's texts...contingent, essentially accidental" (Barnes 1986: 186). By contrast, Barnes argues that 'discretion', or active judgement, is not involved in authority, as it is in raw power: "An authority on Aristotle is the passive agent of Aristotle, rather than the possessor of authority is the passive agent of a power. Note that we have authorities on Aristotle in a way that we could not contemplate having powers over Aristotle" (Barnes 1986: 186). This identifies one very important difference between power and authority: authority is a more subtle matter of right, influential control and legitimacy; power is a much more concrete matter of raw force, executive control and action based on discretion or judgement.

What Barnes somewhat neglects in his definition, however, is the fact that authority is not a decontextualised or possessed 'thing', a point which is discussed in more detail in the next section. While authority can be distinguished from raw executive power, it nevertheless relies heavily on contextual materials and actors in order to exist—authority is something not accidental, incidental, nor something that exists without its interdependence on contexts of legitimation. It is problematic, for instance, for Barnes to claim that 'an authority' on Aristotle has only passive power—authoritative people may hold positions in an institution like a university, for example, which gives them certain rights, privileges, accesses and active powers that someone who is not an authority does not have. This power, I would argue, is part of what we mean when we use the term 'authority'. What Barnes calls 'accidental' or 'contingent' factors are actually fully embedded in this person's 'possession' of authority or the person's identity as an authority; the executive authority of the university professor is intertwined in his epistemic

authority as an expert on Aristotle.⁶ Therefore, despite the fact that Barnes offers useful examples and distinctions between power and authority, his narrow definition of authority as a passive power should be supplemented by a view of authority as an accomplishment or effect, stressing its inseparable link with modes of legitimation, and with constant social interactions and negotiations.

It is important to offer the discussion above on the distinction between ‘power’ and ‘authority’ because they are both abstract, highly fluid and debatable concepts, yet endlessly discussed in both the academe and the wider public. Few topics have been engaged as much in academia, at least indirectly, as that of authority and asymmetric social power relationships. Authority touches and impacts a vast range of human experience, both in the present and the past. As a social concept, it is far-reaching and abstract. We speak of authority, in authority, on authority. Things may be authoritative, people may be authoritative, texts may be authoritative, actions and speech may be authoritative, abstractions like ‘knowledge’ may be authoritative—or not. Authority can have material and physical consequences. A desire for authority can lead people to extremes of behaviour and risk, and the loss of it can cause despair, anger or grief. Individuals or collectives are often drawn to charismatic leaders and social movements in the hope to attain some measure of authority or benefit from authority. Students and apprentices learn from the authority of those who teach them, and authorities lead intellectual endeavours. People in search of or ‘in possession of’ authority can turn into powerful consumers and producers of ‘authoritative’ goods. Importantly, authority can also be mimicked and performed, and people often make deliberate choices in how to perform, seek out, or undermine authoritative people, things or knowledge.⁷

2.2.1.2 Traditional Approaches to Defining Authority

The term ‘authority’, much like the related term ‘power’, has been “used, re-used, and endlessly abused” (Law 1991: 165) in both popular and disciplinary discourse on social power relations: “[f]ew words have greater currency in organizational theory and organizational life than does the term authority. Still the concept of authority is as open to conflicting interpretations as any” (Dalton, Barnes et al. 1968: 199). Defining the term is difficult, since it can refer to both tangible acts and actors—such as persons who may be ‘authorities’ that execute their authority through executive force—as well as abstract qualities and tacit assumptions—such as the ‘authority’ tacitly possessed by a person whose opinion holds influence over others. Authority transcends normal metonymy (i.e., you can be ‘an authority’

⁶ See Section 2.2.2 for further discussion on the terms ‘executive authority’ and ‘epistemic authority.’

⁷ These concepts are unpacked in detail in Chapter Five and Chapter Six.

and you can ‘have authority’, and in both cases ‘authority’ is not just a part standing in for a whole); instead, authority references tacit social relations as well as tangible outcomes and executive measures upon which people and things react and interact. The concept is truly relative, based on social relations and asymmetric power, often deeply entangled with other concepts—such as power, influence, coercion, persuasion, authenticity, accuracy and legitimation—so much that each term feeds into each, and any realistic definition must rely heavily on multiple other concepts in order to exist in meaning on its own. Perhaps it is because authority is seemingly obvious, yet still ambiguous, that the term has been used so prolifically in academic research without any significant deconstruction of what the term actually means across disciplines, or at the very least, outside of the narrow scope of a single literary discussion. Even within disciplinary boundaries the term often remains abstract. It is perhaps not surprising that “[e]very few years a writer will ruefully agree with earlier writers that authority remains a difficult concept on which to establish any agreement in terms” (Dalton, Barnes et al. 1968: 199).

In political science, managerial studies, and sociology, authority has often been discussed in terms of human potential for social power and control, addressing why a person, party or social group is dominant over or resistant towards another (Dalton, Barnes et al. 1968; Lincoln 1994). Political and managerial literature on authority has been primarily interested in cause-and-effect physical outcomes of authority and social relationships—seeking answers to questions such as: why was Hitler able to command so much ‘authority’ over his subjects (Milgram 1974: 438; Patten 1977), or why do some businesses and organisations seem to thrive when headed by a charismatic authoritative figure? (Smith 2009). This type of authority is direct and specific, linked very much to action and people with power in social hierarchies—‘executive’ in nature.⁸

Most traditional sociological literature on authority is interested in power relations in the ‘social order’, how power and authority are sustained or resisted over time by various social communities or ideologies. They ask questions such as, how do communities maintain or collapse orders of authority, power and resistance? Karl Marx and Max Weber’s work, for example, both relate authority specifically to economics, power and revolt; they regard executive control and domination of certain members or groups, in various scales of social communities (Marx 1888; Weber 1964). Weber outlined three sociological categories of authority in society, specifically relating the concept of ‘authority’ with that of ‘legitimation’, a term which implies the power to influence others through the force vested in one’s institutional position or elevated status (OED 1989). In his work, Weber defines authority as a type of

⁸ See Section 2.2.2.2 for further discussion on *executive authority*.

'legitimate power' as opposed to illegitimate force,⁹ and his typology of authorities are grouped into three categories. In the first, Charismatic Authority, authoritative power operates through personal leadership and transformational promise; in the second, Traditional Authority, authoritative power is vested in a sense of fidelity to an established tradition, status or occupied position; and in the third, Legal-Rational Authority, authoritative power operates in obedience to bureaucracy, rules and law (Weber 1978). These categories, while somewhat arbitrary, form a useful framework to begin thinking about how authority operates in social groups; they offer a lens from which a researcher can begin to understand the social operation and impact of authority from the most individual and personal level—Charismatic—to the most communal, structured and complex—Legal-Rational. Many later studies on authority in management, politics and sociology often begin their theses with a nod in the direction of Weber's early work.

Definitions and discussion of authority have also appeared in the fields of education, philosophy and psychology, in addition to this earlier interest by political scientists and sociologists like Weber; however, a different language set is often used. 'Authority' has been frequently divided by terminologies like 'executive' and 'epistemic' (Kruglanski 1989; Lincoln 1994; Pierson 1994), which highlight the difference between action-based authority and knowledge-based authority. In literary criticism, psychology and discourse analysis research, authority is often referenced in terms of being 'vertical' and 'horizontal', where 'vertical' authority identifies power relations that are more structural and institutionally based, and 'horizontal' references a more dynamic plane of social relations, where authoritative power is emergent and actively established between individuals (Landsberger 1961; Hill 1973; Smith and Elliott 2002).

It is interesting to note that most of the studies that have attempted to explain and define authority have divided it up into units or types, manicuring and categorizing this amorphous concept into manageable, understandable and referable bits. However, it is important to stress that, always, these categories are arbitrary and potentially run the risk of oversimplification or misrepresentation. A study of the very different divisions of language and categorical use of the concept 'authority' within and across disciplines is much needed in future research and represents a worthwhile future study; however, a comprehensive study on this topic is beyond the scope of this dissertation. Instead, I simply note these many overlapping and often contradictory terminologies, and I will offer only a specific choice of terminologies in the next section—founded on some of the more prevalent and currently popular terminologies from political science and psychology—for the ease of future discussion in this dissertation.

⁹ See Section 2.2.2.2 for further discussion on *legitimate authority* and *de facto authority*.

2.2.2 Categories and Deconstructing Authority

2.2.2.1 Categories of Authority

This section frames the traditional scholarly categories of ‘executive’ and ‘epistemic’ authority, as well as other subcategories like ‘intellectual authority’, as a starting point for discussion for this dissertation. All of these categories (*de facto* and legitimate, executive and epistemic, intellectual authority) relate to matters of power and control—control over rights, usage, privilege, access, production, reproduction and influence. These categories are arbitrary and are not meant to be seen as more than a useful platform for observation and analysis in this research. The second part of this section addresses the problem of defining authority as a quality versus an accomplishment. It argues for the examination of authority as an accomplishment by addressing the social and contextual nature of its development.

2.2.2.2 Executive Authority

The Oxford English Dictionary offers two definitions of authority, which identify some of the more pertinent qualities of the term. In the first, the OED states that authority is the “power to enforce obedience” (OED 1989). This is what traditional managerial and psychological literature often refer to as *executive authority* (Watt 1982; Lincoln 1994). Executive authority is an active right or power held in a specific context, drawn from a delegated or derived title or right. It is also often referred to as ‘practical’ authority, since it creates the opportunity for the practical application of power. The possessor of executive authority has a conferred right to perform an action, whether by subjugation or by allowance by peers or inferiors (Christiano 2004). This is the kind of classic authority held by a leader at the head of a social group, whose position or charisma confers him or her the right to delegate tasks to others, and to enforce obedience relating to the actions and decisions that he or she makes. It is intimately tied to the concepts of legitimacy and power. Stanley Milgram’s experiments on the power of authority, which tested the limits of subordinate obedience to demands made by authority figures, is an extreme, yet classic example of executive authority in action (Milgram 1974).

Beginning with early political theories of authority by scholars such as Thomas Hobbes (1668) and John Austin (1832), and extending into modern political discourse today (Christiano 2004), the two political science categories of authority, *de facto* and *legitimate*, have been offered as distinguishable types of executive authority. *De facto* authority is very similar to raw power; it refers to a person or group who has the capacity to command the obedience of others, regardless of whether all subordinates or peers universally accept that authority. In other words, a person or collective has *de facto* authority simply because they have power over

others. A scholar like Barnes, who strictly defines authority, might say that *de facto* authority is not authority at all, but rather power. However, many political science scholars argue that it is authority, in that it “amounts to the capacity of a person or group of persons to maintain public order and secure the obedience of most people by issuing commands backed by sanctions” (Christiano 2004). In the seventeenth century, Thomas Hobbes even went to far as to argue that *de facto* authority is necessarily justified (or legitimate) simply because an entity is capable of performing authoritative functions (1668); however Christiano (2004) argues that this is a view that most modern scholars shy from. Instead, they note a critical difference exists between *de facto* and legitimate authority.

‘Legitimate’ authority, according to many political scientists, operates with various structures and contexts of support that legitimise a person or group’s right to power, beyond simply the ability to use that power or impress it upon others. In other words, legitimate authority bases its support on context and means of justification—using such contexts as when a person with a charismatic personality employs justified coercion, or when a person has the personal capacity or the institutional role which allows him or her to impose duties, or when a person has a social position that gives her the right to rule (Weber 1964; Ladenson 1980; Buchanan 2003; Christiano 2004). In the case of legitimate authority, the role of ‘the social’ has much more of a prominent function. People and things hold legitimate authority, or are called legitimate authorities, based entirely on social context. In the case of Barnes’s ‘authority on’ Aristotle, for example, the person who is knowledgeable in Aristotle is an authority through legitimate means. This person accumulates his or her authority through a legitimate study of Aristotle’s text, acquiring more authority as a kind of status through their position in a legitimate institution of authority, such as an established university, and they can gain or lose authoritative status based on their legitimate role and performance within such an institution. In such a case, the social networks, institutions and social acts are “mangled” (Pickering 1995) together with the individual’s status as an authority and his or her executive rights as an authority.

While both *de facto* and *legitimate* authority essentially relate to the power interests and the capacity for action possessed by members of a structural social unit, there is a primary difference in the social performances, artefacts and institutions that are involved in both types. Legitimate authority, as opposed to *de facto* authority, is of key interest to this dissertation. Legitimate authority is deeply associated with social modes of legitimation, social roles and performances, and the contingency of its weight on contextual social outcomes. As discussed in much more depth in the second half of this dissertation, legitimate authority, as a form of executive authority, is an important part of the production and acceptance of ‘authoritative’ archaeological accounts of the past.

2.2.2.3 *Epistemic Authority*

In its second definition, the Oxford English Dictionary defines ‘authority’ as the “power to influence action, opinion, belief” (OED 1989). This is often referred to in scholarly literature as *epistemic authority* (Watt 1982; Kruglanski 1990; Lincoln 1994; Pierson 1994; Raviv, Bar-Tal et al. 2003; Christiano 2004). Epistemic authority is intimately related to knowledge formation, influence, expertise and belief. It regards how or why people accept some information as final or valid, and is apparent when people stop seeking alternative knowledge (Kruglanski 1989; Raviv, Bar-Tal et al. 2003). When knowledge or information becomes labelled ‘authoritative’, people may take executive action based on that information, which can ultimately affect results, situations and outcomes from the actions that people take based on information they perceive as valid. Epistemic authority is often interrelated with the notion of experts and expertise,¹⁰ and it is deeply relevant to studies on the social production of scientific knowledge.

Some of the more recent research on epistemic authority has come out of disciplines such as social-cognitive psychology and education. One of the fundamental theorists in epistemic authority is Arie Kruglanski, who developed the theory of ‘lay epistemics’, which “addresses the process whereby human knowledge is formed and modified, and it highlights the epistemic functions of hypothesis generation and validation” (Kruglanski 1990: 181). Lay epistemic theory, and related research in the fields of philosophy of science and psychology, have particularly focused on the question of why members of the public defer to the authority of experts in society. The reliance and use of epistemic authority is a necessary part of modern life, many of these scholars argue, for “the demands of everyday life require us to make many more decisions and hold many more opinions than we could ever base on personally examined reasons” (Pierson 1994: 398). Researchers in the field of education have also examined knowledge acquisition and power relations by particularly addressing the relationship between students and teachers, observing epistemic authority as “a source of determinative influence on the formation of individuals’ knowledge” (Raviv, Bar-Tal et al. 2003: 17). Fundamentally, epistemic authority rests on a consumer’s reliance and trust in the knowledge, influence and expertise of another person or thing, like a book, article or museum display.

2.2.2.4 *Intellectual Authority*

Closely related to epistemic authority is that of ‘intellectual’ authority, a term that also has some currency in academic literature, capitalising on the power/knowledge relationship (see Collier 1992; Furedi 2004). Intellectual authority primarily deals with all aspects of

¹⁰ See Section 6.2.3 for a detailed discussion about the concept of epistemic dependence and the relationship between expertise, knowledge, epistemic authority and archaeology.

legitimate authority—executive and epistemic—that relate to the pursuit, production and consumption of knowledge. Intellectual authority, for example, can be a type of legitimate authority held by a person such a professor, like the ‘authority on’ Aristotle mentioned above. This professor of Aristotle mostly likely has a high degree of epistemic authority, which was earned through her intimate knowledge of, and experience in, studying Aristotle’s texts, as well as through her apprenticeship in academic training and showmanship in performing the role of academic. If this professor holds a high degree of epistemic authority through her known expertise and authoritative publication of work on Aristotle, she may also hold a position of status within an institution such a university. This position of status can offer her a certain degree of executive authority in her ability to make decisions which have an executable outcome. For example, she may have the power to access and use departmental funds for a specific purpose, or have the right to make decisions about staff appointments within the department, or her position may give her a high degree of influence over her students that impact their behaviour. Because of her high degree of epistemic authority, she may also influence other scholars’ ability to publish in widely-ready publications, both through formal means (exercising peer review or editorial control) or informal ones (her influence over the reputations of other scholars in her community). Zygmunt Bauman, a sociologist on postmodern society, argues that such intellectuals can hold “meta-professional authority, legislating about the procedural rules which allow them to arbitrate controversies of opinion and make statements intended as binding” (1987: 6). In this sense ‘intellectual authorities’ often hold legitimate authority that is both epistemic and executive, often situated in positions of privilege or power, relating to context and involving access or opportunity.

Today, a great deal of social influence, power, and emphasis is placed on the role of scientific expertise. ‘Intellectuals’ are in a privileged position in society, simply because science has developed as a profession that holds and sways a great deal of public influence. This thesis is ultimately focused on the implications of ‘intellectual authority’—regarding how power, influence and legitimation pertains to the pursuit, distribution and consumption of knowledge—and its role in how accounts of the past are produced and accepted as authoritative by archaeologists and the public. The negotiation of opinion by ‘intellectual authorities’, which lead to Bauman’s “statements intended as binding”, and which involve both epistemic and executive qualities, is the central concern of this thesis.

2.2.2.5 *Auctors and Auctoritas*

Finally, it is useful to trace the meaning of authority even further, back to its roots. This exercise provides a stable foundation for thinking about the term in specific relation to

archaeological practice. The word ‘authority,’ like ‘author,’ derives its meaning from the Latin noun *auctor* which, according to *Lewis and Short’s Latin Dictionary*, means:

He that brings about the existence of any object, or promotes the increase or prosperity of it, whether he first originates it, or by his efforts gives greater permanence or continuance to it. (quoted in Watt 1982: 11)

An *auctor* is an originator—for example, an inventor, author, ancestor, or inspirer—as well as a promoter or seller of something (Watt 1982: 11). In ancient Rome, *auctor* also referred to “person who warrants the right of possession; hence, a seller, vendor” (OED 1989), in other words, one who creates or promotes something. In this sense, the *auctor* has a kind of power over an object, in his role as creator and promoter, and thus he is a superior actor or agent. As Watt (1982) notes, this kind of superiority, agency and power results, not in an active sense of obedience by those who come in contact with the *auctor*, but rather in a sense of deference or respect: legitimate authority, rather than *de facto* authority or power.¹¹

The word *auctor* is at the root of the Latin word *auctoritas*, from which the term ‘authority’ is more immediately derived. In ancient Rome, *auctoritas* was a quality that could be possessed by some person or group. As such, it is a “force” that is “more than advice and less than command, an advice which one may not safely ignore” (Agamben 2005). This ‘force’ is distinct from the Latin *potestas*, the power or right to rule or command, often associated with an emperor’s active power to command obedience. Instead, *auctoritas* is a personal condition, a mode of influence held, for example, both by the Roman Senate and by individual senators. It is often compared with sociologist Max Weber’s concept of charisma, or charismatic authority (Weber 1964; Agamben 2005). Authority, then, by its relationship to *auctoritas*, can be a power of character and a force of influence.

It is useful to reference these Latin roots of ‘authority’, particularly when thinking about academic authority, and more specifically, archaeological authority. Insightful connections can be drawn by thinking about the term *auctor*—one who brings into existence and promotes an object—and the idea of what an archaeologist does, or what she or he may be. As discussed in much more depth in Chapter Five, one of an archaeologist’s primary roles is often seen to be a ‘discoverer’ of things from the past, who brings about the existence of things that were long-lost or which could potentially be destroyed if not rescued from oblivion (Holtorf and Drew 2007). Along these lines, an archaeologist’s job is also often seen to create or *bring into existence* new things that represent what they find: site maps, charts, diagrams, reports, physical reconstructions, etc., which come to exist through archaeological acts of authorship, artistry, mapping or interpretive industry. Thus, the concept of an archaeologist as *auctor* is innate in this professional role, which involves acts of creation and authorship. An archaeologist can also

¹¹ See Section 2.2.1.1. for discussion on the distinction between power and authority.

be seen an *auctor* in the sense that he or she is a promoter and champion of objects found (and of objects made in the act of archaeology, such as site reports, museum displays, reconstructions), since archaeologists have the role of defending the worth, need for interpretation and safekeeping of both the things they find and the things they produce.

It is significant to point out that, fundamentally, the profession of archaeology and the professionals who work within it derive their *auctoritas*—and thus their authority—on their role as *auctors*, on their intimate engagement with and promotion of the objects they locate or bring into existence. Finally, it is good to revisit and acknowledge the Latin-based roots of authority because the term *auctoritas* is so active. *Auctoritas* is derived through action and constant promotional upkeep; it is a force of activity, authoring and origination; *auctors* only exist in their active production and promotion of things. This is a strong point to hold into the next section, which addresses the concept of authority as an process, effect or outcome.

2.2.3 Authority as an Accomplishment or Effect, rather than a Quality

I argue that the reason “authority remains a difficult concept on which to establish any agreement in terms” (Dalton, Barnes et al. 1968: 199), and why no solid definition has been established in literature, is because most traditional scholarship has not addressed the concept in an appropriate way. Instead of looking at authority as a complex ‘by-product’ of social relationships, as the outcome or effect of interdependent social interactions, as an accomplishment or product—as I strongly argue it is—most previous studies have been exercises in categorising and qualifying social scenarios. They see ‘authority’ as an object or force, a collectable and potentially quantifiable quality that can be defined without heavy interdependence on context. Authority is instead an accomplishment or an effect, a kinetic outcome of social activity, networking and interrelationships: “power is not something one can possess – indeed it must be treated as a consequence rather than as a cause of action” (Latour 1986: 264). Power by this definition, and authority by relation, is not something that is gained or lost, nor something that is active or passive; rather, it is a “*composition* made by many people...used as a convenient way to *summarise* the consequence of a collective action...It may be used as an effect, but never as a cause” (Latour 1986: 265).

Bruno Latour, in his article *The Powers of Association*, argues that the way we think about concepts like power comes down to a debate about their fundamental qualities: “What makes the notion of power both so useful and so empty is a philosophical argument about the nature of collective action” (Latour 1986: 266). Latour presents the important distinction between what he calls the ‘diffusion model’ and the ‘translation model’, which are two different ways of conceptualising social qualities like power. The traditional diffusion model, as Latour

explains it, ascribes to power a force akin to inertia in physics, where ‘power’ is a kind of thing endowed with its own energy: “what counts is the initial force of those who *have* power; this force is then transmitted in its entirety; finally, the medium through which power is exerted may diminish the power because of frictions and resistances” (Latour 1986: 267). For example, by the traditional diffusion perspective, it is assumed that when orders were carried out by a group like the Nazi party, it took someone like Hitler who initially *held* a great deal of power (as a kinetic force) to command an order. The power behind his order was then transmitted through the party ranks after he gave it, with the power either being sustained or resisted by those who received it through the medium of exertion—that is, through the lack of communication, indifference, ill will or direct opposition by interest groups; this diffusion of power resulted in Hitler’s order being followed to greater or lesser degrees, and his power being sustained, increasing or decreasing over time. The diffusion model is the traditional way of thinking about power in society, where power is a possessable thing held in greater or lesser amounts and transmitted more or less successfully through society. This is why so much scholarly literature (see Section 2.2.2, above) has focused on simply categorising power and authority, since it has been conceptualised as a measurable force.

However, in the alternative ‘translation model’, social abstractions like power and authority become very different things. In this model, the spread of power is entirely in the hands of a multitude of different actors, each of whom “may act in many different ways, letting the token [of power: the claim, order, artefact] drop, or modifying it, or deflecting it, or betraying it, or adding to it, or appropriating it” (Latour 1986: 267). In other words, power is an accumulation or effect generated by a web of different actors, things and influences. There is no inertia to explain the transmission of power or authority, for it cannot be possessed or capitalised. Rather, something like authority is the *accumulation* of acts and negotiations by many different actors, who each interact with a token (of power, like an order or an artefact) in order to achieve their own goals and aims. It is called ‘translation’ because it changes, or translates, as it bounces from hand to hand of each actor. Latour gives the example of a rugby game with a rugby ball; power, like the ball in play which forms the ‘game’, “is the consequence of the energy given to the token by everyone in the chain who does something about it” (1986: 267). Authority in this sense, like power, is made up of constituent actions and parts, a complex force—abstract and physical—with a complex social history of construction and use, made up of thousands of constituent parts (Law 1992). It is the outcome of thousands of social choices, actions and reactions; it is networked in social and interdependent space, not independently, and built from both passive and active social agency. This perspective completely changes the fundamental way we think about power and authority relationships:

[In the diffusion model], the notion of power becomes convenient for sociologists. There is always enough already accumulated energy to explain, say, the spread of the multinationals, Pinochet's dictatorship...[But] If you apply the translation model, this reservoir dries up immediately. You no longer have any stored-up energy to explain why a President is obeyed and a multinational grows since these effects are a consequence of the action of multitudes. (Latour 1986: 269)

From the perspective of the translation model, any explanation that claims that Hitler's orders were obeyed just because he 'had power' is unsustainable. In the translation model, the power and authority behind an order given by a military commander to by a group of soldiers is the result of a complex chain of reactions and social context. From this perspective, each actor who comes in contact with a military order has their own reasons for accepting, carrying out or resisting the order, whether for self preservation, personal honour or professional gain, and each individual takes the order and performs it according to their own account or needs, and negotiated for their own reasons. The authority of the order results not simply from the result of inertia imbued in the leader's possession of power, but because of the complex negotiations and interactions that accumulate from each actor's interaction with it.

It is important to consider the fact that the traditional diffusion model runs the risk of oversimplification, skirting over the complexities behind a subject like 'authority'. It is much more improbable, for example, to think of obedience as a product of perfect social 'alignment' to a kinetic force, where all the people who interact with it assent fully without modifying it. As Latour argues, "Such a situation is highly improbable. The chances are that the order has been modified and composed by many different people who slowly turned it into something completely different as they sought to achieve *their* own goals" (1986: 268). The translation model rectifies this oversimplification by allowing space for the actual complexities of a social abstraction like 'authority' or 'power' to emerge in observation. Certainly in the case of this thesis, approaching a study of authority in an archaeological context through the translation model has allowed room for connections to be made and discussions to form about the interconnectedness of actors, things and social context, which would otherwise have been impossible to describe from the perspective of the diffusion model, where power either exists or does not exist in a quantifiable form.

2.2.4 Authority of Things, Instruments, and Ideas

One of the main benefits of using the translation model in thinking about the way authority operates in society is that it opens up a world of possible ways to observe and think about the way social actors interact. Notably, it allows for social scientists to account for the active agency of *things* and *ideas* as well as people.

On the outset, it is clear that people, things, actions or speech, even abstractions like ‘knowledge’, may be called ‘authoritative’ or can be called ‘an authority’ about or over something else. For example, a person who is called ‘an authority’ can write an ‘authoritative’ text, which refers to both the authority of a book itself as well as the knowledge and ideas behind it. Generally, the space between two or more juxtaposed objects, people or ideas provides a given opportunity for social comparability, and comparability opens space for differences in status and authority. Again, as Mortensen and Kirsch in compositional studies write, “this is because relations in communities are in part defined by differences in knowledge, experience, and status—differences in power that endlessly shift within and across social contexts” (Mortensen and Kirsch 1993: 558). In a model of translation—which offers the idea that various actors each have a performative role in the way authority develops, changes and is maintained—this concept of ‘communities’ can include networks of associations and status that operate between people and things or instruments, as well as between ideas or abstractions.

This idea aligns with the argument made in studies of the sociology of science and technology (see Section 2.2.5, below), which not only argues that “knowledge is a social product rather than something generated by through the operation of a privileged scientific method” (Law 1992: 2), but also that social qualities like ‘power’ or ‘authority’ are socially produced entities. Importantly, this actor-network¹² translation model allows for ‘actors’ to be things, machines, or instruments, as well as people, since something like a stage, podium, telescope or writing pen can influence the generation, outcome and acceptance of produced qualities like knowledge or power (Pickering 1995). A classic example would be the authority relations in a classroom, where the act of standing on a stage with a podium and PowerPoint presentation imbues a teacher with a great deal of epistemic and executive authority, simply because the teacher’s social performance draws from the complex social traditions which inform at spatial setup. Furthermore, any actual active power and authority the teacher has in this scenario comes from a complex web of social interactions at the moment of performance, which are based upon and relying upon the teacher’s accumulated status as an epistemic authority, as well as the level of resistance or accommodation given to her by the students sitting on the benches on the opposite side of the room. This complex relationship of authority, and the agency vested in things as well as people, is an important point that will re-emerge and be explored in much more depth throughout the second part of this dissertation, in the analyses of the two case studies.

¹² See Section 3.2.1.1. for further discussion on Actor-Network Theory.

2.2.5 Authority, Social Constructivism and Scientific Knowledge

This thesis emphasises the role of context and process in the production of knowledge. Over the past thirty years, a great deal of academic discussion has emerged about the production of knowledge, in disciplines ranging from philosophy and sociology to the philosophy of science, and it has been recognised that knowledge is highly contingent on social context (Latour and Woolgar 1986; Pickering 1995; Law 1999). A wide body of scholarship has utilised an array of methods from historiography, ethnography and ethnomethodology to study sociological aspects of knowledge production. Bruno Latour and Steve Woolgar, for example, used ethnographic methods to study natural science laboratories, tracing how scientific knowledge is actively and socially produced (Latour and Woolgar 1986). In another case, Andrew Pickering used historiographic and sociological methods to explore how quarks became socially established as scientific fact (Pickering 1995). In archaeology, for example, Cornelius Holtorf traced the ‘life history’ of a pot sherd in order to argue that even the material identity of an artefact is socially ascribed and contextual (Holtorf 2002). From such studies, it has emerged that science is not a sturdy process that merely reveals facts about the world; rather, it is a complex and interdependent social activity, where scientific facts are produced through social and political negotiations, networks, associations and practices (Latour and Woolgar 1986; Latour 1988; Pickering 1995; Shapin 1996). Further, they argue scientific facts—and scientists themselves—are socially constructed in the sense that they are literally made material:

[A]nalytically, what counts as a person is an effect generated by a network of heterogeneous, interacting, materials...If you took away my computer, my colleagues, my office, my books, my desk, my telephone I wouldn't be a sociologist writing papers, delivering lectures, and producing “knowledge”. I'd be something quite other. (Law 1992)

These multiple studies have been unified under the blanket term *social constructivism*,¹³ which is most simply defined by its central claim: that people, artefacts, reality and knowledge are social constructs, dependent on contingent social variables; they are material by-products of human actions, choices and negotiations rather than extant artefacts of nature (Law 1992; Boghossian 2001). It is important to note that social constructivism does not argue that reality does not exist without social interactions, or that particles or dinosaurs would not ‘be there’ without, say, scientific methods and theories. Rather social constructivism argues that ‘facts’ are socially created things: ‘facts’ are knowledge presented as semi-stable forms and entities—set

¹³ It is important to note that the theory of *social constructivism* is related but different from that of *social constructionism*. Social constructivism is interested in how beliefs, reality, and knowledge are socially constructed, while social constructionism is interested in how artefacts or things are socially produced. While this thesis has a primary concern in how archaeological knowledge is produced, it is also concerned with the materiality and presentation of that knowledge—thus both theories are related to this dissertation. However, for ease of discussion, I only refer to the theory ‘social constructivism’ throughout this work.

and presented through the scientific process as authoritative and correct ways to talk about and look at the world. Their forms and acceptance are contextual and material, dependent on the social, political and material nature of the scientific process (Latour and Woolgar 1986: 180-182).

In social constructivism, a great deal of attention has been paid to the construction of scientific facts, since 'science' is a broad category of knowledge production that holds great status and power in modern society. Most Sociology of Scientific Knowledge (SSK) studies have focused on studying the 'hard' laboratory sciences, such the construction of scientific facts in subjects like particle physics or chemistry. But 'science', by its most inclusive social constructivist definition, is simply "*the production of convincing knowledge in modern society*" (Marks 2009: 2, emphasis in original), and subjects like archaeology fall under this definition. By *production*, social constructivists argue that 'science' is not a passive exercise or activity; rather, scientific methods and knowledge are the end result of some constructive and active social process. By *convincing*, they highlight the fact that scientific interpretations must be first accepted by others in the scientific community before they become facts: the establishment of scientific 'fact' is an active process of argument and convincing, not mere discovery or the passive emergence of objective truths. Finally, by *knowledge*, they mean: "reliable information about the universe...if it were wrong too frequently or too egregiously, it wouldn't be very reliable. So science is information about the universe that comes with some source of authority behind it" (Marks 2009: 4). This last point—which targets an interest in how *authority* is vested in scientific acts—is perhaps most relevant to this thesis, which focuses on how authority is embedded in the production of archaeological accounts of the past.

In many ways, archaeology is much more public and openly witnessed academic field than laboratory science, and it is most certainly a 'social science' in comparison to 'hard' sciences like particle physics or organic chemistry (Holtorf and Drew 2007; Moshenska 2009). However it is still a discipline that endeavours to produce accurate and reliable knowledge about its subject of study, and like any hard science, archaeology is an arbitrary system of classification based on social context (Durkheim and Mauss 1963). Archaeology is a system of classification, a discipline that endeavours to produce reliable knowledge about the world, and it promotes a unified system of methods to maintain a sense of order that will help its practitioners better reach reliable conclusions.¹⁴ Archaeology is, in this sense, a science. Therefore, much of the current social constructivism research coming out of science studies is very applicable to deeper study of the archaeological process.

¹⁴ See Sections 5.2.1 and 6.2.1.

2.2.5.1 *Archaeology from a Social Constructivist Perspective*

The fluid, messy and social process of scientific activity can be observed through the movement of materials (Law 1992). In a field like archaeology, fluid social practices—like excavating or developing museum exhibitions—stabilise into new material products, like texts, physical reconstructions, illustrations or museum displays. STS researchers have referred to this process ‘inscription’, and they have called the new material products created from scientific activity *inscriptions* (Latour 1999: 306-307). The ultimate aim of conducting scientific practices is to create new material forms of knowledge. Inscription involves “all the types of transformations through which an entity becomes materialized into a sign, an archive, a document, a piece of paper, a trace...They are always mobile, that is, they allow new translations and articulations while keeping some types of relations intact” (Latour 1999: 306-307). Pivotal activities of archaeological work involve the production of inscriptions like notes, drawings, images, texts and databases. Inscribed ‘end-products’ of archaeological practice often take the form of texts, reconstructions or displays.

This process of inscription is closely related to another STS concept called *translation* (Latour 1999: 311). Translation “refers to all the displacements through other actors whose mediation is indispensable for any action to occur...actors modify, displace, and translate their various and contradictory interests” (Latour 1999: 311). In scientific activity, various actors and objects can gain, lose or impart authority in the way they negotiate materials and interact in a given network. Translation is the process where individuals interact with one another, with inscriptions and with other material, negotiating their own relationship to that actor or object, and maximising their material situation in a network to their greatest advantage. Bruno Latour loosely uses the metaphor of a rugby game to further explain the process of translation:

The construction of facts, like a game of rugby, is thus a collective process. Each element in the chain of individuals needed to pass the black box along may act in multifarious ways: the people in question may drop it altogether, or accept it as it is, or shift the modalities that accompany it, or modify the statement, or appropriate it and put it in a completely different context...all the actors are doing something to the black box. Even in the best of cases they do not simply transmit it but add events of their own by modifying the argument, strengthening it and incorporating it into new contexts. The metaphor of the rugby game soon breaks down since the ball remains the same - apart from a few abrasions - all along, whereas in this technoscience game we are watching, the object is modified as it goes along from hand to hand. (1987: 104)

Both of these processes—inscription and translation—are critical concepts in social constructivism, and they are extensively discussed in Chapters Three and Four of this thesis (Section 3.2.1.1 and 4.4).

For now, it is useful to illustrate ‘relational materiality’ and stabilisation of inscription and translation in social constructivism through the example of the 2009 Çatalhöyük Archive

Report. At the end of each field season, the Çatalhöyük team produces an Archive Report which they first publish in print text, as per academic standard, then later publish more widely in digital form on their public website. The Archive Reports are intended to summarise the work of the team's most recent field season; they detail the excavation work that occurred, highlight any notable finds or features found that season, and offer detailed reports of work done in various special categories of finds such as specific reports on lithics, animal bones, bone tools or human remains. In the 2009 Archive Report, for example, director Ian Hodder's introductory section synthesises the project activities that took place during the summer field season of 2009. In the "*2009 Season Review*",¹⁵ Hodder begins with a discussion of excavation aims and ends with a summary of activities on site:

The aims of the excavation this year were to uncover some well-preserved burned buildings in the South Area of the site. We have been concentrating our work in this area in order to understand the development of the site through time...There are of course changes that lead up to Level VI, but the fires at the end of this phase seem to be associated with an important shift in the pattern of occupation.

Some of the buildings burned in Level VI are very well preserved. The walls of some of these buildings have been found standing over 3m high. In one of the burned buildings, Building 79, we found a beautiful stone figurine of a bearded man as well as another stone figurine. [...]

The 2009 season ran from the 10th June to 2nd October. We had again a large team at Çatalhöyük this summer, -160 researchers and students of 15 different nationalities worked at the site along with 20 locals... In the one and a half months before the excavation season in 2009, the team worked on post-excavation analyses in preparation for the publication...planned for 2012, and so this season excavation reports were written and animal bones were scrutinized, and samples were taken. [sic] (Hodder 2009a: 1-2)

This summary is an account—not of the interpretations of the past, but of the methodological activities that occurred during that field season.

¹⁵ The 2009 Çatalhöyük field season is the same season that I attended for my ethnographic observation. See Chapter Four.



Figure 1: Front cover photograph of the 2009 Çatalhöyük Archive Report (Çatalhöyük Research Project 2009).

The photo on the front cover of the 2009 Archive Report might be taken as representative of the season [Figure 1]. This photo shows a large open workspace in the Çatalhöyük South Shelter. In the foreground, two excavators look down at a context sheet on a clipboard. The context sheet is a tool for recording relevant contextual information about material found in each stratigraphic layer. Directly behind these two excavators are a host of buckets, shovels, tape measures, ladders and other equipment used in the excavation process to remove soil. These tools and instruments allow excavators to physically access multiple levels of the site, as well as help the excavators grid and map the site in a virtual two-dimensional plan, like the one that the two excavators in the back left of Figure 1 are holding. This excavation plan is another tool for mapping features and recording relevant cultural material found in each stratigraphic layer. In the far-middle background, a group of excavators are at work, peeling away layers of the soil with trowels. In the very back right, a man holds a camera while he photographs the most recent layer of soil. To his right sits a Turkish workman, hired by the project to carry out most of the heavy lifting and soil sifting; he is waiting for a filled bucket to be handed to him so that he can sift the soil for artefacts in the sieves that are located behind him, out of range of the photo frame to the right. The main subject of this photo is the various actors and their tools, working to 'produce knowledge' at the archaeological site.

The rest of the photograph shows the physical site itself, the tipsy floors that have been revealed by years and years of excavation, each layer showing various archaeological levels and periods of the Neolithic. Many of the standing walls are the original white Neolithic plastered

house walls, such as the ones in the centre of this photograph, which are in the house that is being actively excavated by the men with trowels. But many of the other walls in the photograph are 'artificially' created through the act of excavation; they are walls made purely of soil, cut sections that are intentionally left in situ so that they can show the multicoloured strata in their fabric, showing each layer of occupation in profile, as per archaeological standard of good practice. The walls in the far back right, above the excavator with the camera, are examples of this.

This scene shows the full mess and mangle of the practice of scientific archaeology—where human and material, past and present, artificiality and originality, abstraction and physicality are all coming together in a snapshot moment when 'knowledge is being produced'. The 'relational materiality' (Law 1992: 5; Law 1999: 4) of this setting becomes transparent when considering how the pictured archaeologists are directly bounded in relation to the material features with which they are interacting. The archaeologists' actions are both constrained and enabled by the material they find—when they run across a wall, they follow it; when they find human remains or artefacts, they stop to carefully excavate, map, plan and disassemble them. Likewise, the archaeological material in this setting is directly affected by the actions of the archaeologists: it may be cut, angled, carried away, left in situ, propped, bagged, sieved or thrown out, depending on the archaeologists' active decisions. Furthermore, the whole landscape—the geography as well as the human and material agents—are all impacted and mediated by a host of instruments and tools. Instruments and tools actively construct the form of the material landscape in both virtual and physical space (virtual in the sense of mapping or recording before destruction; physical in the sense of alteration, such as when the trowel cuts soil). The technical tools and instruments guide and impact the actions of the archaeologists. Human excavators impact the material by touching, handling, viewing and carrying it off site. The activity here, 'doing archaeology' with the aim to produce knowledge, is a complex array of social and material relationships. The final product of this interaction is Ian Hodder's formal and stable account of fluid activity, where activity onsite is reduced and inscribed in the Archive Report to: "this season excavation reports were written and animal bones were scrutinized, and samples were taken" (Hodder 2009a: 4).

This activity is demonstrates inscription and translation. Archaeology involves the creation of new material products, such as site plans and photographs, which represent 'snapshot' moments of fluid excavation activity inscribed as new mobile forms. In this photograph for example, the archaeologists in the foreground are creating context sheets, the archaeologists in the back left are mapping a site plan, and the photographer in the rear is digitally rendering the site. These actors are all *inscribing* their fluid social activity into movable new inscriptions, representations which are later studied and used to create new texts,

illustrations and other products of knowledge. This inscriptive process is interlinked with the soil and archaeological material, with the tools that the archaeologists are using, and with a broader institutional understanding of ‘the way we do archaeology’ that lies behind these scientific processes. Each time a human actor interacts with a tool, archaeological material or an inscription, they are also *translating* their own relationship with it, negotiating their use and understanding of material space and things, and importantly—maximising the benefit and authority of this interaction. This process is discussed more in depth in Chapter Four of this dissertation. But for now, it is important to recognise how a social constructivist perspective, where knowledge is seen to be produced through social interactions and networks, hinges on the *material* nature of the scientific process.

2.2.6 Social Constructivism: Power Relations, Social Organisation and Knowledge

One of the key interests of social constructivism is the relationship between power and knowledge in social communities. ‘Social communities’ are collective entities composed of diverse social agents, many of whom may have conflicting interests, stakes and aims (Webb 2002). Naturally, a society made up of competing and conflicting interests creates a dynamic situation: nearly every social relationship in a community—between people, between people and things, even between people and ideas—involves an asymmetry of power. As described earlier, *power* can most simply be defined as the capacity or ability to bring about a certain effect, the ability to act or to affect something strongly (OED 1989). When two or more people or things sit in tandem to one another, they usually relate on some level of power and authority, through such matters as domination and subordination, influence or importance, accuracy or reliability (Foucault 1982; Doob 1983: 5). Importantly, power in society is tightly interwoven with knowledge and beliefs. When we believe in something strongly, and have the power to act on those beliefs, then we can make certain decisions that have certain effects (Gordon 1980).

Traditional sociologists have stressed the integral relationship between social structure, power, and beliefs or knowledge. The basic, traditional model is that “[t]here is social structure on the one hand. And there is knowledge on the other. Structure influences the form or the content of knowledge” (Law 1986: 3), and power relations play into this structure/knowledge relationship. Karl Marx, for example, argued that human needs and the material means of production are central to the way society is structured in class systems. He argued that conflicting interests and needs of members of a given social community cause social change, and that the power of social beliefs, knowledge and ideologies, were wrapped in and caused by social action (Marx 1888; Law 1986: 4). This social argument offers the traditional sociological

'structure influences knowledge' model. The influential sociologist Emile Durkheim developed an alternative power/knowledge model, that of empirical *a priorism*, in his scholarship on religion in society. According to Durkheim, social communities create classifications to describe an existing and empirical social reality; classifications of value, status and functionality are templates on which we build our knowledge and structure our thought: "the social, as always for Durkheim, describes a reality that is prior to individuals" (Durkheim and Swain 1915; Law 1986: 4-5). In yet another alternative, Thomas Kuhn described social knowledge production—specifically scientific practice—in terms of paradigms. He argued that people socially create paradigms of practice, which are constantly under resistance by others who "attempt to extend and exploit [them] in a variety of ways" (Kuhn 1970: 91). In all of these sociological models, the relationship between power, ideology or knowledge, and social structure is made apparent: "Structure certainly influences belief but belief in turn acts upon structure, acting to sustain it or, indeed, to change it...The notion that structure and belief are integrally related is not new" (Law 1986: 4). These traditional approaches have argued for a positive connection between the structure of social organisations, and the knowledge and ideology systems that exist in society.

However, these traditional models of power relations in society are problematic for two reasons. First, they have a tendency to question and explain power relations as existing within a 'social order', a unitary thing that operates under grand, stable social models and influences. Secondly, and as relates to the discussion above in Section 2.2.3, they talk about power as if it were something that can be possessed, a quality or a characteristic. More recent perspectives of power in society in social constructivism, however, have departed from such grand functional models or 'first principles', and they have instead focused on the complex, heterogeneous and interdependent nature of social systems. Social constructivist perspectives instead argue that "there is no such thing as "the social order" with a single centre, or a single set of stable relations. Rather, there are orders, in the plural...the effects of power are generated in a relational and distributed manner, and nothing is ever sown up" (Law 1992: 5). In other words, like traditional sociological by scholars such as Marx or Kuhn, social constructivists recognise the intimate relationship between knowledge, power and social structure; however, they depart from these traditional approaches by arguing that society operates in a much more dynamic and complex way, indefinable by neat models, instead full of negotiations, translations and heterogeneous influences.

Michel Foucault, one of the fundamental modern thinkers on the role of power and knowledge in society, argued this point: "[o]ur task is to cast aside these utopian schemes, the search for first principles, and to ask instead how power actually operates in our society (quoted in Rabinow 1984: 5-6). Diverging from earlier scholarship on social power, Foucault's research focused on social 'how' questions—how power operates in society, how knowledge

and power are linked, how authoritative social structures come to be formed—based on explanations and interpretations from observation.¹⁶ One of Foucault's primary interests lay in how humans are turned into subjects, by what he calls "dividing practices" (Rabinow 1984: 8). 'Dividing practices' involve such social acts as the isolation of lepers or the confinement of the insane from the bulk of society, and these practices directly draw on and result from power relationships, the use of 'facts' and the practice of authority within society. These practices of power, Foucault argues, are often "modes of manipulation that combine the mediation of a science (or pseudo-science) and the practice of exclusion—usually in a spatial sense, but always in a social one" (Rabinow 1984: 8). Foucault's argument that physical and social order can operate through a mediation of science or pseudoscience is paramount to thinking about how archaeological accounts come to be perceived as powerful and authoritative.

More recent proponents of social constructivist theory, mainly in the field of Science Studies (STS) have come at the idea of social construction from a somewhat related, but opposing direction from Foucault. Foucault argued for a vision of society as socially constructed, in a conceptual sense. Foucault conceptualised that all knowledge is constituted and that it is socially constructed under conditions of power. However, over the last thirty years, STS social constructivist research has extended and altered this argument to say that there are no such things as 'social orders' or models that define them; rather, social communities are heterogeneous entities made up of interrelated social networks, comprised of actors that are people as well as objects (Law 1992; Pickering 1995). Social constructivism directly relates power structures and knowledge production to the tightly interwoven and interactive networks of humans *and things*: "people are who they are because they are a patterned network of heterogeneous material" (Law 1992: 4)

In social constructivist research today, the connection between knowledge, ideology and social practice is stressed, and social order is represented as fluid—a "dialectical relationship between the person and his or her physical and social context" (Law 1986: 9). Knowledge and social structure are formed from a complex dialectic of resistance and accommodation, where social agents—both human and material—actively assert and accommodate their own interests and needs, and those of others (Pickering 1995). In any social context, the "relations in communities are in part defined by differences in knowledge, experience, and status—

¹⁶ At one point, Foucault argued that his main research objective was not explicitly to study social power: "the goal of my work during the last twenty years has not been to analyze the phenomena of power" (quoted in Rabinow 1984: 7); however, power was a primary focus of much of his research, despite the fact that he rarely used the word 'power' in many of his critical works:

"When I think back now, I ask myself what else it was that I was talking about, in *Madness and Civilisation* or *The Birth of the Clinic*, if not power? Yet I'm perfectly aware that I scarcely ever used the word and never had such a field of analyses at my disposal then." (quoted in Gordon 1980: 229)

differences in power that endlessly shift within and across social contexts” (Mortensen and Kirsch 1993: 558). Sociologists of science often stress the idea that relations like ‘power’ or ‘knowledge’ are outcomes of social interactions, rather than passive qualities that one can possess. This complex weave of power relationships, and ways of thinking about them as embedded in and products of social contexts, is integral to how SSK researchers see knowledge as constructed, perceived and ultimately accepted or rejected.

2.2.7 Social Constructivism: Transparency in Conflict and Contestation

One final concept in social constructivism that should be introduced in this chapter—and which will be further expanded in the next chapter of this dissertation—is that of contestation and the idea that tension in a process or system can allow for its internal complexities to become more transparent, a theory called ‘blackboxing’ in Science Studies. Bruno Latour (1999: 304) defined ‘blackboxing’ as a scenario where a process or system runs so smoothly and efficiently that no one stops to question its internal complexities, only its inputs and outputs, data and results. Social constructivists often talk about ‘breaking open the black box’ or ‘examining the black box’ of a given system by studying scientific practice that is under conflict or contestation. The theory of contestation as a theoretical tool in science studies is that, when contention or conflict arises, or when something goes awry, the ‘blackboxed’ systems of practice become more transparent. Thus, in contested practice, people can more thoroughly examine the internal complexities of their own working system by breaking down the walls of a ‘blackboxed’ system through the examination of a contested case study, or by studying scientific controversies (Engelhardt Jr. and Caplan 1987; Popper 1998[1953]; Lakatos 1998[1973]). Contestation and blackboxing are methodological concepts that I discuss further in Chapter Three (Section 3.3.2), as they directly impacted my practical case study methodology. But for now, it is important to introduce this theoretical discourse, which is central to much theory in social constructivist research that addresses authority in scientific practice.

2.3 Authority in Archaeological Theory

2.3.1 Introducing Authority in the Discipline of Archaeology

This chapter has, to this point, addressed the concept of authority in relation to its general roots and conceptual meaning. This final section discusses authority specifically in

archaeological literature. Authority is fundamentally intertwined with the discipline of archaeology. The field as we know it was founded on principles of the Enlightenment, such as legitimation, convincing, trust, witnessing, logic and observance (Moshenska 2009). From the 19th century, notions of authority, witnessing and trust became cornerstone concepts in archaeological method, at base for why we accept or trust certain archaeological account over others (Trigger 1989: 91-92; Renfrew and Bahn 2000: 24). As archaeology professionalised, concern developed around the role of archaeologists as powerful practitioners who have exclusive access to important historical remains and material culture. Particularly in the last thirty years, archaeological people and institutions have begun to re-evaluate their roles in society, and the role of the discipline in matters of public identity and service. Questions and interest in disciplinary authority have developed in a number of critical areas of discourse.

2.3.2 Authority in Processual and Postprocessual Theory

Archaeological theory over the past twenty years has recognized the highly complex relationship between archaeological practice and material culture. Many archaeologists (Andrews, Barrett et al. 2000; Hodder 2000; Faulkner 2002) have encouraged reflexive methods in fieldwork, following sociological studies of reflexive practice. The social nature of interpretation in archaeological epistemology has been debated, and several scholars have urged better recognition of personal biases and assumptions in the way the past is interpreted, engendered or presented (Gero 1996; Handler and Gable 1997; Merriman 2004). This dissertation targets an important epistemological concern within this trend of archaeological research: the construction and use of individual and institutional authority in how the past is studied and represented.

In recent years, practitioners have started to question: what does it mean to be an archaeologist, and what standards must one uphold in order to be a professional doing 'best practice' in the discipline? Alison Wyle writes that, "From the mid-1950s on, a vocal contingent within the SAA [Society for American Archaeology] has argued the need to codify professional scientific standards of practice, specifying 'who an archaeologist was and what that person was qualified to do'" (McGimsey 1995: 11; Wylie 2002: 229). Such institutional discussion appearing on both sides of the Atlantic, aiming to delineate or categorise who is an archaeologist from who is not, and aiming to understand the professional or scientific obligations behind this role, have resulted in archaeologists reconsidering their own roles in society.

Early discussion about archaeological authority coincided with the wave of New Archaeology theory that developed in the 1960s, driven by anthropological studies in America (Caldwell 1959; Binford 1962; Binford 1965). New Archaeology was concerned with identifying

processual changes and cultural regularities in the material record. This wave of theory stressed the scientific and objective potential of the discipline, reacting against the more imaginative and interpretive Culture-Historical practices that existed before (Trigger 1989: 295). The New Archaeology's explicit concern was in creating new standards of practice, rather than engaging in self-examination or in deconstructing existing archaeological methods (Meltzer 1979). It did, however, have a general interest in taking a critical and deliberate turn away from the Culture-Historical approaches, which relied heavily on archaeologists who were seen as 'authorities' holding expert status in various institutions. New Archaeologists argued that archaeology should aspire to be an objective science, that functionalist and processual trends were of central importance to archaeological practice. The aim was "to be able to produce objective, ethically neutral generalizations that were useful for the management of modern societies" (Trigger 1989: 313). New Archaeology also opened the discipline to numerous other fields of study: "from human geography, economics, political science, sociology, and psychology, as well as ethnology" (Trigger 1989: 373). In other words, New Archaeology reinforced disciplinary and institutional authority as part of wider empirical discourse, while simultaneously questioning the authority of specific individuals in the creation of a general, objective vision of the past.

Starting in the 1980s, a reactionary wave of theory called postprocessual archaeology appeared in academic discourse, deeply situated within a larger academic trend of postmodernism. In general academia, postmodernism has never been a coherent theory about society or research; instead, it involves a variety of theoretical approaches (such as postcolonialism, feminist critiques, phenomenology, poststructuralism, hermeneutics) resulting from self-aware, critical academic debate about the role of individuals, social dynamics and organizational politics of intellectualism (Bauman 1987; Butler 2002). Specifically in the field of archaeology, postprocessual theory first appeared in the early 1980s as a critique to the 1970s New Archaeology. It "aimed at a redefinition of social practice, social units and groupings, and of the nature of culture, all seen to be the heart of a social archaeology aiming at the reconstruction of societies on the basis of their material remains" (Shanks in press: 4).

Postprocessual theory has stressed the arbitrary nature of archaeological interpretations, raising important issues about the social nature of archaeological practice. Postprocessual archaeology has included debates on the impact of personal, cultural or social bias on interpretations, and has cautioned about the dangers of silencing the voices of past and present peoples in a postcolonial world (Bahn 2001; Shanks in press). As Alison Wylie argues, archaeologists have found themselves sitting uncomfortably between their 'scientific' role of advocating the "ideal of professional disengagement" (2002: 229), and the conflicting reality that archaeologists act within their own self-interest, exploiting the material record for their own goals and aims. She explains that archaeologists have "a commitment to scientific goals

[that] provides the justification for archaeological conservation politics and salvage efforts” (2002: 229), but that these goals of archaeology-for-the-sake-of-knowledge are often dissonant from the reality of a “pervasive, often indirect and unintentional, entanglement of professional archaeology with commercial interests in archaeological resources”, and that these goals sometimes run counter to other public interest groups who might “object that they are not served by scientific exploitation of the record” (2002: 229-230).

In the last ten to twenty years, the discipline has recognised the impact of socio-politics on interpretation, and in turn, recognised how communities are affected by archaeology. Discussion has emerged in a number of intellectual arenas. Postmodern social theory has addressed fields of discourse such as gender studies, pluralism, postcolonialism, structure and agency (Gero and Root 1990; Gero 1996). Theories of reflexivity (Hodder 2000; Hodder 2003), critical archaeology (Leone, Potter et al. 1987; Leone 1992; Wilkie and Bartoy 2000; Leone 2010), archaeology as situated practice (Shanks and Tilley 1987), and community or collaborative archaeology (Moser, Glazier et al. 2002; Kerber 2006; Walker forthcoming, 2011) have all engaged in debates over what it means to be an archaeologist working in a social context that might impact or bias how we approach the past. From these, debates around the value, identity and access of archaeological heritage have emerged in fields such as public archaeology, heritage and museums studies, and archaeological theory, with a particular focus on a push for multivocality and the concepts of protection and stewardship of archaeological remains (Kirschenblatt-Gimblett 1995; Lowenthal 1998; Skeates 2000; Howard 2003; Holtorf 2005; Smith 2006; Sorensen and Carman 2009). There has also been a deepening awareness of the issues surrounding presentation, with debates over nature of museum displays, the biases and hidden meanings that might advertently or inadvertently appear in archaeological images and imagery, the socio-politics behind popular-culture representation of archaeologists, and the paradoxes and complexities that exist behind the concept of authenticity (Karp and Lavine 1991; Holtorf 2005; Smiles and Moser 2005; Perry 2009; Moser 2010). Many of these archaeological studies have attempted to address how the researcher affects the ‘final product’ archaeological interpretations that are ultimately produced through his or her engagement with archaeological practice. These various theoretical schools are rooted in a postprocessual, or even arguably a ‘post-postprocessual’, wave of academic theory. They stress themes of multivocality and reflexivity, pressing for greater awareness of how social contexts can affect the outcomes of data collection and interpretation.

Multivocality and reflexivity are two theories that feature in many of these postprocessual debates, and both firmly stake an interest in the notion of authority in archaeological practice. These two postprocessual theories were developed in the growing recognition that archaeological sites and research have multiple stakeholders with varied

interests in the past. Multivocality literally means ‘many voices’ and is an ethical argument that archaeologists should provide a stage for subaltern groups to voice their own interests or interests in the past (Hodder 2008). Reflexivity is a methodological argument that asserts that scientific practice should be self-aware and accountable to its own contextual development and method (Hodder 2000; Tsekeris 2010). Multivocality and reflexivity both address the question: “how should we respond to the fact that so many groups want to tell different stories about the site?” (Hodder 2000: 4). They offer what Hodder calls “positionality”, an admission that one’s own position and biases affect interpretation; they are a critique of and enquiry into taken-for-granted assumptions about what knowledge is and how it is formed (Hodder 2003: 58). With reflexivity, stress is generally placed on the act of self-examination or self-reflection, with a deeper questioning about what social assumptions or biases may exist in methods or standard ways of thinking. With multivocality, the focus is on “changing practices and contexts so that disadvantaged groups have the opportunity to be heard and responded to. It involves trying to move away from the methods and principles that are attuned to the Western voice. It involves ethics and rights” (Hodder 2008: 196). Both of these theories, often interlinked in postprocessual discourse, engage directly with the notion of authority: they question who has the power to speak for and about the past, and highlight how powerful biases can impact the archaeological record.

In all of the theoretical schools and studies expressed above, there is a common underlying theme of authority, as it relates to social asymmetries that might affect archaeological interpretation. As archaeologists have recognized their own contextual and contingent position in society, they have also been forced to renegotiate their own actions and decisions, thinking deeply about the impact of the discipline on the material they study and on other interest groups around them.

2.3.3 Authority in Archaeological Subdisciplines

Three archaeological subdisciplines are of particular interest to this thesis and worthy of note. These subdisciplines directly engage with the notion of authority as it affects archaeological practice and interpretations, and they directly relate to the question behind this thesis: what is archaeological authority, and how does it impact the production of archaeological accounts of the past?

Historiographic analyses of archaeology have become more prevalent over the last thirty years, and authority has emerged as a primary concern of researchers in this subfield. The popularity of interest in the history of archaeology can be seen in recent projects such as the development of the History of Archaeology Research Network (HARN), the Archives of

European Archaeology Project (AREA) funded by the European Union, as well as the steady appearance of historiographic studies in publications and conference papers. Currently, the journal *Antiquity* also informally reserves a section for the publication of studies in the history of the archaeological discipline (Farrington 2009: 294). Such projects have focused on not only the history of major and minor figures in the field, but also address the historical impact of archaeological practice on the wider public in both social and political terms (Trigger 1989; Farrington 2009: 182; Smith 2009). As Farrington writes about historiography and the impact of archaeology in the modern day state of Israel:

A historiographic perspective also enables investigators to understand how a site came to be as it is in terms of academic literature and public presentation; in other words, how the site was created as a site. It allows the investigator to be aware of power structures within the discipline, and to be aware of how text creates history. (Farrington 2009: 182)

Historiographic perspectives have opened the discipline to scrutiny and the examination of its own practices, deconstructing power relationships and the origins of the discipline's authority (Stout 2008). By studying how the profession has developed and by identifying the motivations, biases and power relationships that are entangled with professional status, the concept of authoritative relations have become more visible in archaeological practice. It is perhaps unsurprising that matters of authority have been a primary interest of archaeological historiography, since the notions of expertise, witnessing and institutional stature have played a major role how the discipline has developed.

Archaeological ethnography has also been a growing subfield in archaeological theory, and many studies have highlighted concerns of authority in archaeological methods and practice. Most ethnographies of archaeological practice go beyond the activity anthropologists observing and reporting archaeological activities, although studies of this type have been done (Hamilton 2000; Erdur 2008). Rather, the ethnographies of archaeological practice becoming more prevalent today are: "a trans-disciplinary or even a post-disciplinary and transcultural space for engagement, dialogue and critique...It does not so much aim at combining and mixing archaeological and ethnographical practices" (Hamilakis and Anagnostopoulos 2009b: 73). In general, archaeological ethnographies have sought to deconstruct archaeological practices that have become 'blackboxed'. They attempt to look at excavation, report writing and other archaeological methods with fresh eyes, observing the way archaeology operates within a social context: "the ways in which [archaeology] is created and produced *through* particular relationships, people, things, and practices" (Yarrow 2009: 21). Several studies within this subfield have offered new insight about the way archaeological practices are organised, structured and institutionalised, as well as the way people learn archaeology in practical setting (Gero 1996; Hamilton 2000; Meskell 2005; Edgeworth 2006; Van Reybrouck and Jacobs 2006;

Erdur 2008; Hamilakis and Anagnostopoulos 2009b). This field, like archaeological historiography, is a self-examination of archaeological standards and settings of practice. Issues of authority, power, and identity emerge as concerns when researchers study hierarchical chains of command, student-teacher relationships, and the methods and meaning behind concepts like archaeological expertise.

Finally, *archaeological heritage and representation* has also been a rapidly expanding subfield of interest in the discipline, and authority has played an intimate part of its discourse. The politics of display is a subject that has profound impact on archaeology, since aim of most archaeological activity is the production of public texts, museum exhibitions or reconstructions. For many members of the public, museums, media and other 'authorised' forums of display reflect a pure and simple authority or truth about the past, for these institutions are considered legitimate cultural storekeepers of knowledge (Falk and Dierking 2000; Hein 2000). Recent museological studies have aimed to demystify the museum by investigating the politics of display and representation (Karp and Lavine 1991; Moser 1999; Moser 2010). A number of other studies have addressed the power and presence of archaeological images (Molyneaux 1997; Smiles and Moser 2005), and "archaeologists now speak of pictures as theory-laden, knowledge-generating contentions which structure perceptions of—and archaeological practitioners' engagements with—the past" (Perry 2009: 109). Expanding recognition about the power and politics of display has also emerged regarding other representative activities of archaeological practice, like the creation of maps or site plans (Bateman 2006; Flexner 2009), as well as physical reconstructions and historic villages (Jameson 2004; Garden 2009). All of this recent work has been directed at reorienting the way we think about objects, images and the role of the researcher in archaeological display and representation, critiquing power relationships in archaeological interpretation and practice.

2.4 Chapter Conclusion: But What is Authority in Archaeological Practice?

It is critical to point out that, while authority has been raised as a critical concern in the discipline of archaeology in so many previous studies, rarely, if ever, has the root concept of authority itself been explicitly deconstructed. Most studies that have dealt with authority and power relations have focused primarily on describing the innate power structures within excavation practices (i.e. Gero 1996), or explaining the ethical dangers of blind professional

authority in the presentation of material (i.e. Perry 2009). This thesis extends this discourse by contributing a study on the exact mechanisms and processes which constitute authority, exposing what the term 'authority' actually means in an archaeological context. This dissertation is founded on the premise that archaeological knowledge is socially constructed, and it is concerned with the way in which authority manifests in archaeological organisation, methods and practice. The role of this research, represented in the remainder of this dissertation, is to expand an understanding of how archaeological 'facts' are constructed, explicitly looking at how and why some archaeological accounts come to be valued as more or less authoritative.

This chapter has introduced the concept of knowledge as a socially constructed enterprise. It has deconstructed the term 'authority' as it has been used in traditional scholarly research, and it has offered a new way of thinking about the production and utilisation of authority: as an accumulative affect and an outcome of many different negotiations and translations by people and things in a social network. The next chapters of this thesis explore this concept in detail. Chapter Three introduces the two case studies that this dissertation uses to demonstrate authority in archaeological practice, and it also introduces the methodology that was used for this study. Chapters Four and Five analyse the practice of two archaeological projects in order to illustrate the mechanisms and processes that lie behind the production of archaeological authority and authoritative accounts of the past.

CHAPTER THREE:

Methodology and Case Studies

"If this is an awful mess... then would something less messy make a mess of describing it?"
(Law 2004: 1)

*"It was six men of Indostan
To learning much inclined,
Who went to see the Elephant
(Though all of them were blind),
That each by observation
Might satisfy his mind."* (Saxe 1878[1873])

3.1 Introduction

3.1.1 Introducing Methodology

One of the main experiments of this study has been the construction of its research design. Authority is a conceptual abstraction. How does one design a practical study to analyse a conceptual abstraction? Moreover, how does one examine the way authority impacts another conceptual abstraction—knowledge? The answer is that these conceptual abstractions produce and impact a variety of material culture and social residue. The relationships between social interactions and the material products they produce can be observed and understood even if the actual abstractions themselves cannot be quantified or observed. For this study, in order to study social and material ‘side-effects’ of authority and archaeological knowledge, I relied on an interdisciplinary range of research strategies and methodologies, drawn from research schools such as the Sociology of Scientific Knowledge (SSK) in the field of Science and Technology Studies (STS) and from subfields like Archaeological Ethnography (Latour and Woolgar 1986; Latour 1987; Law 1992; Gero 1996; Hamilton 2000; Yarrow 2003; Law 2004; Edgeworth 2006; Rountree 2007; Hamilakis and Anagnostopoulos 2009a).

As a social scientist trained in both anthropological archaeology and heritage management, my field encounter studying present-day ‘authoritative archaeological practice’ has been a unique interdisciplinary experience, taking a rewarding, sometimes frustrating, and quite personal journey through qualitative methodology. My use of method has been a complicated exploration, involving constant negotiations and

renegotiations with various methodological approaches. This dissertation is the interdisciplinary product of my literary research and writing at the University of Cambridge, my attendance at numerous conferences and presentations hosted by both professional and alternative/amateur archaeologists, my fieldwork observing amateur archaeologists in Bosnia- Herzegovina, and my fieldwork observing professional archaeologists at Çatalhöyük in Turkey. This chapter addresses my methodological process, identifying the direction I ultimately took with my methodology, and it examines the outcomes and effects that my choices may have had on my overall research product. This chapter is divided by two methodological themes: the first is a theoretical model that guided the way I conceptually approached my research; the second is the practical way I approached my fieldwork study. These two themes are interwoven in three sections. In the first section, I identify my two case studies and discuss the purpose of using a case study based approach to examine authority in the archaeological process. The second section addresses the theoretical frameworks and considerations that played a major role in the development and implementation of my research design. The third section outlines the practical methodology and strategies that I used in the process of my fieldwork.

3.1.2 Introducing a Case-Based Methodological Approach

I focused my methodology on the observation of authoritative structures that manifest in decision-making, interpretation and production of knowledge in archaeological practice. This study pays particular attention to the produced ‘final product’ accounts¹⁷ and presented interpretations of what happened in the past, with an aim of ‘tracing back’ the social history of how these accounts came to appear in their ‘final’ presented forms. As explained in more depth below,¹⁸ this study is operationally based on the idea that contestation and tension in a given process allow for its internal complexities to become more transparent. It also relies heavily on the underlying argument that social abstractions like ‘authority’ and ‘knowledge’ can be identified and understood by studying the social interactions, networks and material culture which are produced by these conceptual abstractions. Therefore, this study is framed around two practical, comparative case studies, both of which are involved in various levels of interpretive contestation: the Bosnian Pyramids in Visoko, Bosnia-Herzegovina and the Çatalhöyük Project, Republic of Turkey. The archaeological accounts produced by both

¹⁷ See Section 1.1.2.1 for a definition of a ‘final product’ account of the past.

¹⁸ See Section 3.3.2.

of these case studies are the main focus of examination, and the authority that manifests in social interactions and that impacts the production of these accounts is the primary subject of this research.

To maximize the comparative value of my case studies, I chose one case of professional archaeology and one case of alternative archaeology. Both of these sites have produced their own 'authoritative' accounts of the past through their practices, publications and public presentations. The first case study is Çatalhöyük, an internationally regarded professional archaeological site located near Konya in the Republic of Turkey. Çatalhöyük is a complex Neolithic tell site with an equally complex excavation history and legacy. The Çatalhöyük site was partially excavated in the 1960s by James Mellaart with the British Institute at Ankara, then reopened again in 1993 by Ian Hodder, first with the University of Cambridge and later with Stanford University. Today, Hodder continues research at Çatalhöyük, extending his own excavations and encouraging researchers from other universities to collaborate on-site with their own independent excavations. Çatalhöyük presents a unique opportunity to engage with the issue of authority and authoritative archaeological practice, especially regarding the kind of interactive authority that builds with translation and site structure. The site has a deep-layered excavation history and holds an important place in archaeological history. Open almost any introductory archaeology textbook today, and you are almost certain to find a reference to Çatalhöyük or Ian Hodder. The site has a unique authoritative status in the archaeological community, and its influence on archaeological thought, in relation to its actual impact on archaeological practice, is nuanced and complex.

The international recognition of Çatalhöyük in archaeological theory can be divided by two general themes: first, the site has sensational archaeological finds, which have been matched by a few equally sensational interpretive accounts of the past produced by the primary site excavators. Secondly, the site under the current direction of Ian Hodder has been situated at the forefront of an 'experimental' exercise in postmodern theory and practice. Hodder, considered by most in the academic community as the leading figure in 'postprocessual' archaeological theory, has bound his theoretical arguments into his practical excavation of Çatalhöyük. Due to the currency of Hodder's theoretical ideas and experimental practices, Çatalhöyük's place as an 'authoritative' postprocessual site holds a high degree of status and prestige in academic archaeology, and a great deal of contestation has developed around this attention. It is this authoritative status, and the contestation that has developed from Hodder's postprocessual theoretical agenda, that is of interest of this thesis. By examining

Çatalhöyük and its excavators' authority, this case study offers a more nuanced view of how authority manifests and develops in professional archaeological practice. This case raises important questions about the nature of archaeological interpretation that go beyond simply asking how does one identify executive power, offering a research opportunity to engage with a deeper understanding about the nature of epistemic authority and how this connects to executive authority and structural space within a particular discipline.

The second case study in this dissertation is a site of alternative archaeological practice called the 'Bosnian Pyramids', located in the small town of Visoko near Sarajevo in the current Balkan state of Bosnia-Herzegovina. This project represents an interesting dynamic: on the one hand, it has been labelled as 'pseudoarchaeology' by the mainstream professional archaeological community and thus is considered to produce non-authoritative accounts of the past by those who consider themselves authorised professional experts. However, the project defies convention. Because of its role in wider Balkan socio-politics and its performative methods which draw on science as a master discourse, it is approached and treated like an authoritative site by many—if not most—of the Bosnian public, by various marginal groups in the wider international public, and by a sizable number of accredited international scientific professionals. This case demonstrates how people in search of or 'in possession of' authority can turn into powerful consumers and producers of authoritative goods. Importantly, it addresses the fact that authority can be mimicked and performed, and how people often make deliberate choices in how to perform, seek out or undermine authoritative people, things or knowledge. This contested site offers transparency into the way authority operates, giving insight into why some aspects of archaeological presentation, performance and socio-politics may lead certain accounts of the past to be accepted or assumed valid.

3.1.3 Chapter Themes and Structure

The following section of this chapter offer the methodological considerations and sources behind this research, and they address the central methodological theme of 'contestation' which drove the choice of case studies (Section 3.2). Section 3.3 identifies the aims, delimitations and background behind this dissertation's two case studies, and it identifies the practical approach that guided the collection of data and general fieldwork of this study. This section also identifies the resolution of ethical issues, as well as limitations and difficulties that occurred during practical fieldwork. The

conclusion of this chapter (Section 3.4) summarises the aims and approach expressed in this methodology.

3.2 Methodological Considerations

3.2.1 Methodological Sources

This section outlines some of the main the methodological sources and considerations I used in my practical approach. I drew on several sources both inside and outside of the field of archaeology as useful models and theory to frame my practical methodological approach. The primary aim of this study was to identify what turns archaeological accounts from simple ideas and observations into ‘authoritative’ factual accounts about what happened in the past. Authority and power relationships are conceptual abstractions, therefore I designed this study so that I could observe them through the social interactions and material produced in archaeological practice. All of the material and social aspects involved in the production of archaeological knowledge—from archaeological recording and mapping, excavated material from the past, publications and presented presentation slides, to the social interactions that used these ‘products’, such as interactive performances given during lectures and presentations to the public, the behaviour of archaeologists as they excavated and interacted with material, the social use of space and social interactions—were my research ‘archive’ from which I drew my research ‘data’.

I arranged my practical fieldwork around the central question: how does an account of the past develop, and what is the role of personal and institutional authority in this process? All of my qualitative research methodology was oriented around this question. All related research questions emerged in the field and during later literary research at Cambridge. In order to approach my research question, I needed to identify what makes an account, or any item or person in the archaeological process, authoritative? How are data and information negotiated, interpreted and reinterpreted in the process of ‘discovery’? How are data accounted for and manipulated in the process of study? How does that data end up in the format of a ‘final product’ authoritative account, such as a slide on a conference PowerPoint, or as a statement of fact in a tourist brochure? To answer these questions, I concentrated my analysis of authority in two arenas of archaeological practice: (1) the practical acts in the field, laboratory, classroom or writing desk that lead to the production of accounts of the past; and (2) the presentations of ‘final product’ accounts of the past, whether active (such as

a conference presentation), or passive (such as a printed document). I targeted and followed specific 'final product' accounts in my case studies that I thought were illustrative to my overall thesis. I drew from two major theoretical frameworks in order to develop my practical fieldwork methodology. The first was derived from methodological discussions offered by Sociology of Scientific Knowledge; the second I drew from the developing subfield of Archaeological Ethnography.

3.2.1.1 Science and Technology Studies (STS), Material Inscriptions and Translations, and the Actor-Network Theory

As discussed in depth in Chapter 2 (Section 2.2.5), social constructivists in Science and Technology Studies (STS) have argued that knowledge is produced through complex, interconnected social networks. From the early 1980s, many STS researchers contributed to a central research concept where "the social construction of knowledge, that is, the problem of how decisions about the credibility of knowledge claims and methods involve a mix of social and technical factors" (Hess 2001: 234). In this approach to better understand science as a social and technical enterprise, researchers turn inward. They ethnographically observe the physical and material movements of scientists engaging in the practice of science itself. Since focus is placed on the way evidence and facts are contingent on social events, researchers study local decision-making processes that materially develop through scientific acts: the production of texts, the use of scientific tools and laboratory equipment, as well as the movements of people themselves operating within their physical landscape. This body of scholarship has engaged a wide array of methods and epistemologies in order to study sociological aspects of knowledge production, including historiographic, sociological, ethnographic and ethnomethodological approaches. In my own research methodology, I have drawn from many of these examples. For instance, Bruno Latour's observational fieldwork methods (1986; 1987; 1988; 1999; 2003) were particularly insightful in the construction of my own methodological design. In the now classic study in the book, *Laboratory Life* (1986), Latour and Woolgar ethnographically observe scientists at work, and these observations methodologically inform their conclusion that science is a socially constructed practice. Researchers like Andrew Pickering have engaged historiographic and sociological methods to study how ideas developed in laboratories become socially established as scientific fact (Pickering 1995). Others, such as Star and Griesemer (1989), have used ethnographic and literary methods to study the way material things can become representations or tokens of meaning for different social groups.

The Actor Network Theory (ANT) has been a particularly lasting methodological contribution among such approaches. ANT was developed by STS researchers as a practical way to examine and think about the production of knowledge. ANT is a method for studying general social processes and outcomes. Latour, one of the founding theorists of ANT, states that it is a method “about how to study things...Or rather how to let the actors have some room to express themselves” (2003). John Law explains further, “Here is the argument. If we want to understand the mechanics of power and organisation it is important not to start out assuming whatever we wish to explain” (Law 1992: 2). In other words, ANT begins by a researcher looking at a given process with ‘fresh eyes’, ethnomethodologically observing actions like ‘science’ taking place in a lab or in the development of a museum display as if the researcher has never seen the process before, with no assumptions about the reasons for the social interactions that lead to its development. Power relations are one of the principal discussions in ANT research: “analysis of ordering struggle is central to actor network theory” (Law 1992: 5). One of the core assumptions of ANT is that power and authority are the result of accumulated, derived social interactions; they are accomplishments or outcomes of social interaction, not possessable things. Law argues that “we should be studying how this comes about – how, in other words, size, power or organisation are generated” in a relational and distributed manner (Law 1992: 2) by exploring and describing the “local processes of patterning, social orchestration, ordering and resistance” (Law 1992: 5).

John Law states that ANT stresses two important points about the social production of knowledge: *relational materiality* and *performativity*. By ‘relational materiality’, Law explains that ANT “takes the semiotic insight, that of the relationality of entities, the notion that they are produced in relations, and applies this ruthlessly to all materials—and not simply to those that are linguistic” (Law 1999: 4). In other words, ANT diverges from theories like post-structuralism (which focuses primarily on linguistic discourse) and deliberately aims to identify how all of the processes and forums in which various actors and materials—‘entities’—are interrelated, deconstructing how they constantly engage with one another in a physical and material way that produces scientific fact. By ‘performativity’, Law explains that ANT highlights “how it is that things get performed (and perform themselves) into relations that are relatively stable and stay in place” (Law 1999: 4). In other words, ANT simply argues that the process of knowledge production involves diverse, interlinked and related entities which ‘perform’, and through the act of performance they become stabilised. A researcher looking at a complex process, like the development of a museum display, can use ANT as a methodological model to orient their study. For this dissertation,

interested in how ‘final product’ accounts of the past become stabilised as authoritative, ANT is a very useful tool for orienting ethnographic research in academic field, lab and presentational settings.

Andrew Pickering’s related studies on scientific practice also offer a useful theoretical model for research methodology. Pickering’s “basic image of science is a performative one, in which the performances—the doings—of human and material agency come to the fore” (1995: 21). In his book, *The Mangle of Practice* (1995), Pickering not only acknowledges the role of the human in the production of knowledge, but also stresses the agency that material things (such as instruments or artefacts) have on data collection and the construction of scientific fact. Particularly important to my vein of research is Pickering’s model of scientific practice as a mangled “dialectic of resistance and accommodation”, where “scientists are human agents in a field of material agency... [and] human and material agency are reciprocally and emergently intertwined in this struggle” (1995: 21). This is a point that archaeologists such as Andrew Jones (2002) and Sharon Webb (2002) have taken up in archaeological research. In Jones’s work on the social construction of archaeological fact, for example, he argues that:

[T]he material world also operates with a degree of intentionality...while the material world may be observed and interpreted in a multiplicity of possible ways, interpretations are not wholly open-ended; the nature of the material world resists some kinds of interpretation while it provides the means for others. (2002: 171)

This argument, that the material world actively influences and constrains interpretation in archaeological practice, that science is a performative process of resistance and accommodation involving various actors which are both material and human, is paramount to my own methodological approach.

Related to this argument by social constructivists—that scientific practice, people and knowledge have essential materiality—is the idea that you can actively trace such materiality by ethnographically observing the physical movement of people and things in scientific practice (Law 1992). By following the material production of *inscriptions*—the “types of transformations through which an entity becomes materialized into a sign, an archive, a document, a piece of paper, a trace” produced in scientific practice (Latour 1999: 306-307), a SSK researcher has a material base to witness and analyse the production of knowledge by scientists. By witnessing the *translation* of these inscriptions—that is, “all the displacements through other actors whose mediation is indispensable for any action to occur...actors modify, displace, and translate their various and contradictory interests” (Latour 1999: 311)—an SSK

researcher can observe the way various individuals articulate and negotiate their own power and authority in relation to the material production of knowledge. The concepts of translation and inscription, as well as the central tenant of ANT as a methodological approach, offer a conceptual framework for ethnographic study of authority in the production of archaeological knowledge

The primary usefulness of ANT and related STS methods for this dissertation is in how it draws attention to the way multiple actors engage with one another on a practical level, addressing how scientific practices move from the abstract and unstable realm of 'ideas' and 'data' into the realm of 'interpretation' and 'fact' through the stabilising act of appropriate performance. During my own research, I found ANT to be a useful model to frame my own thinking about the way I witnessed actors in the field engage in the production of archaeological knowledge, especially since ANT stresses the 'practical materiality' of how facts come to exist as 'final products'. Anni Dugdale explains in her discussion of ANT: "Committees of all sorts sit in rooms, drink coffee, and shuffle through paperwork. And it is in and through such material arrangements that decisions are made possible" (1999: 116). ANT draws attention to this *practical materiality* of knowledge production, and this perspective offers a new way of looking at the processes and social relations that lead to stabilised products, such as an authoritative account of the past published in a highly regarded journal.

3.2.1.2 *Archaeological Ethnography*

Outside of sociological philosophy, my research methodology also drew on practical methods from the subfield of Archaeological Ethnography. Ethnographies of archaeological practice practically study "the ways in which [archaeology] is created and produced *through* particular relationships, people, things, and practices" (Yarrow 2009: 21, emphasis in original). Several studies in archaeological ethnography have offered new insight about the way archaeological practices are organised, structured and institutionalised, as well as the way people learn archaeology in practical settings. I drew my own methodology from such studies (Holtorf 2002; Webb 2002; Yarrow 2003; Bateman 2006; Edgeworth 2006; Erdur 2006; Holtorf 2006; Van Reybrouck and Jacobs 2006; Yarrow 2006; Hamilakis and Anagnostopoulos 2009a). For example, Van Reybrouck and Jacobs (2006) studied the socialisation and education of trainee archaeologists in a rescue excavation located in the town of Oss in the Netherlands. In this study the researchers followed Latour's actor-network theory, conducting ethnographic fieldwork in order to turn attention onto "the factual construction of social

agents”, and they used participant observation as a method to study excavations as “places where observations are turned into facts but also where individuals are turned into archaeologists” (Van Reybrouck and Jacobs 2006: 33). Lynn Meskell relates that such archaeological ethnographies are ‘hybrid’ studies in nature, as they are short interactive ethnographic studies that aim to “understand how the value of the past is calibrated across a wide social spectrum” (2005: 82); they involve “holistic anthropology that is improvisational and context dependent. It might encompass a mosaic of traditional forms including archaeological practise and museum or representational analysis, as well as long-term involvement, participant observation, interviewing and archival work” (2005: 83).

Archaeological ethnographies often draw heavily on the concept of ‘artefact biographies’. Scholars like Arjun Appadurai, for example, have examined the role of material culture in social life, arguing that “commodities represent very complex social forms and distributions of knowledge” (1986: 41). In his work *The Social Life of Things*, Appadurai argues that objects travel through different arenas of value, and that their different ‘life stages’ communicate complex context-dependent messages in a given culture (Appadurai 1986). Scholars like Igor Kopytoff have argued that consumption and exchange are communicative acts. He emphasizes the idea that objects may gain social meanings in both the process of commoditization—giving an object exchangeable meaning “for more and more other things, and...making more and more different things more widely exchangeable” (Kopytoff 1986: 73) and in a process called singularization—where “Culture ensures that some things remain unambiguously singular, it resists the commoditization of others; and it sometimes resingularizes what has been commoditized” (1986: 73). These ideas stress the social nature of both things themselves and the social categories involved in the movement of material through time, space and culture.

I drew my own methodology from archaeological ethnographies that have taken these root ideas of ‘artefact biographies’ and applied them to social-material studies of archaeological categories and practice. For example, Cornelius Holtorf has traced the ‘life history’ of a potsherd from its discovery to its final interpretation by following the sherd through complex networks of social relationships, negotiations and materialisations until it becomes stabilised as a ‘pot sherd’ in a site report (Holtorf 2002). Andrew Jones has used approaches from STS to study how ‘facts’ are created and effectively ‘blackboxed’ by archaeologists (see Jones 2002: 29-35). His ‘biography’ of ceramics from Neolithic Orkney follows Grooved ware from their site of production through their different roles of consumption—in both the past and present—until they

become accounts of the past. He illustrates how a methodological approach using STS theories of materiality and scientific practice, as well as using a study of 'biographies' of archaeological things and categories, can contribute to an analysis of how the material world operates with a degree of intentionality (Jones 2002: 103-182). I also drew on useful methods of observation used by feminist writers such as Joan Gero, who "brings science studies and related constructivist approaches together with feminist cognitive theory to examine archaeological field practice and the production of archaeological field data, ultimately to reveal how the organisation of gendered personnel in the field insinuates itself in the creation of archaeological fact" (Gero 1996: 251).

Richard Handler and Eric Gable, who studied 'history making' at Colonial Williamsburg (1997) were also helpful methodological sources. Handler and Gable's study of Colonial Williamsburg focuses on the way reconstructions of the past are produced within what they call 'social arenas'. Social arenas, as defined by Handler and Gable, are the interpretive spaces created by institutions as well as individuals, where knowledge is produced and actively performed or presented. Of particular help to my own work has been Handler and Gable's research design explicitly outlined in the first chapter of their book *The New History in an Old Museum: Creating the Past at Colonial Williamsburg* (1997: 9-27), which involved ethnographically observing individuals in these 'social arenas' of knowledge production. In order to study the "social production of museum messages" (1997: 13), the researchers observed people performing in what they called 'frontline' and 'backstage' social arenas. The researchers also accessed documentary and archival sources that were promoted as 'final product' interpretations of the past, and they attended public presentations to see public performances of 'final product' interpretations about the past (1997: 9-27). Because my own research involved two case studies that had a similar archive of data to draw upon, I found Handler and Gable's research design to be a close, practical parallel.

Previous archaeological ethnography studies have been done specifically on the archaeological site of Çatalhöyük, and they have also been of methodological worth to my own research design. Sharon Webb's doctoral research at the University of Cambridge (2002), on multiple interpretations and museum displays at Çatalhöyük, was also structured around the concept of contestation, and she directed qualitative methods like informal interviewing and participant observation at the Çatalhöyük site museum. Webb's museological study proved to be a valuable model for my own work observing researchers at Çatalhöyük at the excavation mounds and in the dig house laboratories. Several other traditional ethnographies have also been done at the site of Çatalhöyük, providing an interesting perspective from which to base my own

observations. Oguz Erdur, for example, attended and observed the site of Çatalhöyük as an anthropologist, writing a 'site diary' for his unconventional PhD dissertation from Columbia University (2008). Erdur's intellectual-literary dissertation diary provided insightful background observations on working and conducting an ethnographic study at Çatalhöyük. It is noteworthy to say that I learned some things about what *not* to do for my own methodology from Erdur's work: do not sit by the sidelines and simply watch excavators work, thereby visibly turning the excavators and specialists on site into the anthropological 'Other' or specimens. In his research, Erdur describes how his seeming lack of participation created an atmosphere akin to annoyance, if not actual hostility between the observer and observed: "in the art of sitting...I surely become a feature of curiosity too. To them, my work is perhaps like what their work is to me: far from self-evident in terms of its—grounds of legitimacy?" (Erdur 2006: 106).

A more traditional and heavily referenced example of ethnographic fieldwork from Çatalhöyük is that of Carolyn Hamilton's report on 'faultlines' between excavators and specialists in the excavation season of 1996. Hamilton conducted a limited, one-month session of fieldwork at the site during the 1996 season and observed conflict and rifts between two major working groups of the site: the field excavators and the specialists (Hamilton 2000). Hamilton's project, as well as its insightful observations about the nature of knowledge construction at the site through social interactions, was much welcomed and very supported by director Ian Hodder, and it has arguably set the stage for many of the later ethnographies which have come through the site.

3.2.2 Central Methodological Theory: Contestation

The central concept used in my methodological approach is that of *contestation*. Contested practices create a space of transparency that can allow a researcher to better observe why and how some knowledge seems to be more or less accepted as 'authoritative' by consumers of that knowledge. The idea that contestation creates a window of transparency is not new. For example, Bruno Latour argued that 'science' as a process usually operates so rigorously and efficiently that scientists rarely question the internal social complexities of their own routine actions and methods; they only question their data and results (inputs and outputs). Latour coined the term 'blackboxing' to define this process, where a model runs so smoothly and efficiently that no one stops to question its internal complexities: "when a matter of fact is settled, one need focus only on its inputs and outputs and not on its internal complexity. Thus, paradoxically, the more science and technology succeed, the more opaque and obscure

they become” (Latour 1999: 304). According to the ‘black box’ theory, it is only when contention or conflict arises, or when a process goes awry, that we can better examine the internal complexities of that working process, breaking down the walls and looking inside the ‘black box’ of our normal system of actions.

This theory of breaking down the ‘black box’ of routine by examining contested case studies is particularly fruitful when studying academic controversy from the perspective of social constructivism.¹⁹ Stuart Blume, who has studied scientific disputes, argues that “Controversies in science seem to offer a research focus permitting concurrent exploration of cognitive and broad social structural factors” (Blume 1977: 13). This approach seems especially appropriate when examining how a social abstraction like authority manifests in archaeological practice and accounts; authority by its very nature relates to social power relations and social politics. Contested practices often lead to noticeable struggles over both *executive* control and authority over something (i.e. for example, the use of lab space, the use of funding, access to physical material or space), as well as noticeable differences over *epistemic* authority (i.e., the qualifications of a researcher, the usefulness of an experimental method, the validity of an hypothesis).

Following this philosophy, arguing that conflicts in a system allows its internal complexities to become more transparent, I intentionally structured my research approach around the case studies of Çatalhöyük and the Bosnian Pyramids, which are two tension-riddled archaeological projects, as described above. Contested archaeological practices and accounts are taken as the ‘other’ in this study: they were the primary ‘subjects’ of my field research. I investigated the complex negotiations, transformations and heterogeneous acts that went into the production of accounts of the past in both case studies, and I worked under the methodological theory that contestation lays bare some of the intent behind the choices that led to ‘final’ constructed forms of knowledge. My primary aim was to identify what turned selected archaeological accounts from simple ideas and observations into ‘authoritative’, factual accounts about what happened in the past. These *methodological* sources and frameworks directly affected the way I practically approached my study, which is further discussed in the next section..

¹⁹ See Sections 2.2.5, 2.2.6 and 2.2.7 for further discussion on Social Constructivism.

3.3 Methodology in Fieldwork and Data Collection

3.3.1 Case Study Parameters: Aims and Delimitations

My two case studies were not picked at random; they were chosen to be compatible, so that when brought together in a discussion, remarks about their operation would provide meaningful conclusions in an analysis of 'authority'. The studies of the Bosnian Pyramids and Çatalhöyük help illustrate the overall research question: what is archaeological authority and how does it manifest in the production of archaeological accounts of the past? This dissertation is not simply presenting two in-depth studies of contested archaeological practice; rather, it uses the case studies as illustrative examples that contribute to an overall analysis of authority in archaeological practice. The purpose of using two very different case studies is also not to provide a universalist picture of archaeological 'types', such as 'pseudoarchaeological versus professional'. The aim is not to explicitly compare two very different case studies; they are not directly comparable and equal sites. Rather, they are complementary and demonstrative examples for this thesis for a variety of reasons.

These two case studies are compatible because of their form and appearance. Both sites are sizeable archaeological, earth-moving operations, with unusually large teams and a complex site history. Both sites are also very *conscious* examples of archaeological practice; Ian Hodder and his team's very conscious approach to interpreting and presenting the past of Çatalhöyük is well known, and this practice relates very closely to the very conscious preparation and presentation produced by Semir Osmanagić and his team, whose public publications and presentations are very mindful of building a scientific presence and, as I found during my research, very 'plugged in' to current trends and archaeological language. This similarity between two sites that are very mindful and responsive to their own interpretations, at least in appearance and performance, provides a firm foundation for a study on authority in a comparable 'archaeological' context. Both sites are also well-represented in media sources and publications, so a great deal of 'final product' accounts of the past exist for both sites. This allows a researcher a great archive of material to access and study. Many of these accounts are produced by the projects' own official organisations, but also by other people or groups who sit outside of the official team units also produce other accounts relating to these sites. This offers a chance to study how sites and individuals attempt to maintain their authority in the face of alternative or non-authoritative contestation and debate outside of the official team. Since both sites are currently ongoing projects, with regular practice and production of knowledge, both projects

afforded me the opportunity to visit and observe live production of knowledge—at the actual sites of excavation, as well in spaces where interpreted accounts were presented, such as conference venues.

These two studies can also be viewed as complementary, comparative opposites on either end of the ‘demarcation line’. In the philosophy of science, ‘demarcation criteria’ are the characteristics that scholars have “used to differentiate science from its counterfeit: if a discipline fails to meet one of these conditions, then it judged to be non-scientific” (Curd and Cover 1998: 2). The act of ‘demarcating’ or categorising authorised science is a matter of authority in itself, for who has the right to judge what is or is not counterfeit, and who has the authority to define conditions?²⁰ As Curd and Cover write, “Ultimately discriminating between science and its counterfeit depends on a detailed understanding of how science works” (Curd and Cover 1998: 79), addressing the fact that in order to understand what makes something ‘scientific’ versus what is not scientific, or to define what is ‘pseudoscientific’, one must first recognise that both science and pseudoscience are products of complex socio-political interactions and performances. The Bosnian Pyramids, as a case of pseudoarchaeology, and Çatalhöyük, as a case of professional and scientific archaeology, present different angles of archaeological debate over the construction of facts and the production of knowledge. Both sites, despite their given labels of ‘pseudoscientific’ or ‘scientific’ can be considered ‘authoritative’ in certain circles, and ‘non-authoritative’ in others, and such contestation is useful when approaching an analysis of authority. In Visoko, Bosnia, the pyramid project was initially given full permissions and political support by the national government, was treated as authentic and authoritative by many media outlets, was given support by many people with authoritative credentials and institutions behind their names, and was directed by a man who a majority of the Bosnian public considered to be an authority about the past due to his credentials and performance as an archaeologist. In comparison, Çatalhöyük is also an authoritative site, supported by the national government, as well as by numerous political and social institutions, and acknowledged by the entire professional archaeological community. Furthermore, a majority of media, the profession and the public also treat Ian Hodder as an authority about the past. This thesis, using two sites on opposite sides of the demarcation line that are both creating ‘authoritative’ accounts of the past, examines fundamental tensions behind what makes someone an authorised authority and what makes an account of the past authoritative.

²⁰ See Sections 5.2.1 and 6.2.1 for further discussion on the authority of categories and categorisation.

Also, individually, both sites also offer interesting tensions in their political, social and conceptual backgrounds, regarding the nature and origin of their authority. As mentioned above, in both projects, contestation arises over the application of methodology and standards of practice, as well as over the validity of the 'final product' accounts of the past produced through practices like excavation and presentation. In the Bosnian Pyramid case, the main contestation revolves around the disparate acceptance of the site's accounts by the archaeological community and the international public. The primary tension is over its label as and categorisation as 'pseudoarchaeology' by academics and professional archaeologists, while the general public sees the project as more or less authoritative and authentic. In Çatalhöyük, contestation frequently arises in the archaeological community regarding the site's epistemological and theoretical stance as a successful reflexive, multivocal and postprocessual site. Çatalhöyük is often quoted as an authoritative, textbook-quality example of scientific archaeological practice; however, the site represents itself as experimental and pushing the bounds of interpretive practice. This results in Çatalhöyük almost having two identities—a site of standard scientific methods versus a site of experimental practice—and certainly results in contestation over whether the site's 'talk' matches its 'action'. Contestation at Çatalhöyük has also involved disputes over public arenas and access, with conflicting interpretations coming from groups such as the Goddess Community, as well as the local government and public who have questioned who can or should have access to the site.

The epistemic contestation in the Bosnian Pyramid case study is very public, and most debate has been focused on whether or not the physical material being excavated is, in fact, archaeological at all. Debates over the project's archaeological material primarily take place on the Internet, in informal settings. In formal settings, such as conference presentations, conflict at Visoko is usually stamped out, and interpretation is stabilised by the performance of science and influence of the 'academic'. Epistemic contestation at Çatalhöyük, on the other hand, mainly takes place within professional boundaries between professional archaeologists in formal academic settings; although some contestation over 'final product' interpretations has been loudly voiced on public sidelines from alternative archaeological groups, such as the Goddess Community. Interpretation at Çatalhöyük is often 'stabilised' in informal settings, such as public museum and site displays, and public Internet forums.

As a final note, three points of awareness must be made about the compatibility and use of these two case studies in this dissertation. These points are drawn Susan Phillips' (1994: 64) study of social movements. First, I oriented my focus on the converging and differentiating elements within these studies, but allowed room for both

sites to be seen as epistemically independent. In other words, while this dissertation offers points about each case study that can be compared or offset against the other, it does not intend these sites to be seen as ‘comparable’ or ‘similar’ in any way beyond what they have to offer an analysis on authority. They are meant to be seen as compatible cases to the argument, not comparable cases in a universal sense. Secondly, I operated under the assumption that any empirical analysis of compatible case studies should be sensitive to the historical specificity of each. In other words, I approached each case study by recognising that it sits within a unique social context and academic climate, which must be addressed in order to establish a baseline for further analysis in a given thesis. Thirdly, I considered the fact that any analysis of compatible case studies should also take into consideration the “life stage” of each case study or social movement (Phillips 1994: 64). Both Çatalhöyük and the Bosnian Pyramids are in very different stages of their unique site development and in their historical situation and evolution in academia as a whole; therefore, any direct comparability is limited. However, an analysis that identifies the current life stages and social complexities of individual sites can still offer a wealth of information to a thesis which addresses them as compatible, not comparable case studies. I found that many of the issues that arise from some of the main concerns about the use of case studies and comparability can be rectified by situating each primary case study in its own individual, socio-historical and developmental context.

3.3.2 Case Studies: Data Collection

3.3.2.1 The Bosnian Pyramids in Visoko, Bosnia-Herzegovina

My initial research aim for my fieldwork in Bosnia-Herzegovina was to provide a basis for understanding of how the Visoko case study—popularly known as the ‘Bosnian Pyramids’—was situated in a complex socio-political environment in post-war Bosnia. I conducted introductory research that allowed me to identify some of the ways the Bosnian Pyramid Foundation gathered data, constructed knowledge, presented accounts of the past, controlled their image and mimicked archaeological practice in order to promote the site’s authenticity and authority to a wide public audience (Pruitt 2007). My initial two short fieldwork visits to Sarajevo and Visoko operated under standard sociological guidelines and methods, although I did have some difficulties and limitations, mostly issues regarding planning and translation (Pruitt 2007: 11-12). This round of research contact with the Bosnian Pyramids site served as a pilot study to see

what methodological approaches did or did not work, and it gave me greater awareness of the ethics involved in fieldwork practice.

This early study was based on two stints of fieldwork in the spring and summer of 2007. For further research, I spent the summer of 2008 (June-July and the first half of September) in Sarajevo and Visoko in order to complete a more in-depth study on the Bosnian Pyramid project. I collected published documents and brochures, spoke with members of the excavation team as well as with local tourist agencies and members of the public. I visited the site multiple times, both as an 'average' tourist and as an 'academic' visitor.²¹ I accessed a large volume of publicly available material through television and print media in Bosnia, as well as media presented internationally through the Internet. I also attended independent public events that promoted the Bosnian Pyramids, like the 2008 Sarajevo Film Festival, which proved very useful in my awareness of how the general Bosnian public perceived and received the pyramid project. During my months in Sarajevo, I also attended a language course so that I could develop a better cultural awareness of the Bosnian language and better recognise nuances in how the pyramid project was represented in literature and language. However, I still retained my translator from my previous fieldwork to help me translate Bosnian documents and interviews.

Over the course of the past three years, I also attended and gathered data from public presentations made by Semir Osmanagić and his team about the project. These presentations were given in formal and political as well as informal and alternative places, including: the Bosnian Embassy in London, the 'Histories & Mysteries' alternative academic conference in Edinburgh, and most importantly, the '1st International Scientific Conference of the Bosnian Pyramids' hosted by the pyramid Foundation in Sarajevo. The latter event, hosted in September 2008, was integral to my research and understanding of the Bosnian Pyramid project operation. It offered close contact with the many levels of alternative archaeological community present at the site, paved the way for many important contacts in the alternative academic arena and offered solid insight into the Foundation's 'scientific' image and practice. This event provided me with the bulk of my understanding of the 'backstage', inner workings of the pyramid Foundation. It firmly showed how the 'final product' accounts of pyramids presented in the media are, in fact, complex culminations of negotiations, decision-making and academic debate. My fieldwork on this case study helped me establish an illustrative background for how

²¹ I formally identify these two types of visits as distinct by how I represented myself to team members and volunteers on site. Depending on my visit type, I was offered very different experiences in the way the excavation team managed their image and presented an authoritative presence.

'authoritative' accounts of the past are, as Baxandall (1985) puts it, complex 'by-products of activity' and performance.

3.3.2.2 *Çatalhöyük in the Republic of Turkey*

My exposure to the site of Visoko and the case of the Bosnian Pyramids was long-term and deep, with over four years of interaction with the site and project. My exposure to the site of Çatalhöyük was slightly different in nature. While the Bosnian Pyramids is a relatively new project (operating since 2005), Çatalhöyük is, on the other hand, a project with a long, complicated history that stretches back to 1961. Because so much about the Çatalhöyük site history exists in print, and because so many other ethnographies and histories about Çatalhöyük already exist for research and reading, I arrived at Çatalhöyük with a decent understanding of the site history and operation. My goal for conducting practical fieldwork at Çatalhöyük was primarily aimed at gaining personal exposure to the actual way the site operated. By gaining exposure through participant observation at the site, I hoped to better understand how the Çatalhöyük past was being prepared for public consumption in its 'backstage' arenas. My main interest in the site was in the way issues of space, place and access played into the development of professional accounts of the past, and how materiality affected the resistance and accommodation of archaeological authority. My research goal during fieldwork at Çatalhöyük was to observe the methodological standards and approaches actually in operation at the site, and to understand how authority was translated through space, things and people. I aimed to see first-hand how alternative and non-team groups—like the Goddess Community, members of the public or academics outside of the main research team, as well as individuals or subgroups within the official team—constructed interpretations that competed for access to interpretive space. Çatalhöyük is famous for its rallying call for multivocality and reflexivity, and so one of my primary interests in visiting the site was to have the opportunity to personally observe how various voices are utilized and addressed, as well as what kind of authoritative discourses emerged through processes of negotiation in the presentation of information.

I lived and worked at the site of Çatalhöyük as an independent researcher during the summer fieldwork season of 2009. This fieldwork (five weeks in July and August) was planned to mirror ethnographies of a similar length previously conducted at the site, most notably that of Hamilton in the 1996 season, Rountree in the 2003 season, and Erdur in the 2006 season (Hamilton 2000; Rountree 2007; Erdur 2008). The fieldwork on site at Çatalhöyük allowed me the opportunity to talk with the archaeological team

and with members of the public who visited the site, as well as the chance to see the methods in operation, to observe both private and public on-site presentation of material, and to briefly participate in excavations. Like at Visoko, my fieldwork at Çatalhöyük used mixture of methods: ethnomethodological observation, the collection of documentation, informal interviews and participant observation.

Both on site and back at Cambridge, I accessed a large volume of publicly available ‘final product’ material through media sources. I also attended several presentations given by members of the academic team, most of which were given by Çatalhöyük team members and directed at diverse members of the public who were visiting the site. I also observed presentations that were given by members of the Çatalhöyük team, meant only for the Çatalhöyük team. I observed displays at the Çatalhöyük site museum (also called the Visitor Centre), and accessed site narratives presented in a variety of different forums—from those presented on the official website, to others presented in alternative settings, such as that of the ‘virtual world’ of *Second Life*.²² Finally, I also attended general academic conferences—such as the European Association of Archaeology annual meeting in 2008, the Association of Social Anthropologists conference in 2009, as well as seminars hosted in the Department of Archaeology in the University of Cambridge, in order to see members of the Çatalhöyük team formally present information about the site to the general academic community.

3.3.3 Research Strategy

In order to conduct practical fieldwork in my two case studies, I developed a mixed-method qualitative research strategy (Axinn and Pearce 2006), primarily using qualitative and ethnomethodological approaches. Mixed-method research strategies are “those that are explicitly designed to combine elements of one method, such as structured survey interviews, with other elements of other methods, such as unstructured interviews, observations, or focus groups in either a sequential or a simultaneous manner” (Axinn and Pearce 2006: 1). The main unit of study in my research program were the active producers of archaeological knowledge, including but not limited to: professional and amateur archaeologists, excavating personnel, members of the public who exercised their own agency in the production of knowledge, as well as *things* employed in the construction of knowledge such as machines, instruments,

²² A digital project operated by the associated Berkeley team under Ruth Tringham (Çatalhöyük Research Project (2010c) “Remixing Çatalhöyük.”).

artefacts or material culture, which actively influenced the authority and production of 'final product' accounts of the past.

3.3.3.1 Document Collection

One of my main research methods was document collection and text analysis. In this context, documents are defined as "any preservable record of text, image, sound, or a combination of these" which are "produced as part of an established social practice" (ten Have 2004: 4), with the idea that by using documents, the researcher engages with a consideration of some of the processes that produced them. For this research, I gathered many documents that were 'final product' accounts of the past: anything from newspaper headlines announcing discoveries or interpretations, recordings or slideshows from public presentations, images or videos that recorded archaeological finds, public brochures or tourist pamphlets, as well as site reports, scientific articles and other academic publications. During and after my fieldwork, I also collected documents that were in the process of being developed (for example, the *2009 Çatalhöyük Archive Report* and the *2008 International Scientific Conference of the Bosnian Pyramids Radiocarbon Dating Report*, which were both being actively compiled while I conducted fieldwork at the sites), as well as documents that already existed in 'final' form by the time I accessed them (for example, all previous articles and reports produced by the Çatalhöyük project team, or television reports other such visual media that aired on the Bosnian Pyramids project). Such documentary material provided most of the 'final product' accounts from which I could access, pull apart and retrace the social interactions and decisions that led to their production.

3.3.3.2 Participant Observation

While much of my case study data was sourced from a distance (i.e. collecting documentation, literature and video from media such as libraries and websites), the bulk of my understanding of the cases took place during fieldwork, at the actual excavation sites or in various public forums where team members physically presented their accounts of the past. My fieldwork primarily involved accessing the sites first-hand and personally observing field practice, accessing published documents that were sometimes available exclusively on-site, and attending the public presentations of archaeological material which could only be witnessed at the dig site itself. My fieldwork activity was ethnographic in nature, in that I was "committed to the close observation of

the actual, 'natural' situations in which people live their lives" (ten Have 2004: 6), or in this case, the natural spaces and situations in which amateur and professional academics lived out their vocations.

Ethnography, in the broadest use of the term, is not "a particular method of data collection but a style of research that is distinguished by its objectives, which are to understand the social meanings and activities of people in a given 'field' or setting, and an approach, which involves close association with, and often participation in, this setting" (Brewer 2000: 59). During my fieldwork, I described myself to those I interacted with as an 'ethnographer' of archaeological practice for a number of reasons: I gathered my data through active participation in a social environment, I immersed myself in the day-to-day processes of the people and practices I was attempting to observe and understand, I conducted series of semi-formal interviews while engaging in many of the same on-site activities of my informants, and I stressed that I was interested in observing what people 'did' when they performed actions or utterances. My ethnographic methods drew on two types of ethnographic methodology: ethnomethodology and participant observation.

Ethnomethodology is "the study of the methods people use for producing recognizable social orders...based on the theory that a careful attentiveness to the details of social phenomena will reveal social order" (Rawls 2002: 6). As a practical research method, it is designed to observe the *procedural* aspects of individual and group behaviour, such detailed physical processes, or acts of practice, and not just the final outcomes or interpretations produced through black-boxed actions. In other words, ethnomethodologists study "*overt activities*, what is 'scenic' (that is directly observable) to participants, and their intelligibility and organization" (ten Have 2004: 27, emphasis in original). For my own research, the usefulness of ethnomethodology as a method was logical and straightforward. My research aims—to identify authority in the social production of knowledge—naturally relied upon the use of a method which would help me to identify actions and processes in social organisations, and which would provide a useful platform from which to draw meaningful conclusions about social order, power relationships and authority from these observations. Ethnomethodology, as my primary research approach, provided a framework that guided the whole of my data collection. For my field research in both Visoko and Çatalhöyük, I engaged with the projects—and represented myself to people on site—as an ethnomethodologist, whose primary interest lay in observing and understanding the methods they used, as well as the actions they took, to produce accounts of the past.

In my fieldwork, I also used participant observation as a research methodology. Participant observation “involves data gathering by means of participation in the daily life of informants in their natural setting: watching, observing and talking to them in order to discover their interpretations, social meanings and activities” (Brewer 2000: 59). For ethnomethodology, traditionally the researcher is ethnographically distanced from the research ‘subjects’ and deliberately avoids any involvement or intrusion on the process being studied (ten Have 2004: 6). During my time at my case study sites, however, I quickly found that some level of personal involvement, under the method of participant observation, was not only insightful to my overall research (e.g. when I participated as a site excavator at Çatalhöyük and thus could closely observe the team’s excavating standards), but in some cases it was an absolute necessity to participate on site if I was to gain any observational access to certain people, processes and data (e.g. when I needed to register and perform as a conference participant in the 1st Scientific Conference of the Bosnian Pyramids). Therefore, I found the standards broadly employed by participant observation, as well as ethnomethodology, an ideal complement to my qualitative program.

3.3.3.3 *Informal Interviews*

Interviews are a classic staple of qualitative research (ten Have 2004; Axinn and Pearce 2006; Kvale and Brinkmann 2009). Interviewing supplements observation by ascertaining the personal views and motivations of the people who are involved in the social situation under study. In my doctoral research, I incorporated a number of informal, conversational interviews into my overall fieldwork program. Because my main research goal was to unobtrusively conduct ethnographic observation of the people and things involved in the production of accounts of the past, I did not incorporate formal interviewing into methodology, mainly because I found it to be interruptive and overly rigid for my purposes.²³ However, on many occasions I did conduct informal interviews. I found that casual, conversational interviews with people, using targeted questions that were intended to open up conversation and ascertain reasons and motives behind my subjects’ actions, was often integral to my overall understanding of the social activities that I observed.

²³ During my MPhil research at the Bosnian Pyramids site, I made a number of attempts to conduct formal interviews in Visoko with tape recorders, and I found this to be unhelpful. Most local people and team members did not respond well to being recorded. Also, the rigidity of needing to access people in one setting for a certain length duration of time clashed with the benefits of being able to grab people fluidly so that information came up organically, which I found more useful to my observation of methods and thoughts in action.

Interviewing is itself an active knowledge-producing process by which “interviewer and interviewee through their relationship produce knowledge. Interview knowledge is produced in a conversational relations; it is contextual, linguistic, narrative, and pragmatic” (Kvale and Brinkmann 2009: 18-19). It is important to note that all of my informal interviews were targeted and ‘active’, in the vein of Holstein and Gubrium’s (1995) argument that interviews are, by nature, very active acts of knowledge ‘production’, rather than passive acts of knowledge ‘uncovering’. In each interview conversation, I was active in the knowledge construction process though my suggestions of topic, questions and leads in the course of the narratives or facts that emerged through mutual interest, digression and discussion. This process ultimately led each of my casual interviews to “become a conversation, which stimulates interviewee and interviewer to formulate their ideas about the research topics and to increase their knowledge of the subject matter of inquiry” (Kvale and Brinkmann 2009: 160).

In the course of my research, I employed two distinct types of informal interviewing structures: computer-assisted and conversational. My computer-assisted interviews employed the use of the Internet and e-mail, which allowed me to converse with people at a distance, at asynchronous times. This proved to be useful in maintaining multi-national conversations over months or years. I also found that computer-assisted interviews allowed people working in controversial settings—including myself—to frame their thoughts exactly the way they wished, a point which in itself offered interesting insight about the power of presentation and the authority of accounts. The obvious drawback of this method was that it did not involve “a bodily presence with access to non-linguistic information expressed in gestures and facial expressions”, which face-to-face interviews provide (Kvale and Brinkmann 2009: 148-149). However, my second interview method—conversational—did allow access to body language.

The bulk of my interviews were conversational and primarily took place in the field. These interviews usually consisted of me, the interviewer, taking an interviewee aside for a short while and having a conversation on a specific subject or topic, usually in a casual setting such as sitting in a café or standing by an archaeological site. These interviews usually had three aims: to gain factual, conceptual and discursive information. Obtaining valid factual information was a central part of these conversational interviews; I wanted to know who the person was, where the person sat in any project hierarchy, what actions the person was taking, and what reasons or motivations lay behind their actions. These interviews were also conceptual in nature, in that I sought to understand how the interviewee conceived of a given situation or of

certain social phenomena. These interviews served “to uncover respondents’ discourse models, that is, their taken for granted assumptions about what is typical, normal, or appropriate” (see also Gee 2005; Kvale and Brinkmann 2009: 151). By asking my interviewees, for example, why they thought a given sequence of events was ‘odd’ or ‘appropriate’, or by asking for them to clarify how they define ‘respect’ or ‘accountability’, I gained interesting insight into some of the underlying assumptions and social structures that were operating on site.

In all of my informal interviews, I approached my interviewees with a short number of predetermined questions, but these were only used to stimulate discussion. By not forcing a strict regime or standard list of questions on my interviewees, it allowed all interviews to remain open and adaptable to the priorities and information that emerged during the course of conversation. None of my interviews were voice recorded or taped (as opposed to the majority of the formal presentations that I attended, which I did voice record). I found this approach to be very valuable, mainly because it preserved casual conversation and seemingly allowed more to emerge in the course of discussion. Since my research took place at contested and often controversial sites and settings, I found that, especially at Visoko, tape recorders were not conducive to the free flow of conversation.

At Visoko, many of the amateur archaeological project members were, understandably, quite defensive about their work and excavations, and they were especially wary of outsiders (especially foreigners associated with well-established universities) who tended to be hostile to their amateur archaeological activities. Therefore, I found that team members, volunteers and even members of the local Visoko community often became very nervous when I approached them with tape recorders. Ironically, many workers and volunteers on site seemed to relish the attention of cameras and video recording by local media services, and many allowed video recording from me when I was on site anonymously acting in the role as ‘interested tourist’. However, when they knew I was a researcher from Cambridge, I found that often the opposite reaction occurred: on more than one occasion, when I approached potential interviewees as a Cambridge researcher, direct communication with me was avoided entirely, and on some occasions I was politely asked not to record conversations. I also found that, even if interviewees were willing to talk with me if I agreed to preserve their anonymity or agreed not to record the conversation, just the mention of having a tape recorder with me could hamper our future discussion. Eventually, I abandoned the use of my tape recorder entirely in my summer fieldwork in Visoko, except when I attended the ‘1st International Conference of the Bosnian Pyramids’ in Sarajevo, in September

2008, when I recorded presentations. By the time I began my fieldwork Çatalhöyük in the summer of 2009, I decided to maintain the same standards of casual interview and documentation. Therefore, at Çatalhöyük, as at Visoko, my interview methodology consisted of informally conversing with members of the team and public, then immediately writing a series of post-interview notes, impressions and transcriptions directly after the conversations took place.

3.3.4 Ethical Research Guidelines and Issues

Since this dissertation qualifies as a qualitative study that impacts 'human subjects', I followed standard sociological ethical guidelines that guided my awareness and operation of: informed consent, confidentiality, consequences and the role of the researcher (APA 2002; Iphofen 2009; Kvale and Brinkmann 2009: 68). This section briefly details some of the ethical guidelines that I followed in the course of my research. Although my degree program did not require me to submit an ethical review of my work, before I began my fieldwork I observed the ethical protocols outlined in the Stanford University Institutional Review Board (IRB) for human research (HRPP 2010), and during my fieldwork I adhered to the guidelines set out by the Stanford IRB board.

3.3.4.1 Informed Consent

In all informal interviews during the course of my fieldwork, research participants were informed of the purpose of my investigations, namely that I was on site as an ethnographer interested in their methods. In all my interviews, the interviewees participated voluntarily, with verbal agreement between me and my informants that I may include their opinions in my work. Their statements, expressed in this dissertation primarily in Chapters Four and Five, should be regarded as the opinions and property of their respective owners. In any cases where conversations were overheard, or views were expressed in a non-standard or non-interview context in the course of participant observation, or in the cases where participants were aware of recording but not aware of the potential purpose or use of my investigations, this material went through three stages of observation and conditioning: first, any material that showed any potential risk of adverse affect on the speaker was thrown out and not used in my final dissertation (see 'Consequences' section below); secondly, any material that was overheard in the course of participant observation that did not pose any risk to the speaker or project is explicitly noted in my research as a 'non-interview context' and

the speaker is kept anonymous in my final work, with their identity only kept in my personal field notes; finally, this material was peer-reviewed by my supervisor and other colleagues in the course of preparing my dissertation in order to maintain another layer of review and assessment of this material.

3.3.4.2 Confidentiality

In all informal interviews, participants were made aware of the reasons for our targeted conversations, and any wishes they expressed for confidentiality were always respected. As mentioned in the section above, material that was overheard or 'picked up' during the course of my participant observation in fieldwork was also kept confidential. Especially in the case of material taken from children,²⁴ I decided to keep that information confidential if used in my doctoral work. I found my use of confidentiality to be both enabling as well as disabling: "Anonymity can protect the participants, but it can also deny them "The very voice in the research that might originally have been claimed as its aim" (Kvale and Brinkmann 2009: 73). In the case of my work, I found that respecting the confidentiality of my informants could make some of the later referencing somewhat difficult, since I have to rely on using names like "one of the excavators" or "one of the pyramid conference organisers". This makes connecting 'anonymous' people, hierarchies, organisations and ideas in my research more difficult; however, confidentiality allows for me to both ethically avoid any risk to my informants as well enables me to use their contributions in my ethnographic study.

3.3.4.3 Consequences

By following the Stanford IRB ethical guidelines, I was made aware of the potential risk and consequences of my ethnographic research on 'human subjects'. During the collection of information from the people under study, I tried to also maintain a subjective awareness about any information that was given to me, advertently or not:

From a utilitarian ethical perspective, the sum of potential benefits to a participant and the importance of the knowledge gained should outweigh the risk of harm to the participant and thus warrant a decision to carry out a study. This involves a researcher's responsibility to reflect on the possible consequences not only for the persons taking part in the study, but also for the larger group they represent. The researcher should be aware that the openness and intimacy of much qualitative research may be seductive and can lead

²⁴ In the rare cases that I engaged with children, the material was always freely given.

participants to disclose information they may later regret having shared. (Kvale and Brinkmann 2009: 73)

As Kvale and Brinkmann note, ethnographic fieldwork often involves openness and intimacy.²⁵ In cases where I later felt that one of my informants might regret something they said, especially when it might involve a given risk—i.e. later difficulties with their employers, peers, the media, etc.—I intentionally left this material out of my final dissertation, using it only to inform my own personal awareness of my case study background.

3.3.4.4 *Role of the Researcher*

During the course of my fieldwork, I maintained an awareness of my role as a researcher and the ethics that I should abide to: “Morally responsible research behavior is more than abstract ethical knowledge and cognitive choices; it involves the moral integrity of the researcher, his or her sensitivity and commitment to moral issues and action” (Brinkmann, quoted in Kvale and Brinkmann 2009: 74). One of the primary aims of my research, alongside protecting my informants, was to abide by a rigorous standard of methodology myself, having a strict adherence to scientific quality: “publishing findings that are as accurate and representative of the field of inquiry as possible. The results reported should be checked and validated as fully as possible, and with an effort toward a transparency of the procedures by which the conclusions have been arrived at” (Brinkmann, quoted in Kvale and Brinkmann 2009: 74).

These four categories of ethics—informed consent, confidentiality, consequences and the role of the researcher—were used as a framework “when preparing an ethical protocol for a qualitative study, and they [were] used as ethical reminders of what to look for in practice when doing interview research” (Kvale and Brinkmann 2009: 76). The ethics of respecting my informants, as well as respecting my own role as researcher with high standards in the ethical production of knowledge, served to guide my fieldwork methodology.

²⁵ Here I am also referencing particular occasions when things were told to me while subjects were under the influence of alcohol. While I believe the things they said to be true and relevant to my research, I am not including this material for ethical reasons, because of the later regret these individuals might have, or the risk that this information might pose to their employment if this information was traced back to them.

3.3.5 Limitations and Difficulties Encountered in the Field

Three significant limitations affect this study. First, this study is mixed-method and multidisciplinary, meaning it affects multiple disciplines within academia and has multiple potential audiences. Since this research examines present-day practice of a discipline which impacts the way the past is interpreted, it falls within the disciplinary realms of science studies, sociology, anthropology, as well as archaeological theory. Thus, I engaged with all the advantages and disadvantages of drawing upon different methodologies and theoretical links from more than one field. While I hope that this work can be insightful to all of these fields, ultimately, this work very much aimed at and intended for an audience with an interest in archaeological theory and heritage management. This is a study of how archaeology operates today, and it has most direct relevance to those who are interested in how the field of archaeology is presented, interpreted and how end-product accounts of the past are produced, which are matters of concern in the heritage subdiscipline in the field of archaeology. The primary contribution of this study regards how power relationships are developed and how authority affects the production of knowledge, and therefore this work aims to contribute to a greater self-awareness about the role of authority in the practice of archaeology today.

The issue of multidisciplinary caused some difficulties when I worked in the field. I found that many of my informants on site, both in Visoko and at Çatalhöyük, expressed confusion about my research project and aims, notably about how I was a researcher coming from the field of archaeology whose interest was in investigating methods of the present-day, not in investigating the past that they were studying. In Visoko, problems arose when I tried to explain my ethnographic interests to an amateur audience: many of the people working with the 'Bosnian Pyramids' project found the concept of doing an ethnography of archaeological practice very foreign, and they were wary of a 'mainstream' academic student watching their controversial activities. In Çatalhöyük, I also had difficulties when I expressed my project as an ethnography of archaeological practice, which was surprising to me, since Çatalhöyük has had a long history of ethnographers attend the site and report on methods and activities of the excavators and specialists. During my stay on site, especially initially, some people withdrew from socialising or interviewing with me during work hours, perhaps due to worries about misrepresentation and accountability (c.f. Berggren 2009).

After acknowledging this problem during the course of my fieldwork, I found the concept of *boundary objects* to be a useful methodological tool to cope with this difficulty. The concept of 'boundary objects' comes from the work of Susan Leigh Star

and James Griesemer, who used this concept in a study of a museum populated by people working in different social arenas (Star and Griesemer 1989). In their article, Star and Griesemer find that the “creation of new scientific knowledge depends on communication as on creating new findings. But because these new objects and methods mean different things in different worlds, actors are faced with the task of reconciling these meanings if they wish to cooperate” (1989: 388). In my own work, I used what Star and Griesemer call ‘boundary objects’, objects that have a different meaning for each social actor who engages with them, yet serve as a common denominator for discussion and cooperation in work.

In my research I found that, although members of the Çatalhöyük or Bosnian Pyramid community may not fully understand my research, they did understand when I invited them to discuss a concrete object or event. For example, when I asked a Pyramid team member for their opinions or experiences about the discovery of a certain ‘artefact’, they would understand the object and event in question and would often gladly inform me about the event. Similarly, at Çatalhöyük, I found that by focusing on specific events or artefacts—such as the discovery of a specific burial or the movement of specific ‘cluster’ material²⁶ through lab space—the team members seemed to understand my interest and gladly walked me through the process of finds handling. In both such examples, the objects and events in question were *boundary objects*. To the excavators and team members in both sites, the artefacts I asked about were part of their experience of a given event; these artefacts constituted data and evidence that informed the members’ opinions about what these objects were used for in the past. However, to me, as an ethnographic researcher, I was interested in the process and handling of the objects during and after the event described, as well as how the team member was describing and informing me about the object in the present. The handling and the descriptions of the objects were offering me ‘data’ about power relationships and sources of authority in the archaeological process. For both a given team member and myself, the discussion and handling of objects and events were meaningful;

²⁶ The use of cluster material is discussed further in Section 4.2.2.3. At Çatalhöyük, ‘clusters’ of archaeological material were not my original or intended target of research at the site. Initially, I found accessing sites and people difficult due to uncertainty about my role on site as an ethnographer. To solve this problem, I used ‘clusters’ of archaeological material as a ‘boundary object’, using one artefactual category as an arbitrary way to give me access to a variety of labs. At Çatalhöyük, clusters are by nature made up of a variety of different material: for example, human remains mixed with obsidian and faunal remains would be a ‘cluster’. By following the movement and processing of cluster materials on site, I had a way to start discussion in interviews and a reason to access different lab spaces. This way, I was able to move freely between the labs and more freely interview team members on the site, without scepticism or confusion about my aims and role at the site.

however, we were each addressing very different meanings around the same object. Thus I found the concept of 'boundary objects' to be vital to my data collection methodology during fieldwork.

Another limitation that affected my research—which primarily concerned my work on the Visoko case study—was that of language translation. Any research working with a foreign language has many inherent problems. While I have tried to minimize miscommunication by restricting my research primarily to English-speaking contacts and English literature, some translation from the original Bosnian was inevitable. I used one primary translator, Amna Hadziabdić, throughout the entire course of research. She accompanied me throughout much of my fieldwork, translated my questions back to non-English speakers, and translated quotes from Bosnian literature and media sources. During fieldwork in Visoko, I briefly used a second translator on one occasion, which turned out to be disastrous when she began to fight and debate with my research subjects in Visoko over the interpretation of certain artefacts. This hampered some of my future work at the site for a number of weeks.²⁷ After this incident, I returned to my first translator, and all other translation was computer-assisted with the help of online programs such as Google Translate.²⁸ While I have done everything possible to minimize errors in translation, I recognise that it is always possible that some may have occurred.

A third limitation that affected this study is that of specificity and case studies. As mentioned above in Section 3.3.1, I chose two specific case studies for a number of reasons, including their high-profile nature, the contrast of a 'pseudoscientific' site and a 'mainstream' site, as well as the aspect of theoretical contestation that both case studies contribute to the discussion of authority in this thesis. Nevertheless, the question remains as to whether these two sites are 'representative' of a discussion on the broad topic of authority in archaeology, and therefore whether conclusions in this thesis can be generalised. This limitation is generally characteristic of research that involves case studies, representing the "'central tension' in science between divergent viewpoints and the need for generalizable findings" (Star and Griesemer 1989: 387). Despite the overarching connections that I make in this work regarding the entirety of the 'archaeological process', this dissertation is not able to examine every facet of every stage of the archaeological process; it is constrained by time and space, and it is meant to contribute to and open up a much larger discussion about the nature of authority in archaeological practice.

²⁷ I discussed this incident and the methodological issues it raised in an (unpublished) paper presented to the Cambridge Heritage Research Group in Fall 2009.

²⁸ <http://translate.google.com/>

To alleviate the problem of addressing such a large and abstract topic, I narrowed the focus of my study on specifically looking at authoritative accounts of the past and the way authority manifests their production. This is a targeted direction in the much broader scope of the production of knowledge in archaeology. I also targeted two specific case studies and specific archaeological ‘moments’ in both of them in order to offer a solid discussion on this topic with concrete examples. By making selections, this study is inherently a constructed perspective, and it is aware of this stance. This study is not meant to be an exhaustive discussion of the meaning of the term ‘authority’ in archaeology, nor is it meant to represent the whole of either case study—other studies have focused on the deep development of each (Dalton, Barnes et al. 1968; Doob 1983; Barnes 1986; Collier 1992; Hamilakis 1999; Christiano 2004). Rather, this research aims to engage interdisciplinary, qualitative methodologies developing in fields such as STS in order to examine the complex construction of knowledge in the archaeological process and to better understand how structures of authority play into the production of accounts of the past.

This research is only one small study of a much larger theoretical problem. Like the parable of the three blind men who each touch and describe one different part of a whole elephant—one describes the tail as a rope, one describes the leg as a tree, and one describes the trunk as a snake²⁹—this research only touches on a small part of a much larger picture and yet contributes its one interpretative part of a whole. As Richard Geertz has said, it is “not necessary to know everything in order to understand something” (Geertz 1973: 20), and this research, while it may perhaps only feel out the very beginnings of a much larger research question, offers an unprecedented analysis of situated theory with supporting evidence from two comparative case studies. A case study-based approach, as described above, is a useful and productive enterprise that adds detailed knowledge about a problem in a larger issue. As Nietzsche has argued: “The *more* affects we allow to speak about one thing, the *more* eyes, different eyes, we can use to observe one thing, the more complete will our ‘concept’ of this thing, our ‘objectivity’ be” (Nietzsche 1969 [1886]: 119). This research is founded on the observation and analyses of parts and builds towards a greater understanding of authority in the archaeological process and how authority impacts the acceptance of knowledge about the past.

²⁹ One of the most famous versions of this parable is the 19th century poem “The Blind Men and the Elephant” by John Godfrey Saxe (1816–1887). See lines at the introduction of this chapter.

CHAPTER FOUR:

Authority as Accumulated, Translated and Stabilised: Çatalhöyük as a Case Study

"By what authority are you doing these things?" they asked. "And who gave you authority to do this?"
- Mark 11:28

4.1 Introduction

4.1.1 Introduction: Authority as Accumulated, Translated and Stabilised

This chapter argues that in archaeology, the production, exchange and consumption of messages involve a number of social processes—notably, inscription, translation and blackboxing—which affect the way knowledge stabilises into solidified, authoritative ‘final product’ versions of original fluid ideas and practices. This chapter demonstrates that authority is rooted not only in people, but in material actors and systems—such as the methods of inscription and translation, and in the agency of nonhuman actors like material culture—which create and stabilise authority in the production of knowledge. This chapter employs the case study of Çatalhöyük, Republic of Turkey as an illustrative example.

The Çatalhöyük project, under the direction of Ian Hodder, is a controversial archaeological excavation. Most of its highest-profile controversies today often do not involve debate over interpreting accounts of the Neolithic past (although scientific scuffles over data do take place, as with most archaeological projects). Instead, a great deal of debate revolves around how the site operates and what better methods or approaches can or should be taken to produce more faithful knowledge about the past. Archaeological research at Çatalhöyük is an example of very conscious—or reflexive—archaeological practice. The site has a history of deliberate engagement with the concept of authority, asking questions like: how can personal biases impact the outcome of authoritative accounts of the past? How can archaeological knowledge be better imparted directly to the public? Director Ian Hodder's strong opinions about the way the entire discipline of archaeology should operate have made him a powerful, if

controversial presence in the field. His authoritative voice has impacted the way archaeology has been taught and presented to generations of archaeology students over the last twenty years, making him a key figure in ‘postprocessual’ theory (Renfrew and Bahn 2000: 44-45; Wylie 2002: 16-17, 171). This framework—of conscious methodological debate, history of dialogue with issues raised by archaeological authority, and authoritative presence in the field—makes Çatalhöyük a particularly well-adapted case study for this thesis. By going one step beyond more traditional debates over authority, and by examining the practice and presence of Çatalhöyük’s scientific authority in depth, this chapter argues that epistemic and executive authority in archaeology is something that is physically accumulated and translated through the accessing and narrowing of physical and intellectual spaces.

4.1.2 Case Study Parameters: Relevant Project Background

Çatalhöyük is a Neolithic tell site in the Republic of Turkey located near the city of Konya in Central Anatolia. The word ‘Çatalhöyük’ means ‘forked mound’, which accurately describes the site’s two connected earthen mounds full of Neolithic material culture: the larger and older Neolithic East mound, and the later Chalcolithic West mound. The site was discovered by archaeologist James Mellart, who excavated large sections of the East Mound between 1961 and 1965. Under his direction, Çatalhöyük quickly became internationally recognised for a number of reasons.

First, the site was unusually large and complex for such an early date, and this led to Mellaart’s claim that Çatalhöyük was the “world’s first city”,³⁰ as well as the claim that this site was one of the earliest settlements to domesticate plants and animals (Shane and Küçük 1998; Hodder 2000: 3). Secondly, the site has been a source of sensational finds, thanks to exceptional preservation of rare early art and unusually arranged cultural habitus. Mellart discovered sculptures and paintings in what he called “shrines”. Mellaart interpreted depictions of decapitated humans being eaten by vultures and “murals depicting men pulling the tongues and tails of aurochs and stags” as signs of funeral rites and social behaviour (Shane and Küçük 1998: 43). He also interpreted a Neolithic goddess cult from female figurines found in the mounds. Mellaart’s graphic finds—coupled with his equally graphic descriptions and interpretations—put Çatalhöyük on the academic map.

³⁰ The idea of Çatalhöyük as a ‘city’ has been disputed and debated in Ian Hodder’s more recent project, and it is now more commonly referred to as a large ‘settlement’.

The site is also famous not just because of Mellart's work, but because of the circumstances of his sudden departure in 1965 after the so-called 'Dorak Affair'. By most accounts, this affair involved a mysterious woman named Anna, who supposedly showed Mellart a set of illicit antiquities, from which he later published illustrations, and then she disappeared. Because he was never able to produce evidence these antiquities or find Anna herself, and because he was unable to defend his publication of the claimed artefacts, the government forced Mellart to quit his excavations and leave Turkey. This story drew a great deal of attention to the site of Çatalhöyük (Baltar 2006: 44-54). It is noteworthy that this early history of the site—full of Mellart's sensational finds, his equally sensational claims and finally his sensational departure—are the foundations of Çatalhöyük's fame, status and international recognition.

From its dramatic past and into its present history, Çatalhöyük has come to hold attention and influence in the realm of archaeological theory. The current project, under the direction of Ian Hodder, is representative of 'postprocessual' archaeological theory, and it is seen as a site that "well illustrates the changing approaches to archaeology in the second half of the 20th century" (Renfrew and Bahn 2000: 44). Ian Hodder, who continues Çatalhöyük excavations today as his primary archaeological project, has been called "the most influential figure in the post-processual movement of the 1980s and 1990s" (Renfrew and Bahn 2000: 44). Hodder has built his own career, fame and professional authority around his postprocessual theories and experimental archaeology. Çatalhöyük is the site where he has actively tried to put his theoretical arguments into practical operation. The Çatalhöyük project today has two major aims.

First, it promotes the unique and sensational archaeological finds from the mounds, arguing that "the site is an internationally important key for our understanding of the origins of agriculture and civilisation" (Online Mission Statement, Çatalhöyük Research Project 2010b). The project argues that Çatalhöyük is of global heritage importance: the site actively tries to address problems raised not only by the site's archaeological interpretations, but also by heritage management issues, such as the need to focus on conservation and public access to archaeological practice—thus, the project is said to have "a wider applicability to many sites in the Eastern Mediterranean" (Online Mission Statement, Çatalhöyük Research Project 2010b). This agenda aims for Çatalhöyük to be recognised as critically important to Turkish and global history, and it aims for the site to be seen as representative of current heritage management trends. To that end, the Çatalhöyük team has invested a great deal of research toward solving problems of access and presentation, such as in how to integrate their work with local Turkish communities (Bartu 2000; Matthews, Hastorf et al. 2000; Shankland 2000), and

in how to involve 'other' voices and interpretations of interest groups outside of the archaeological community in the interpretive process (Rountree 2007; Atalay 2009).

Hodder has advocated for the archaeological discipline to become more engaged with multiple or alternative perspectives of the past. Hodder and his team have stressed that the accounts of the past that they produce are interpretive and speculative in many ways, and they recognise that there are alternative accounts of the past that challenge or compete with their own interpretive space (Hodder 2003; Rountree 2007). In a concrete step towards transparency, the Çatalhöyük project makes data from the project quickly available on its website.³¹ Members of the public and other academic professionals can immediately access the site excavation reports and data. Hodder's team hopes that transparency of their aims and work will further public involvement and theoretical engagement with the material. This was a particularly novel and groundbreaking idea in the early stages of Hodder's excavations in the early 1990s, when archaeological information distributed to the public via the Internet was a rare and new concept. On a more theoretical level, Hodder also pushes for a program of 'multivocality'. He seeks to "allow more open-ended and multivocal approaches to the interpretation of the site as a whole, allowing not only different specialists to have a voice, but also the local inhabitants" (Renfrew and Bahn 2000: 44). This openness extends as far as allowing of alternative public groups such as the Mother Goddess community to have their views "respectfully entertained by the excavators, even if they do not share them" (Renfrew and Bahn 2000: 218). However, as this chapter examines more in depth, despite much talk about engagement and multivocality, the site in practice still retains ultimate authority over how accounts of the Çatalhöyük past are presented.

The second aim of the Çatalhöyük project is to bring postprocessualism to bear in the site's practical operation. Hodder's project began with the ambitious aim to completely reorganise excavation practice, so that it could be free of some of the more overt and intentional modes of personal modern bias. Hodder aimed to:

[D]evelop a more flexible and open approach to stratigraphic excavation...he set out deliberately to avoid the early division by the excavation director of the observed strata into closely defined "phases" and "units" – the more standard practice – with the director thus taking ultimate responsibility for the stratigraphic interpretation (a practice which some postprocessual critics have seen as authoritarian). (Renfrew and Bahn 2000: 44)

Hodder initially argued for a complete revamp of methodological frameworks at his site, like excavation categories. The idea was that the Çatalhöyük excavations could be a new experiment in conceptualising excavation practices. Computer recording, site diaries

³¹ <http://www.catalhoyuk.com/>

and new databasing methods were implemented “to allow more interactive stratigraphic interpretations” (Renfrew and Bahn 2000: 44). For example, instead of using traditional recording categories like ‘midden’ or ‘hearth’, the team instead used broader categories like ‘pit’ or ‘fire instalment’ in order to indicate these feature finds. The idea was that by using more general terminologies and more open theoretical frameworks, the material culture was freed from immediate biases, which were instinctive in the original terminologies, such as the notion of a ‘hearth’ as a central, homey, warm space of domestic interaction.³²

Finally, Çatalhöyük is internationally recognised for being a project that has—and can afford—excellent standards in scientific methodology and practice. Because of the site’s international reputation as a cutting-edge site with innovative practice, it has been able to draw a number of reputable institutions, researchers and funding bodies. Each fieldwork season, nearly a hundred researchers attend the site, doing original studies in anything from environmental research on vegetation and phytolith remains (Deckers, Riehl et al. 2009), to biological anthropological research on local genetic relationships (Pilloud 2009), to ethnographic observation of modern day archaeological practice (Hamilton 2000; Erdur 2006; Erdur 2008). The attendance of so many specialists, who work at the specially designed dig house laboratories alongside the excavators for the whole season, is unique at Çatalhöyük. During my own research on the site, I heard one visitor exclaim that the dig house, with all of its specialists and microscopes and well-organised facilities, looked like a “NASA space camp!” (site visitor, personal communication, 2010), and this sentiment has been echoed in other anthropologists’ observations at the site.³³ Because of the rigorous standards and theory-laden practice at Çatalhöyük, the unusually high status and attendance of researchers from prestigious institutions, and its sizeable funding from unusual donors like the Visa and Boeing companies, Çatalhöyük has been internationally regarded as an

³² See Section 4.3.3. for further discussion of this activity in actual practice. During my fieldwork, this study found that some team members felt that these broad categories collapsed back into more traditional categories over time. The ‘pits’ that resembled ‘middens’ were, in the end, interpreted to simply *be* middens by the team who excavated the recurring material. Therefore the broader ‘open’ categories collapsed back into these more traditional ones as familiarity with the material lend stability to more solid interpretations (site specialist, personal communication: 2010).

³³ Anthropologist Oguz Erdur had a similar interview during his own fieldwork. From his doctoral dissertation, Erdur writes:

“Even I myself was scoffed at by an elderly archaeologist: ‘Oh dear! Why aren’t I surprised? Seems like, everybody’s going to Everybody-knows-land nowadays!’ Another [Turkish archaeologist] was more subtle, regarding at least my quest: ‘That’s no real archaeology over there, I’m telling you; it’s more like a NASA camp! The money, the labs, the tools, the people... It’s all surreal! We the locals could never even attempt something like that. Would we even want to—that of course is another story.’” (Erdur 2008: 557)

authoritative project with the resources and skills necessary to do ‘good’ scientific archaeology.

The primary goals raised by Hodder’s Çatalhöyük project—that of flexible and reflexive interpretation in site practice, as well as the importance of allowing multivocal interpretation and archaeological access—have a great deal to do with authority. Not only do these theories directly engage with the notion of authority and the questions that authority raises—such as who should be allowed to access the material past, to speak for and interpret the past, to utilise resources that are sourced from the past, etc.³⁴—but they also affect the authoritative status of the site itself. Çatalhöyük today draws most of its attention from its deliberate engagement with issues of executive authority, control, access and epistemic authority. These issues affect a deeper, and yet unexamined, root concern of *what authority is* in relation to archaeological practice, which has been not explicitly discussed by the Çatalhöyük team. This thesis uses Çatalhöyük to address the root causes and effects of authority on the production of archaeological knowledge. This chapter does not just address authority as a symptom of other issues, like personal bias, gendering of accounts or problems of physical access to site remains, which have been central concerns of previous research. Rather, this chapter offers a detailed examination and analysis of authority from social structure and interaction (Section 4.2), an examination of how authority manifests through the processes of inscription, translation and blackboxing which stabilise and solidify ideas into archaeological accounts (Section 4.3), and ultimately argues that authority is a cumulative process—an outcome of the resistance and accommodation of people and things in intellectual and physical space (Section 4.4).

4.2 Authority from Social Structure and Interaction

4.2.1 The Social Construction of Facts and the Factual Construction of Social Agents

Ethnographers David Van Reybrouck and Dirk Jacobs have written that “Excavation seems not so much a process of *salvaging* but of *solidifying*” (2006: 34, emphasis in original). Archaeological sites are the physical spaces where archaeological practices turn piles of dirt and rubbish into knowledge about the past. Archaeological

³⁴ Issues that have been previously introduced in literature (Hamilakis 1999, Rountree 2007, Webb 2002).

practice is far from an operation of simple salvage; it is the making of something new and solid from something old and incomplete, the creation of narratives and histories that solidify our understanding about what happened in history (Hamilakis 1999; Yarrow 2003; Edgeworth 2006; Hamilakis and Anagnostopoulos 2009b).

The concept of solidifying offers three points of interest to this thesis: (1) first, archaeological facts are solid forms of knowledge that are socially created, and like in any social endeavour, the production or solidification of archaeological facts is affected by social asymmetries of power and authority. (2) Likewise, archaeologists are factually constructed social agents: “Social actors do not precede natural constructs but are as much the outcome of scientific practice as are facts” (Van Reybrouck and Jacobs 2006: 37). In other words, facts may be created or solidified through the social interaction of people and things in an interrelated network, but people can also *become* or solidify into factual things—like ‘archaeologists’—through their participation in an appropriate network of people and things. Thus, the process of fact-constructing itself can directly impact the factual status and authority of people. (3) Finally, the way authority is formed in intellectual power or control emerges from the interplay of (1) and (2)—the solidification of facts in the scientific process (often by experts), and the solidification of agents who factually *become* archaeologists or other experts, who thus gain the authority to profess those facts. These processes directly affect the executive and epistemic authority of individuals, collective groups or institutions, and the accounts of the past that they produce.

The idea that archaeological facts are socially created is not new;³⁵ and since facts are socially created, authority must be a major player in the production of knowledge. Questions therefore remain: where and how does authority manifest and affect the knowledge production process? How important are power asymmetries in both the production and consumption of archaeological accounts? Authority is integral in the way facts are constructed and received. Furthermore, sometimes we forget that “excavations are not only places where observations are turned into facts but also where individuals are turned into archaeologists” (Van Reybrouck and Jacobs 2006: 37, emphasis added). Authority manifests in this mutual constitution of actors and facts through the interrelationships between social asymmetries in this network.

For example, facts in archaeology materialise out of essentially nothing (the unknown or un-found) and become something (the discovered material thing, the known, something interpreted) by their interaction with people who give them meaning

³⁵ See Section 2.2.

through categories and narratives (Gero 1996; Yarrow 2003). Facts also gain authority and status through their association with a reliable excavator or site specialist. Van Reybrouck and Jacobs use an example of how a discolouration in the sand *becomes* a 'posthole' when a reliable expert finds and identifies it. Naming a discolouration a 'posthole' is the creation of a new fact, changing a find from nothing into something. Pertaining to point (1) above, the 'fact' of a 'posthole' is socially constructed through the complex institutional and personal associations that lie behind why an excavator is considered a reliable expert, someone competently able to identify a pothole. Such a 'fact' also has status simply because the category of 'postholes' are considered worthy of attention by the discipline of archaeology for socio-historical reasons. Pertaining to point (2), the archaeologist in this example who finds a 'posthole' is also a factually constructed social agent. She gains authority and status through her interactions and associations with a discolouration in the sand. By validating a discolouration as a 'posthole', and by using the appropriate tools and performing the appropriate behaviours of an archaeologist, she is articulating and maintaining her own professional identity. If her fellow archaeologists concur with her finds through their witnessing and trusting of the sincerity and competence of her identification—and if this interactive process between the excavator, the material and her peers is reproduced over time—then her authority as a competent expert becomes more and more established. Both the archaeologist and the posthole in this scenario “mutually articulate each other; they emerge simultaneously from actual practice” (Van Reybrouck and Jacobs: 37). The archaeologist needs the posthole as much as the posthole needs the archaeologist in order to maintain professional authority, status and identity.

This point is further expanded by the fact that (3) with both individuals and institutions, executive and epistemic authority is derived from this interaction between the social construction of facts and the factual construction of social agents on a much larger scale, in a complex network of people, things and motivations. The entire 'discipline of archaeology' is an institutionalised, recognisable category of practice, a networked system of all the micro-interactions and interrelations between material remains, tools, technology, ideas and philosophy about the past, and the human actors who call themselves archaeologists. As archaeologist Thomas Yarrow writes, “archaeologists create the objectivity of the artefacts and features they excavate by themselves embodying archaeological conventions, skills and knowledge” (Yarrow 2003: 66). Within the discipline, facts and actors are mutually constituted.

4.2.2 Social Arenas of Authority and Practice at Çatalhöyük

4.2.2.1 Structure and Space

Like most archaeological projects, the Çatalhöyük Project—which includes the arenas of archaeological material in the earth, the dig house laboratories, the machines and tools, as well as people who work in these spaces and with these things—is part of a complex system and society, a culture operating under the awareness that they ‘do archaeology’ and work as ‘archaeologists’ handling ‘archaeological material’. Thomas Yarrow addresses the fact that, “the site, composed of artifacts, is itself also an object or artifact” (Yarrow 2006: 24). People often refer to ‘the site’, ‘the dig’, ‘the dig house’ and even ‘the archaeology’ as if it were an object, subject or artefact—a distinct category or recognisable unit. The idea that an archaeological ‘site’ is a specific cultural thing is an understanding that impacts, enables and constrains the way we understand and approach any archaeological place or material. Sites are seen to be distinct, bounded, accessible spaces; they are physical units of the landscape where people go to identify, access, utilise, study and contest material culture from the distant past. The material itself articulates the site as an *archaeological space*.

People who intend to access archaeological spaces for the purpose of ‘doing archaeology’ operate as part of a wider network of people and associations, and those who identify themselves as archaeologists operate in socially distinct ways that classify and represent their actions as archaeological. People who are not archaeologists before they begin work at an archaeological site can *become* archaeologists through the embodiment and performance of what it is to be an archaeologist: through the enactment of archaeological methods, the access of archaeological space and material, and the use of tools identified as archaeological (Van Reybrouck and Jacobs 2006). In this way, archaeology is a social culture that is intimately connected to the idea of what an archaeological space is, what archaeological material is, who an archaeologist is and what it is to perform archaeological acts. Authority in this context involves the power asymmetries that are built into this social culture.

During my time at Çatalhöyük in the 2009 field season, I found that the arrangement of physical space and the movements of people and things through this space dramatically affected the way knowledge was produced at the site. The structure of physical and intellectual space at Çatalhöyük impacted how or why people or things held authority and status. Networks of people and things were directly impacted and shaped by spatiality, by the movement of people and things through physical space.

The idea that physical and intellectual structure affects human and material agency is also not new. Scholars, working particularly in the late twentieth century, have developed and debated theories related to structuralism, post-structuralism and agency. Studies in structure and agency have discussed how the patterned arrangements of social life and physical space limit or influence the choices and opportunities of individual agents, and importantly, they have addressed how this might impact the production of knowledge. In archaeological practice, theories of structuration have focused on how human patterns might be recognized in the material past (Renfrew and Bahn 2000: 486). Some archaeologists have critically argued that structuralism limits interpretations to dialectics or pattern categories like cooked/raw, dark/light, left/right, man/woman, which might bias archaeologists, undermining the nuanced and varied complexities and differences of social understanding that humans held in the past (Renfrew and Bahn 2000: 486). However, there are remaining questions that lie beyond this work on structuralism, such as: how are human *power relations and authority* enabled or constrained by structure and space? How does the structure of physical and intellectual space impact archaeological methods and the production of knowledge?

These latter questions were forefront as I observed the interaction of people and materials at the site of Çatalhöyük during their 2009 field season. I observed the *practical materiality* of knowledge construction—the use of the physical things and space as mundane as the social use of coffee cups and lunch table space, to the most scientific use of microscopes and Bunsen burners as well as laboratory space—and I examined how archaeological practice relied on a plethora of different power relationships, hierarchies, groups and individuals who all interacted in physical spaces with physical things. To quote Anni Dugdale again: “Committees of all sorts sit in rooms, drink coffee, and shuffle through paperwork. And it is in and through such material arrangements that decisions are made possible” (1999: 116). Executive and epistemic authority at Çatalhöyük operates on various levels, by individuals as well as collective groups and institutions. There are the team members who produce knowledge on site, the local and extended scientific community who create and sustain a discourse about the Neolithic past and who debate present archaeological methodology, the general public who relate to the site, and the government who authorises its discourse through laws and social promotion.

During my stay at Çatalhöyük, I identified and observed social arenas of knowledge production. The term ‘social arena of practice’ is drawn from Handler and Gable’s study on Colonial Williamsburg, where a ‘social arena’ is a defined space “in which many people of differing backgrounds continuously and routinely interact to

produce, exchange, and consume messages” (1997: 9). In the 2009 Çatalhöyük field season, various groups engaged with accounts of the Neolithic past as well as with accounts of contemporary archaeological practice and method. The production, exchange and consumption of knowledge in every social arena directly impacted the way the archaeologists on site and the public understood and interpreted the Neolithic past. Interactions within and between each social arena not only established why some materials and accounts were more handled or were more powerful than others at the site, but also established why certain groups and individuals appeared to have more or less authority over others—both in terms physical or executive authority, as well as interpretive or epistemic authority. The next subsections identify some of the social arenas at Çatalhöyük, where messages and interpretations were produced, exchanged and consumed: the excavation site, the dig house, on-site public arenas and off-site physical and virtual public spaces.

4.2.2.2 The Çatalhöyük Excavation Site as a Social Arena of Knowledge Production

In 2009, when I observed work at Çatalhöyük, the two East and West ‘forked mounds’ formed the primary ‘site of Çatalhöyük’. At roughly 100,000 square feet, the site was considerably large. The excavation space on the mounds had been divided by different teams, under individual directorship and institutions (such as Cambridge/Stanford, Berkeley, Istanbul, Team Poznań from Poland, etc.), who each operated different trench sections that were attributed as their ‘own’. All of these individual excavations and material remains still fell under the ultimate direction and authority of Ian Hodder, who was the head Çatalhöyük Project director, and who had the authorisation to be ‘site director’ from the Turkish Government. In 2009, the East Mound was divided by two distinct teams, the Stanford excavations run by Ian Hodder and his right-hand field director Shahina Farid, and a second team called Team Poznań from Poland, who mainly used this season as a study season to catch up on post-excavation work in the laboratories. The West Mound was similarly divided (there was a SUNY Buffalo trench, and also a separate Turkish team trench, but only the former excavated in the 2009 season while I was there, and both teams still fell under the ultimate authority of Ian Hodder’s directorship).

Two of Ian Hodder’s Stanford-run trenches on the East Mound had expensive permanent shelters constructed over them, singling them out as the primary project dig sites. The trenches under the permanent shelters were the sites that tourists were drawn to, and many of the houses under both shelters had been excavated only to a

certain point and then left with the intent to be preserved, managed and displayed for public consumption. During this season, several other trenches on the East mound were 'closed' and non-operative—like the large overgrown cut section left by James Mellaart's activities (1960s) and the in-filled sections by Ruth Tringham's BACH team from Berkeley (1997-2003), which were only visible if identified by a site expert. Other trenches, like the active section in the West Mound by Peter Biehl and Eva Rosenstock's team from SUNY Buffalo, only had temporary shelters. Members of the public were routinely not invited to visit the West Mound excavation space.

The excavation trenches were diverse arenas of social practice, where issues of expertise and epistemic dependence were negotiated in different ways, on different levels, by different teams and people. During this season, because of constraints on time and dissertation space, I found it most relevant to focus on the Hodder excavations that occurred in 2009 on the East Mound. The 2009 season was originally organised to be a "study season", with focus on researching post-excavation data from previous years:

As the 2009 season was primarily a study season, [new] excavations took place in three areas only in the South Area on the Neolithic East Mound and Trenches 5 and 8 on the West Chalcolithic Mound. The study season ran from 15th June until the end of July during which time teams worked on post-excavation analyses in preparation for the publication of four new volumes covering the excavations in the 4040 Area, South Area, TP Area and IST Area excavated from 2000 to 2008. The aim of the phase of work in preparation for publication addressed the social geography of the settlement and larger community structure. (Farid 2009: 7)

As Farid writes, the study season was meant to be focused on post-excavation preparation for publication, so excavations on the East Mound in 2009 took place only under the South Shelter, and many archaeologists on site referred to these excavations as a 'bonus' dig.

The excavation site was the immediate space where archaeological material was first found, examined and removed by excavators. 'Excavators' in this instance consisted of a group of professionally hired and trained excavators who were attended by apprenticing students with different skills and backgrounds, including a group of undergraduate students from Stanford University, a group of training archaeologists from universities and institutions in Turkey, as well as a few independent researchers, such as myself. Local Turkish field hands were also present; they worked seasonally and part-time, with minimal archaeological training. These local field hands (often alongside the Stanford undergraduates, who were the 'bottom of the rung' in the site hierarchy while on their field school) sifted and bagged the material from the dirt buckets, which contained the majority of earth removed from the site.

All of these individuals came together in the excavation space, where they physically interacted with material remains from the Neolithic. On an interpretive level, all of these individuals—from the professional who decided where to dig and when to record, to the field hand and student who decided what to bag and what to throw from the sifters—made active decisions, negotiations and choices about how to handle the material as they found it, and they made immediate interpretations about what the material in the excavation context ‘means’. Hodder has long recognised the powerful position that this places excavators in: his signature claim is that first impressions and interpretations begin “at the trowel’s edge” (Hodder 2003: 58). In some ways, the excavators had the most immediate and raw executive power and authority at the site, at least in the initial stages of interpretation. They were the first to access material remains, the first to see them and touch them, holding the power to decide what material to cut into, what to keep or destroy, and what to do with the material they found. This power, of course, affected the ‘final product’ interpretations that came out of this field season, for specialists could not study what was not saved, and the entire project’s data archive was founded on the records and inscriptions³⁶ that were taken in the field. However, the authority of this social arena was also regulated by a whole tacit system of rules and accountability. People ‘did archaeology’ as if there was a ‘right’ approach, a ‘correct’ way to take samples, a ‘correct’ way to bag or sieve, a ‘correct’ system of deferring decisions to the authority to those with more or less expertise or experience.³⁷ This deference took two forms.

First, the excavators gave external deference to the greater institution of archaeology. The discipline as we know it today is a product of generations of socio-political and disciplinary context and development. The recording and excavating methods used at Çatalhöyük during the time I attended the excavations were standard techniques that have been more or less accepted as ‘tried and true’ methods in the field of archaeology. I saw little difference from the excavation practices at Çatalhöyük than those methods I had seen or used in other excavations and field projects. Throughout history, the discipline of archaeology has developed and narrowed these techniques as reliable, normal or ‘correct’, and so they hold a high degree of authority in the field through the history of their use and continued acceptance. The Çatalhöyük excavators, while in the powerful position of deciding what and how things were saved or destroyed

³⁶ See Section 4.4.1 for detailed discussion on *inscriptions*.

³⁷ Reference the Faunal Laboratory practice flow chart [Figure 8], which illustrates this kind of deference to experience and authorities.

in digging, were constrained and limited by the institutional authority of archaeological disciplinary practice.

Second, there was also an deference to the internal structures of executive hierarchy and socio-political context of the excavation site. It was understood that the entire project operated under the authority of the Turkish government, which legally owned the site and had full control and ownership of all the material unearthed and studied. There was the authority of Ian Hodder, the director who controlled all of the strings—purse strings, academic strings, publication strings, and who had ultimate say over what academic activities took place at the site. There was the field director Shahina Farid and the professional excavators, who held primary executive authority over the excavation dig spaces. There were the site specialists who held authority over various intellectual (and sometimes physical) arenas, with authority narrowed by the categories/types of remains unearthed. Finally, there was the public and visiting scholars, who often held authority over the consumption of messages produced in the excavating spaces, especially when they were vocal in recommendations or changes. Each of these internal groups held authority in a hierarchy of deference and in specific domains of practice.

Specifically at the Çatalhöyük dig site, the practice of excavating with such a diverse group of people made for an dynamic arena of executive and epistemic authority. In 2009, Ian Hodder rarely attended the digs personally. When he did, he was usually giving site tours to visitors, or he observed the trenches from the sidelines and asked the excavators questions. His directorship seemed to involve more 'behind-the-scenes' managerial work: visiting the specialist labs and interviewing his team members to gather a broad understanding of the site activity and scientific progress, performing his role as a site organiser who hired and fired, arranging and attending important meetings with the government representatives and funding bodies, giving tours and presentations to the public, interacting intellectually with visiting and attending researchers, and working on publishing books or articles that gave an overarching narrative of the site's history and the project's methodology. The actual excavation arena was instead the domain of Shahina Farid, the field director and right-hand to Ian Hodder, who had been working at the site since 1995 (Baltar 2006: 122).

The excavation hierarchy began with Farid as the highest epistemic and executive authority on the mounds, then extended down to other members of the professional excavating team who had a great deal of expertise and experience excavating as contract archaeologists, then to the specialists and graduate students who were excavating for their own research or interest and who had institutional backing,

then to the field school students who were learning excavation for the first time, and then finally to the field hands, who sifted, never touching a trowel. Both the executive authority and the epistemic authority of these groups was tacitly recognised in the order of this hierarchy, with the exception that an elite core group of the professional excavators were recognised to have field skills (but not managerial skills) on the same level as Farid.

For the majority of excavating work in the 2009 field season, specialists remained in the dig house to work (except the conservationists). When site specialists were called up from the dig house to take samples for their work, or to lend interpretation or advice on something found during the dig—usually in the setting of a “Priority Tour” (when an usual or spectacular find was unearthed)—it was because their expertise was recognised and valued because their epistemic authority in some way ‘trumped’ that of the field excavators. However, in the case of excavating fieldwork itself, the executive authority of the professional excavators on site was never ‘trumped’ by specialists—the excavation site was their domain, the dig house was the primary domain of the specialists, and all of this was a tacit understanding between the groups. This regulation of authority in separate tacit ‘domains’, albeit interlinked and with blurry edges, may have emerged as a positive compromise or resolution to the long history of tension between excavators and specialists on site (Farid 2000: 27-29; Hamilton 2000).



Figure 2: Excavation site under the South Shelter. Ian Hodder giving a site tour to a group of tourists. Photo by Tera Pruitt.

4.2.2.3 *The Çatalhöyük Dig House as a Social Arena*

At Çatalhöyük, the dig house was the place where excavated material—after being dissected, bagged and categorised by the excavators—went directly for post-excavation study and analysis by the laboratory specialists. The dig house was also space for post-excavation database recording by the field excavators, as well as living and accommodation space for all members of the East Mound and West Mound projects. During the 2009 field season, it was also the primary place for the exchange of ideas, especially because this was a Study Season, and the majority of post-excavation activity took place in the dig house. Exceptions included the brief interaction between excavators and specialists on the mounds during the Site Tours and Priority Tours, or in the special circumstances when directors or specialists were called in to offer expertise about specific finds.

The physical dig house was situated at the base of the East Mound, located on a road that led into the nearby village of Küçükköy. The building was divided into laboratories, living areas and recreational spaces. The dig house was open in plan, surrounding a courtyard and a covered veranda. This encouraged social interaction, as people could socialise on the veranda and immediately access all other living and laboratory areas through the courtyard. Immediately outside of the dig house (in 2009), there was a set of external buildings, including the 'experimental house' (Stevanovic 2006), a makeshift party bar for social activities (which was later turned into a storage shed) and several large storage areas. The main working areas for the team were in the laboratories, the rooms which lined two entire sides of the dig house. These laboratories were arguably the most important arenas for the last stages of the knowledge production process at Çatalhöyük. In these rooms, the specialists and excavators did post-excavation study and inputted records into the database, scrutinized and studied selected artefacts in detail, discussed theories and interpretations, illustrated and reproduced material in text and visual forms, and readied the site interpretations and narratives for publication.

The laboratories were roughly arranged by a division of archaeological material, such as faunal remains, human remains, obsidian, conservation, etc. In 2009, the specialists spent the vast majority of their working time in their own individual laboratory, closely interacting with members of their own specialist team, unless they needed to consult another member of the project—often more an exception than the norm. Other groups worked in the dig house as well, such as the field excavators who

had their own large seminar room for post-excavation analyses,³⁸ and the West Mound or Polish teams, who worked together in one laboratory as a complete team unit. As one of my interview subjects³⁹ explained, the team was divided in the dig house arena “like collective pods that work together”.

As a physical construction or landscape, the dig house was designed and ordered in specific way, with the space doled out in specific ways for specific reasons. Most of that reasoning seemed to be based around material types like stone remains or human remains. Focus on these material ‘types’ was more a product of the way archaeologists in the discipline are generally trained to specialise in specific material types rather than whole features or units (i.e., focus on ‘lithics’ or ‘bone’ instead of whole ‘burials’ or ‘clusters’ that include multiple material types). This setup of dig house space affected interpretation, because people who specialised in a specific interest, such as faunal remains or human remains, primarily gathered and worked in their own laboratories for the bulk of the workday, establishing a social ‘pod-like’ base of operation. Naturally, human relationships and internal hierarchies formed within each spatial ‘pod’. Networks of people and things were directly impacted and shaped by spatiality, by the movement of people and things through physical space, and this affected what kind of intellectual engagement occurred between humans, material and final product interpretations.

Other groups beyond the Çatalhöyük team also interacted with material and people at the dig house. One notable group were academic visitors, including general archaeologists, students of archaeology, and specialists that were not Çatalhöyük team members but who came to observe the activities at the dig house or mounds. During my stay, a number of different academic groups like this came to see the site, on a metaphorical pilgrimage to view ‘postprocessual archaeology in action’. For example, team members of a neighbouring excavation in the Konya region run by Professor Nicholas Postgate from Cambridge visited the site, and students from this group told me that they were ‘excited to see the famous site’. Another group involved a teaching classroom of professors and undergraduate students from a New York university, who were given a long tour of the site, and who asked many questions about postprocessual methods and the relations between the Turkish authorities and Çatalhöyük’s excavation permits. On another occasion, a postgraduate student from a German University who had an interest in working with the team in the future came to observe work for a day

³⁸ This space was where the excavators inputted all of the hand-written plans and finds sheets into the project-wide database, so that every team member could have access to the excavation data through a networked internet system, run out of the dig house administrative/IT office.

³⁹ This interviewee was a returning ethnographic researcher and excavator at the site.

and speak with members of the team about archaeological ethnography. All of these ‘research pilgrims’, as I came to call them, were expressly interested in the site for its archaeological value, and its authority as a noteworthy site included in most introductory archaeology textbook ‘pantheons’. During my time at the site, many in these groups directly interacted with archaeologists at the dig house, asking questions to the directors and to other approachable team members, and in some cases added—or tried to add—to the intellectual discussions, perhaps influencing knowledge production on site.

Another social group, which I defined as having ‘intimate local interest’ in the project, also interacted with knowledge production in the dig house. This group included people from the general, non-archaeologically trained public, such as people from the nearby village of Küçükköy and other Turkish members of the public who did constantly interact with the site in specific outreach programs.⁴⁰ Many of these members of the public interacted closely with team members and site material and had a vested interest in the project, but they did not have specialist knowledge. I found that archaeologists themselves gained greatly from this collaboration. By interacting with local populations, team members better understood how they themselves worked or engaged in their own subject matter, making them reflect on the implications and necessity of collaborating with local populations. Some of these implications were later reviewed and discussed in the last pages of the 2009 Archive Report (Çatalhöyük Research Project 2009). The dig house was the primary area where this public group was able to interact with team members and archaeological material. All non-team members were restricted from access to most of the laboratories and storage spaces on site (this restriction was usually unspoken, but understood).⁴¹

The most important spaces in the dig house structure were two main rooms that operated like ‘hubs’ for the physical network of people, material and space. The first was the Finds Room laboratory [see map, Figure 3]. This room held a number of desks for various specialists, including people working on finds, figurines and the illustrators. The Finds Desk, however, was the critical place where all excavated material was immediately taken after excavation was finished for the day. The Finds specialist’s job involved recording all data from the artefact bags into the database, then redistributing the material from the Finds Room to the various other laboratories for post-excavation analysis. For example, when a group of various material types were found in a single

⁴⁰ Some locals were employed by the project to do services like cooking, cleaning or sifting. Others came as part of specific outreach programs to involve local communities in the knowledge production process.

⁴¹ See Section 4.3 for discussion on access issues.

context—which was recorded as a single feature called a ‘cluster’—the finds would be generally recorded together in the field as one find on a context sheet, then the different material types would be separately bagged (obsidian in one bag, human remains in another, stone beads in one bag, bone beads in another) and then taken in a bucket to the Finds Room. The Finds specialist then would input all of the recorded data (which she would find written on slips of paper in the finds bags, recorded by the excavators earlier that day in the field as they bagged the material) into the site database. Then she would split the bags up and distribute the material—obsidian, human remains, faunal remains, ceramics, etc.—to the various laboratories where the different specialists worked on analysing material types. In essence, her role was to transfer the physical single context into the database, and then transfer the material on for more detailed study.

Theoretically, the idea is that the different materials in a cluster, after it is recorded, is forever inscribed into the same context just by going into the database. The idea is that by breaking up the material, the specialists can each examine it and input more data into the database, with more interpretive authority attached to it because of the specialists’ formal training in material types. The breaking up of material shows how important a theme it is in archaeology to *inscribe* information into a virtual or representative form, and shows the powerful assumption that this is the most efficient way to maximise information.⁴² However, whether this method in any way actually helped interpretation was debatable. When I asked team members how finds ended up back ‘together’ to be interpreted from multi-type features like a cluster, the answer was scattered. In the case of a profound find, like the Plastered Skull Burial (see Section 4.4.3, below), where materials like human remains and faunal remains were found together, it was very likely that they would be interpreted as one entire unit, since it was likely to be a much discussed find and would quickly find its way into print (Hodder 2006; Hodder 2010b: 129). However, when I asked some team members how things like stone beads and bone beads found together as part of a bracelet in a grave would come back together as ‘a bracelet’ in the interpretive process, since they would have been separated in the Finds Room and sent to different labs for processing as ‘stone’ or ‘bone’, the answer was less sure. The usual way the ‘coming together’ of site material happened, several team members told me, was during the Discussion Season (which was scheduled for the future year of 2010). In the Discussion Season, team members come together to sit around and discuss material, interpreting it on a general team platform and readying it

⁴² See Section 4.4.1 for further discussion on the importance of inscription.

for publication. Theoretically, the team members said, this is the place and time for the ‘coming together’ of material like clusters. However, more than one specialist admitted by the end of our conversations that the likelihood of something like stone beads and bone beads coming back together on a less than spectacularly interesting find was ‘a bit of a hit or miss’ (site specialist, personal communication 2009).

These discussions implied to me that the distribution of materials via the method of dividing things by material ‘type’ did impact the production of knowledge at the site, and this impact would affect what ‘accounts’ of the past were ultimately created and distributed by the Çatalhöyük team. This approach of studying the past also implied that the authority and prestige of certain clusters or finds—and the likelihood that they would make that last step from being ‘raw data’ to ‘accounted for’ in final product publications—remained in the hands of individual excavators and specialists who may or may not think about them in the future, and who may or may not have a loud enough authoritative voice during the discussion seasons to make these finds memorable to those who would write the most solid, prestigious or authoritative articles.

The second important room of the dig house was the team office. This was the space where the site field director Shahina Farid had her main administrative workspace, and where the IT terminals were located. When speaking with Ian Hodder, he told me that the administrative room was the operational “nerve centre” of the dig house. Not only was the administrative office the place where paperwork was filed and official business was checked, stamped and communicated, but it was the place where administrative and organisational team records were kept. This was the executive hub of the dig house, and people would go to this centre to inform the managerial level of the site hierarchy about their whereabouts, needs or plans. The database was a central system and network for all members of the site—holding authority over information access in both physical and intellectual ways. In our conversation, Hodder referred to the database as an ‘amazing interlocked thing’ that connects everybody on the site, linking the virtual site records with the physical actions of all the team members working on site. He explained in good humour that when the database went down or broke, everything in the dig house seemed to shut down. People would suddenly emerge from their dark laboratories and come out onto the terrace or veranda, waiting for the record system to be fixed so that they could get back to work. This technology was vital to the way excavated materials were inscribed, and these inscriptions were critical to the production of archaeological knowledge. The social space and work interaction at Çatalhöyük was physically altered or limited by the availability of technology and virtual space, which was centred in both the IT office, as well as in the communal ‘ether’ of the

site space. This kind of interaction, with its reliance on tools like the database and on systems of practice like centralised recording, showed how the dig house was a structural, physical space that radically affected the way people on site worked, and the way people physically worked radically affected the way they socially produced knowledge on site.

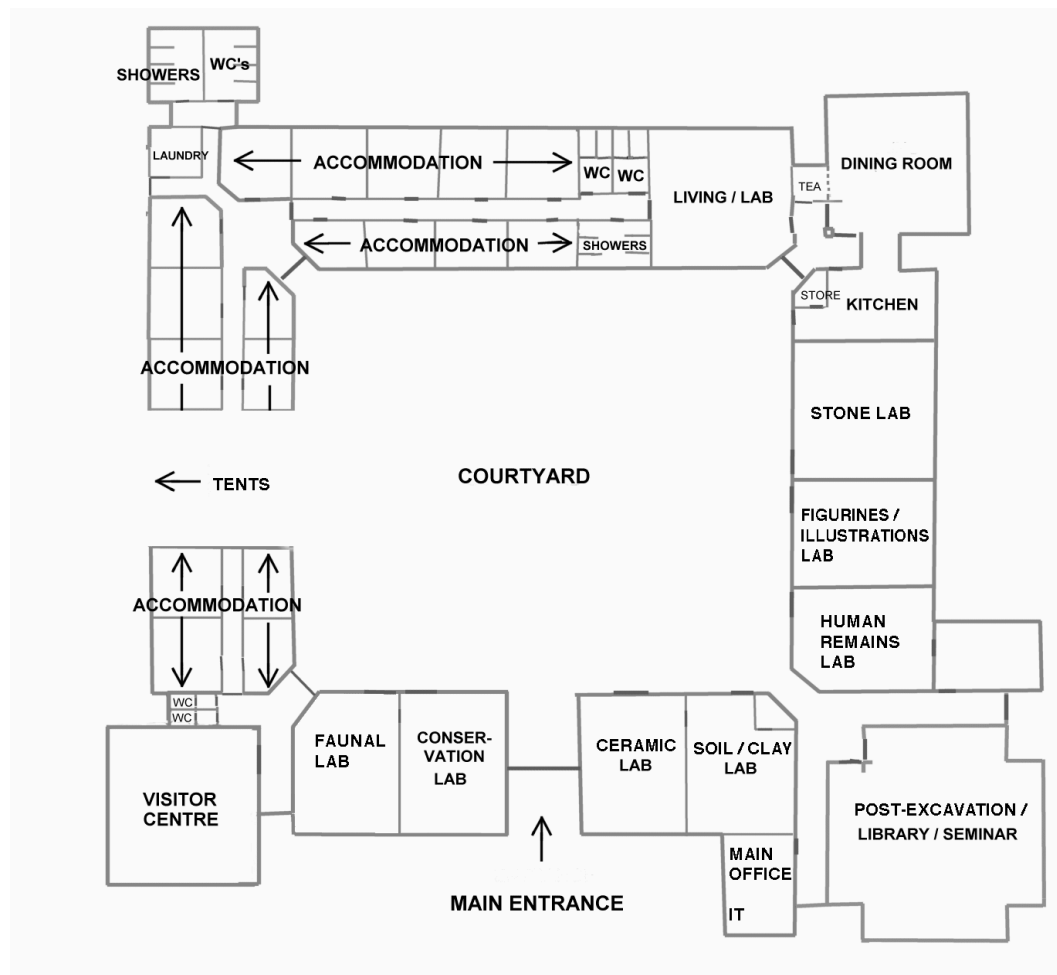


Figure 3: This is a map showing the general layout of the dig house. *Original map courtesy of the Çatalhöyük team; updates and modifications to the map made by Tera Pruitt.*

4.2.2.4 Public Spaces: Onsite Expert Witnessing and Public Engagement at the Dig House and Excavation Sites

The excavation sites and the dig house were also distinct social arenas for on-site public engagement. At the excavation sites, two large trenches on the East Mound had permanent shelters: the 4040 Area and the South Area. During the 2009 field season, I attended and observed the work of a group of conservators whose main efforts that season went toward the continued cleaning and preservation of houses under the '4040 Shelter'. This shelter covered several houses that were intended for future

excavation, along with several other houses that were intended for long-term conservation in a 'museum-like' way (personal communication 2009, conservator). The 4040 Shelter had traditional archaeology witnessing platforms (Moshenska 2009), walkways and tourist displays. They were deliberately left open and active, inviting both people inside and outside of the profession to come see the site and learn from displays, to intellectually interact with the archaeology and to potentially engage with interpretation more closely. I say 'potentially', because while the dig sites were left open and welcoming for people to enter and view, they were also set up to physically divide the public from the working archaeologists. For the most part, the visiting public that I observed during my time at the site were more passive spectators than active witnesses, in the terminology used by Moshenska in his study on how archaeological 'witnessing platforms' can be arenas of public engagement (2009).

Public groups could also visit the dig house, although they had very limited access. One corner of the dig house held the Visitor Centre, often called the 'museum' (Webb 2002). The Visitor Centre housed a small collection of artefact casts and replicas, and it offered wall posters that simply introduced the site interpretations for public consumption. This room was relatively small and bare, with not a great deal of significant information [Figure 4]. Instead, when I was present in 2009, a member of the excavation team (usually a high-level director like Shahina Farid or Ian Hodder) and/or a site guard would accompany visitors around the site, supplementing their Visitor Centre experience with verbal information and interactive question-and-answer sessions. The Visitor Centre and the Experimental House were public domains, while the laboratories and the living areas were the private domains of the Çatalhöyük project team. These two domains (public/private) were separated by a small, unlocked barrier door inside the dig house (see Section 4.3.2 on site access, below).



Figure 4: Image of the Çatalhöyük Visitor Centre (also known as the Site Museum). Here, Shahina Farid gives a lecture to teachers from the Turkish Cultural Foundation Teacher Tours in 2009. Photo online at: http://www.catalhoyuk.com/news/press_release_2009.html

4.2.2.5 Offsite Social Arenas: Laboratories, Museums, Press and Virtual Spaces

Another separate but related arena of knowledge production was in the off-site laboratory spaces, where various team members took material for further interpretation. In many cases during my stay at Çatalhöyük, I watched material being boxed and taken away to offsite labs, whether to conservation labs at the nearby Konya Museum or to scientific laboratories as far away as Stanford or Cornell Universities in the United States. This material, which was examined in various laboratories or presented to the public in museums like the Konya Museum in Turkey, was intentionally studied and then inscribed into a presentational form (texts, illustrations, displays) for a wide international public that interacted with the material off-site. Some of this material ended up in academic arenas, such as in academic textbooks or conference presentations, where Çatalhöyük material was deliberately crafted to meet the needs and expectations of this broader interested academic public.

Other material ended up in public arenas for groups that I came call the 'casual offsite public'. This public consisted of people who had a more or less indirect or casual interest in the site, who particularly interacted with Çatalhöyük material through their exposure to the popular journals, magazines or newspaper interest articles. For example, such individuals might be browsing a magazine and stumble into an article on Çatalhöyük, or they might find a link to the website, or accidentally stumble across the

site through virtual social spaces like the *Second Life* online virtual Çatalhöyük reconstruction experiment, which was set up by Ruth Tringham at Berkeley [Figure 5]. These groups often learned about and interacted with Çatalhöyük without any initial goals or aims, and without much previous interest or knowledge about the site. I found that these groups did affect knowledge production and interpretation of Çatalhöyük material, for they were always a considered audience when the team created and distributed general news releases, brochures, websites and virtual reconstructions. Several offsite Çatalhöyük interpretive experiments, programs and services grew directly from this relationship with the casual public, such as Tringham's *Second Life* project (Çatalhöyük Research Project 2010c).

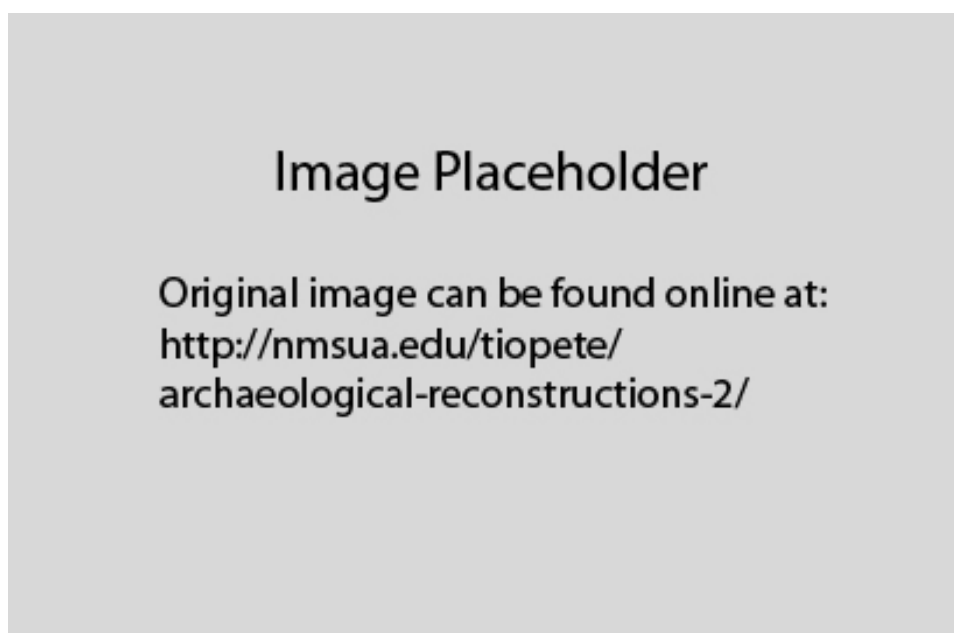


Figure 5: Screenshot of the Berkeley 'Remixing Çatalhöyük' virtual project on Second Life. Image from the New Mexico State University Alamogordo website: <http://nmsua.edu/tiopete/archaeological-reconstructions-2/>

4.3 Authority from Access, Spatial Constraint and Consent

4.3.1 The Authority of Spatial Constraint and Consent

This section argues that, at Çatalhöyük, the way any given person or group *accessed* the site was perhaps the most fundamental way archaeological authority was articulated. It was a primary way that people and material became distinguished as important, influential or authoritative. Issues of access were of central importance to the way the project was run, and central to the way individuals and units of the Çatalhöyük team defined their own identity—and authority—by establishing, opening or restricting its own physical and intellectual borders. During my fieldwork, the importance of *consent* in the role of building and controlling authority became apparent. The question arose: who has the authority to give or restrict access to archaeology? Fundamentally, access is a matter of individuals or groups relating themselves to social power asymmetries, for one person or group is always asking (or demanding) to receive consent to access archaeology from another person or group who allows access, meaning that the latter has control and authority over space or material.

4.3.1.1 Physical Access and Control

On the most fundamental level, access to the physical site of Çatalhöyük and its Neolithic remains, in terms of the management of simple proximity of individuals to the site itself, was a somewhat difficult affair. The ‘Remixing Çatalhöyük’ website—which promotes the virtual existence of the BACH project’s *Second Life* virtual Çatalhöyük by raising the distance and access problems of the actual Çatalhöyük mound—states:

It takes more than 24 hours of travel time to get from California to Turkey, and then more than an hour to drive from the nearest urban area to Çatalhöyük. Visitors are welcomed at the Visitor Center, but must be escorted throughout their tour of the site. Few people get to work at the mound itself. Archaeologists, however experienced, cannot work there without official permission from the Turkish government. A fence surrounds the mound and a guardhouse protects it. (Çatalhöyük Research Project 2010c)

As this BACH blurb explains, physical access to the site was complicated by a number of factors, but particularly: distance for non-locals, ownership and permissions rights.

Distance is an obvious issue regarding access to archaeology. The Çatalhöyük site mounds are located in the rural Konya province in the Republic of Turkey, in the centre of the country, far from any major airport or tourist route. Even for relatively

local populations (such as people in the cities of Konya, Ankara or Istanbul), the site is distant. No public transportation goes directly to the site; once you take public transport to major towns, the only way to get to the site is by taking relatively expensive taxis or to pre-arrange tourist agency transport from cities like Konya, the nearest large city (50km by car), or in Çumra, the nearest local town (10km by car).

Proximity and ease of access—as necessitated by geographical, financial or social reasons—naturally creates a dynamic whereby those who take the initiative to be present at the site have more potential access to the physical place and material. Those who have the interest and resources to get to the site are few in number, and they often come with specific aims and interests. While at a glance this may seem like a banal connection between ‘accessing the site by being at the site’, its importance relating to executive authority can accumulate on more nuanced levels. For example, when a site is so physically difficult to access, you might raise the question: who might actually be able visit the site other than those who have enough money and resources to get to it? What does this do to the executive and epistemic authority, influence, status and power relationships of those who can personally visit the site and those who cannot—the ‘haves’ versus the ‘have nots’? Since knowledge production is a socially interactive process, and since archaeological authority is accumulated from the interaction between humans and materials, might this power imbalance skew data and conclusions toward those who have the resources and abilities to access the original site?

Projects like the BACH virtual Çatalhöyük reconstruction on the online virtual-world program *Second Life* and Ian Hodder’s interactive website⁴³ have made the attempt to extend access of the site’s data to those who may not have the financial or physical abilities to see the site in person. However, regardless of the intent, this creates a dynamic power/knowledge imbalance between those who have seen the site ‘first hand’ versus those who have not—for two reasons. First, there is epistemic power in simply having close, intimate access to archaeological material, from the idea that ‘the closer you can get to the material, the better and more authoritative your interpretation will be’. This is the authority of prime sources, the idea that if you ‘come see for yourself’ and actively witness archaeological material, then you have more authority to speak experientially about a subject matter. A reproduction or an account, no matter how carefully attended, is always distanced from its source material. The creation of a

⁴³ <http://www.catalhoyuk.com>

reproduction is a social affair, always involving choices, negotiations and some kind of interpretation, so all information from a reproduction is received second-hand.⁴⁴

A second power/knowledge imbalance from distance and proximity comes from the fact that those who do attend a difficult-to-access site often have taken greater lengths and effort to reach it, which usually correlates to having a greater vested interest or stake in the archaeology. For a site like Çatalhöyük, the foreign (and Turkish) archaeologists who go to great lengths to obtain permits and visas, funding, space and time in their schedules, among other efforts in order to physically visit or work at the mounds and dig house, usually have more stake in the archaeology and the knowledge that is produced there. Importantly, they assert their stake by *physically occupying* the site each summer and actively influencing activities and interpretation taking place there, deciding what archaeology to keep or destroy, simply because they have occupying control and authority over that physical space. Similarly, highly interested groups like the Goddess Community members who go to great social, economic and physical lengths to visit the site have garnered respect, authority and positive attention (as well as negative attention in the form of territorial distain by local populations and archaeologists who disagree with their social behaviours and beliefs, see Webb 2002) for their efforts to be physically present and close to the site. The fact that physical presence and proximity increases authority, and that distance from the site and material decreases authority, also relates to *temporal* issues, discussed below.

For those members of the archaeological community and the general public who do manage to physically attend the site, the issue of proximity to actual physical remains is compounded by their limited freedom of access after they arrive on site. Most notably, permissible site access is heavily controlled, both by the archaeological project and by the Turkish government. Chain fences guard and restrict the boundaries of the site, and a guardhouse sits on the only open and accessible gate. The fences protect the remains from vandalism and illicit collection, and they prevent any unauthorised visitation to the site. Authorisation is most heavily controlled and ruled by the Turkish government: only team members with government permits are allowed to excavate and live at the site; and the government determines who is allowed on site, what material they are allowed to keep, and where they are allowed to go at particular times. However, in practical field archaeology practice, consent to access archaeological space and remains comes down to permissions under the control of high levels of the team hierarchy (like director Ian Hodder or field director Shahina Farid) and the Turkish site guards based in the site

⁴⁴ See Section 6.2.3 for discussion on first hand and second hand knowledge production and the concept of epistemic dependence.

guardhouse and the local village. According to government regulations, all visitors and site archaeologists must be escorted around the site by Turkish site guards. This results in all non-team members having their physical (and arguably intellectual) experience of the site explicitly directed and controlled by their site guides, whether they are guided by directors Hodder or Farid, or solely by the Turkish site guards.⁴⁵

During my fieldwork, asymmetries in executive authority were obviously manifest in the way the strict Turkish guard-accompaniment rule was relaxed for some individuals in everyday practice. For example, in 2009, it was common and obviously allowable for Ian Hodder or Shahina Farid—as well as a handful of team members who had been working at the site for a very long time—to walk up to the site mounds unaccompanied by a Turkish guard or representative. For a new team member like me, or even a specialist who was only returning to the site for a second or third year with little business on the mounds, it would be impermissible to visit the mounds alone after working hours. Inappropriate access could lead to being kicked off the project. While some rules were negotiable or could be relaxed (see next section), others were not, such as the ban on carrying of unauthorised material off the site.⁴⁶ During the quiet working hours of the non-excavating field season, it was possible for certain people or groups—for example, the conservation team—to get away with attending the site without a guard if it was obvious that they had work they needed to do at the dig site. During active excavation workdays, there were enough high-level supervisors and disruption of the mounds that only non-team members needed to be escorted by site guards or directors.

4.3.1.2 *Executive and Legal Consent*

As introduced above, bureaucracy and territorial rights complicate direct access to the physical site material remains at Çatalhöyük. Executive power over space and material at Çatalhöyük is a matter of ownership, and the Turkish government holds absolute authority over the site materials due to its powerful ownership claim. Despite being almost entirely filled with Neolithic remains, Çatalhöyük sits on geographical space that is the sovereign territory of the Republic of Turkey, and due to socio-

⁴⁵ See Section 4.3.1.3 for further discussion on this point.

⁴⁶ In his dissertation, graduate student Oguz Erdur recounts an announcement made by Shahina Farid in 2005, and I was given a similar mandate in 2009: “We are constantly being watched!” [Farid] explained, “Always be polite and answer all questions. If you pick something up from the ground, do make sure to throw it back. It is forbidden to import archaeological or geological finds out of Turkey. If you get caught with something you yourself might consider unimportant, a piece of stone or obsidian, just anything, you might get blacklisted. You’d never be allowed into this country again” (Erdur 2008: 75).

historical and geographical reasons, it is linked to the country's heritage. The government re-stakes this claim yearly with by doling out permits, visas and permissions of access to foreign archaeologists and visitors to the site, and the government asserts the high standards of practice that one must work under if they are allowed to remain. Turkish government officials (working off-site, in consulates and government administration) have ultimate authority over the permissions for who is allowed permits to stay on site and dig into the earth, and who is allowed to take material off the site and out of the country for research purposes. Famously, in one field season when the political climate between Greece and Turkey was particularly tense, all team members of Greek citizenship who applied to work at Catalhoyk that year were denied visas by the Turkish government. No reasons for this action was given by the Turkish government, but it is reasonable to surmise that this was a political move having nothing to do with the individual Greek workers and everything to do with large-scale national politics (Çatalhöyük team member, personal communication 2009).

Officially, the Turkish national representatives who live on-site alongside the team have the executive authority to decide who can touch what, when, where and how. Regarding archaeological remains, it is the on-site Turkish representative who decides which artefacts are sent to the Konya and Ankara museums, and what stays behind at the dig house to be studied or left in storage. All materials considered interesting or important are mobilised and change status at the point of a finger of the Turkish representative in the dig house: they either become prized museum objects, in special need of conservation and attention to detail for display, or they become second-class artefacts in need of permanent storage, put away in the dark and only seen by archaeologists who include them in their data.

Despite the rigorous rules and executive control by Turkish officials, in actual practice the movement of the team and the authority of the site's operation is nuanced and complex. Hodder and his team are given work permits with relative ease, due to the high-profile nature of the site.⁴⁷ To my knowledge, other than the now infamous ban on Greek workers, no other qualified applicants to Çatalhöyük who have been pre-approved by Hodder's project team have been denied access. By denying Greek

⁴⁷ In his PhD, the Turkish student Oguz Erudur tells Ian Hodder in an interview that: "I don't think this project is prone to being closed down at all [by the Turkish government], as you suggest you sometimes are afraid of. That is, of course, unless some huge and unforeseen scandal happened somehow. But I don't think that fear is necessarily too well-funded since, [Çatalhöyük] feeds very much into the whole discourse of 'our contribution' to the study of the human past. It's the flagship of that discourse actually. You are helping the ministry enormously, feeding into that discourse and the pride that there is this world-famous multi-national project we're hosting in our country and these foreigners found our country, this site in our country, significant enough to pour so much money into this business" (Erdur 2008: 262).

excavators and specialists access to an archaeological site, Turkish nationalists in the bureaucratic hierarchy were flexing their muscles, making a very obvious statement about their executive authority in order to assert a political point. Years later during the 2009 season when I was present, the Turkish government had proposed new laws that would “require that each excavation season last at least 4 months and that a Turkish co-director be appointed for each dig” (Baltar 2009). This announcement appeared to be, at least at the outset, more a matter of red tape, changing little actual practice or executive structure on site; the Turkish director was more about a display and assertion of executive right and authority. This added presence was more a reassertion by the Turkish authorities that they controlled archaeological territory in the region, and that foreign archaeologists were there by permission only. It was understood during my time at Çatalhöyük that, at any time, the Republic of Turkey could decide to take the control of the excavation entirely out of the hands of the foreign teams working there.

4.3.1.3 *Epistemic and Intellectual Access and Consent*

Access to an archaeological site and its material culture is also a necessary part of epistemic authority and part of the knowledge production process. Because knowledge is produced through social and material means, a scientist must have some degree of access to material—again, the closer the better—in order to justify his or her own hypotheses and conclusions. When I first arrived on site at Çatalhöyük, about a third of the way into the 2009 field season, the importance of the authority of consent and access in relation to archaeological space and material became particularly apparent. On my first day, I arrived alone by taxi and set foot in the dig house in the middle of a normal, bustling working day at the site. Because Ian Hodder was not present at the site, and because the field director Shahina Farid was busy, a woman named Jules, who was the Finds Administrator, gave me an initial introduction to the site.

Jules worked out of the crucial Finds Room laboratory (see Section 2.2.3), so she was an ideal or pivotal person for *social access* to the site. Because of her position as the Finds Assistant, Jules had executive and epistemic access to all of the dig house laboratories, because her job was to distribute different artefact types to the different labs. All material from the excavation sites went directly to her desk in the Finds Room, then she re-distributed the material out to all of the other laboratories by type. This made the Finds Assistant something of a executive ‘gatekeeper’ of all material on site. This gatekeeper role granted her a good deal of executive authority to control, grant or

restrict the movement of materials through the dig house. Jules, particularly, was also a charismatic personality and a returning team member (see Section 4.3.3, below), resulting in her holding ‘social capital’ with other team members. This charismatic authority granted her more social access and influence with other team members than, for example, a person like me who was arriving for their first day. Her personal history as a returning team member gave her stronger roots on the site, so she naturally held more social authority than me. The fact that Jules took me by the hand and introduced me to various members of the laboratories, and to others in recreational spaces like the site bar, meant that I was granted *social access* and given general social consent to be on the site. I was also given a space to sleep, met my roommates, shown the appropriate places for work and socialisation—essentially, I was granted some social authority to interact with others in this community and culture of archaeologists through the social introduction given by Jules the Finds Assistant.

I found that social consent to access physical and social space in an archaeological project is critical for two reasons. First, archaeology is a social and team-based activity, and thus introduction and social consent is necessary in order to access any epistemological and physical activity in the field. Secondly, social consent is a vital gateway to executive consent, allowing a person freedom of access to material. To expand on this last point, Jules’s introduction to the site was a tacit consensual agreement that I was a new and should learn the new social rules and my place in this social culture, but also recognition that I was now an included team member. I was given a space to work in a laboratory, and a space to sleep and eat in the accommodation; thereby I was granted physical access to the private team spaces of the dig house. This consent to access the private spaces and work areas of the dig house was a key step into my allowance of access. It was tacitly understood that I held less executive authority than the others upon arriving, by the simple fact that I was required to gain social consent to access these territorial spaces in the dig house.

However, even this grant of social consent and social access still did not give me any rights to access archaeological materials, or to use workspace and to interact with people during work hours (rather than in off-work social hours). I still needed consent of access given by a greater epistemic and executive authority in the site hierarchy, by someone with more authority than Jules. It was not until several hours later that same day that I met in more detail with Shahina Farid, the camp and field director, and only then did I obtain more *executive access* to the site. Jules opened social consent for me to be at the site and to interact in social spaces. It was Farid, though, who was accepted by others at the site as being the real gatekeeper for executive and authoritative access to

archaeological material and working spaces, due to her high position in the site hierarchy as camp manager.⁴⁸ Farid assigned me a desk for work, and she introduced me to the main office and to most of the excavators working in the same laboratory as my desk space. Since this consent to work was given by one of the top members of the site hierarchy, it affected the degree of *executive access* I had at the site. At this point, I was not only given a social place, but also a work space so that I could ‘do archaeology’ alongside the rest of the team. It was a very physical introduction and granting of limited authority to access the site, the materials and the other team members working there. This performance of granting and gaining consent for executive access to material was also obvious at later times that season, such as during the start of the excavations, when I was given more or less authority to touch specific archaeological material or to excavate in particular places when I worked under the direction and authority of my trench supervisors.

Finally, after my conversations with Ian Hodder, I was granted more executive and—particularly—*epistemic access* to the site. Shortly after arriving, I spoke with Hodder about my research and reasons for being at the site (I was an attending team member who had asked to join the project, rather than a team member who had been sought out by the project to join for a particular purpose). During our conversations, Hodder seemed to accept my epistemic reasons for being on site and to accept my research questions as valid ones. When it became clear to Hodder that conducting observations and interviews would be my clearest path to answering some of these questions, he introduced me to several key team members whom he thought might give me epistemological (and perhaps material) access to what I was seeking. Practically, this consent involved Hodder walking with me to a few laboratories and physically introducing me (personal presence, hand shakes and head nods) to those team members, who in turn agreed to speak with me. It was an informal and casual affair. However, this very physical and direct consent with team members greatly facilitated my access to both people and material during working hours. After the introduction and epistemic consent by the site director, I found that team members were later much more likely to put aside what they were working on in the moment and make themselves readily available to interview. Importantly, for my own research, a complete cocktail of *executive, epistemic and social* consent maximised my authority (or potential for

⁴⁸ It should also be noted that there was yet another stage of ‘resistance and accommodation’ (Pickering 1995) in the consensual process of archaeological access. While Farid was able to grant me consent to access material on a high level, it was still up to individual specialists in specific laboratories to grant me access to archaeological material that they were actively using or for things that they might have expertise on handling (i.e. a broken pot under conservation).

authority) on the site, and allowed me the closest access—and greater freedom to access—archaeological material, or at least talk closely with the experts who did have that freedom of access. I confirmed that, structurally, authority operates through such gatekeepers who can grant or further consent to access physical and epistemological space.

4.3.2 Public Access and Consent

A high level of public involvement and democratic (or ‘multivocal’) contribution has long been a concern at Çatalhöyük. Ian Hodder has argued that “Subordinate groups who want to be involved in archaeological interpretation need to be provided with the means and mechanisms for interacting with the archaeological past in different ways” (Hodder 1992: 186). Along these lines, in practice, Hodder has instituted interactive public tours of the Çatalhöyük excavation site, has established outreach programs for local and school groups and has supported community projects, for both archaeological communities as well as the general public. In the 2009 field season alone, the site welcomed a visualisation project team run by image expert Stephanie Moser from Southampton, a summer school project for local Turkish schoolchildren that promoted cultural heritage awareness, a collaborative participatory community research project on sustainable archaeology run by specialist Sonya Atalay from the University of Illinois, which included a project using local women’s community groups and interns from the nearby village of Küçükköy, as well as supporting general community and archaeology research by independent scholars and graduate students (Archive Report, Çatalhöyük Research Project 2009: 162-179).

However, in the 2009 field season, while observing some of the visiting groups, I felt that most were more passive spectators than ‘active witnesses’⁴⁹ engaged in interactive or multivocal interpretation. Two active public groups caught my particular attention when I visited the site and illustrate this point: one was a ‘casual public’ group, and the other might be called a ‘close interest’ group. To explain the first, I observed two sets of American schoolteachers who were visiting Çatalhöyük on a teacher-study tour with the Turkish Cultural Foundation. These schoolteachers had a casual interest in the site; many of them taught prehistory to young students in America, and the Turkish Cultural Foundation gave them an immersive pan-Turkey tour that taught them about

⁴⁹ The concept of ‘active witnessing’ is discussed by Gabriel Moshenska (2009).

Turkish history and prehistory. From speaking with the program organisers, I learned that Çatalhöyük had consistently been on the program tours for several years.

When I attended the teachers' tours, given by Shahina Farid and Ian Hodder, respectively, on two separate occasions in 2009, the Teachers' interaction with the site was almost entirely that of passive spectators. They were first shown the Visitor Centre. Shahina Farid (and Ian Hodder with the next group) supplemented the displays with a lecture, and then showed them the inside of the experimental hut, again supplementing the lack of displays with a lecture. The teachers were then taken up to the mounds and they walked around the carefully marked visitor paths and were shown the displays. They were also given lectures under the two main excavation shelters. These lectures were interactive only insofar as the teachers were willing to ask questions, and when questions were asked, they were usually given a prompt and direct answer. When the group was taken around the site, they were lectured to with the deference of a teacher/student relationship. The person giving the lecture, again usually Ian Hodder and Shahina Farid, supported this authority and structure by often physically separating themselves from the public group—with the group standing on platforms above the site, but the lecturer standing down on Neolithic ground, physically accessing the remains [Figure 6]. Undoubtedly this setup was arranged for reasons of safety—for both the public and for the conserved archaeology. The result of hundreds of visitors accessing the ground would be disastrous to the Neolithic remains and ill-advised for archaeological conservation. However, regardless of the necessity or intent, the outcome certainly reinforced typical authoritative structures of professional/public interpretation on site. This public group in this context was perhaps not provided with the “means and mechanisms for interacting with the archaeological past in different ways” (Hodder 1992: 186), and this teacher/student and interaction/spectator setup was typical of most groups that I observed who came to visit Çatalhöyük that season.

This is a point which, along with raising issues about control and authority of access, also raises connected issues with the postprocessual authority of the site (see Section 4.1.2). Previous debate in the field has questioned the ‘talk’ versus ‘action’ of the Çatalhöyük postprocessual program, and situations like this teacher/student arrangement of public displays and lectures at Çatalhöyük arguably undermine their own authority in postprocessual arguments for multivocality. On the one hand, the public was given intimate access to the *experts* of the project—they were guided by the site director, for instance, who is one of the highest experts at the site, with his intimate knowledge of the project and as the person who holds greatest executive authority of anyone besides the Turkish representative. But the public was not given access to the

physical remains themselves, nor given the opportunity to interpret the past in their own ways, or to offer their voices to the presumed ‘multivocal’ mix. The question raised by such an arrangement is, what exactly do we mean by giving people outside of the archaeological community “the means and mechanisms for interacting with the archaeological past in different ways”? Çatalhöyük is famous for technologically opening its borders and boundaries through such ‘means and mechanisms’ as the publically available website, and the BACH *Second Life* virtual world reconstruction. However, this is always secondary access to data and yet another step removed from the ‘interpretation at the trowel’s edge’. This is a problem long recognised by Ian Hodder in his own theoretical literature, but which, to my observation in 2009, had not been fully dealt with or negotiated by Hodder or his core team, beyond the initial outline of a problem, at least regarding general or casual public groups.



Figure 6: Hodder giving a site tour to teachers on a Turkish Cultural Foundation Tour.
Photo by Tera Pruitt.

A second public group that I observed—who I call a ‘close interest’ group—was given more involved and intimate interaction at the site. Examples of a ‘close interest’ group included members of the local Turkish community, whose close association with the site meant the project was more inclined to open boundaries and encourage

interaction, as well as other groups like the Goddess Community. I found noteworthy interactions between the Çatalhöyük team and a Goddess Community group that visited in the 2009 field season. The Goddess Community is arguably both a subordinate and a stakeholder group, and a great deal has been written about their involvement with the site during Hodder's excavations (Webb 2002; Rountree 2007). In June 2009, a small group (seven or eight people) on a Mother Goddess tour came to the site, and it was clear that they had phoned ahead to schedule a time to visit. When they first arrived, they were warmly (and relatively intimately) greeted and then given tour of the mounds, much like the American teachers. However, unlike the teachers, they were also allowed back into some 'private' areas of the dig house, such as the back vegetable garden and the dining hall. They were also offered tea and welcomed to sit at the lunch tables, and an interactive discussion about site material took place. The composition of this group discussion included the seven or eight member Goddess tour group, Shahina Farid, myself, and later Scott Haddow, a team archaeologist.

One of the notable exchanges of this group discussion revolved around a human skull that had recently been 'rediscovered' by a team member who was inventorying James Mellart's human remains collections from the 1960s. This skull was remarkable in that it had been stained with bright red pigment. Initially the team thought the pigment was common red ochre, but after the skull had been analysed using PXRf machine (or, as one team member described it to me, "was zapped with the science fiction laser that tells you its mineral composition"), the team discovered that the pigment was actually cinnabar, a common ore of mercury that would have been poisonous to process and handle [Figure 7]. This interesting scientific conclusion had only been reached in the previous few days, so Shahina Farid brought the skull to the attention to the Goddess group as they were casually drinking tea in the dining hall. The offhand mention turned into a table discussion that included topics like whether or not cinnabar was carried on the silk trade, whether or not silk was traded in the Neolithic, and what kind of symbolism could be made from the pigment mark on the red Mellart skull. Archaeologist Scott Haddow, who had found the skull during his inventory, was called to the table, and he brought his computer full of images of the skull to show the group. The group asked the archaeologists many questions about the mineral makeup of the pigment and the find, and in turn they tried to offer interpretations. It was noteworthy that the Goddess group was given more intimate team information and findings that would not have been told to most public groups. However, they were not given access to the original human remains: they saw only photographs on Scott's computer, even though the original materials were sitting in a nearby room. This was a clear communication of executive

authority by both the archaeologists, but also by the Turkish authorities, who have a history of tension with the Goddess Community and who may not have liked them accessing archaeological material (Webb 2002).

One of the more interesting (and perhaps stereotypical) exchanges between the team and the Goddess group involved a Goddess group woman who was at that time an anthropology professor at Michigan State University. After briefly examining the photos, the Goddess member suggested that the cinnabar was used for healing purposes, because the stripe across the brow is on a very energetic part of the body and that many cultures highlight that area of the body for healing purposes. Scott pointed out that the pigment was painted over and into the eye socket, which indicated the person was dead and defleshed before being painted. This caused the woman to pause and think, and a few moments later she insisted that perhaps the Neolithic owners could have painted the skull and then put it on a shelf in the house and still kept it as a symbolic or energetic object that represented healing. Scott and Shahina Farid were unconvinced. However, this was an example of an interpretive negotiation of data and an epistemic engagement by the Goddess community, an exchange between the group and archaeologists. For me, this exemplified a situation where the project attempted to give a subordinate or public group greater access or the “means or mechanisms” to actively engage with material. However, when observing this interaction, I realised that the Goddess group was given only access to secondary photos of the material and access to the experts who had been privy to the original material. I still felt that for the most part these two groups were doing a great deal of talking, a good amount of listening, but there was little absorption—or desire to create an agreed account of the past—on either side. The archaeologists held the clear authoritative ground and were not interested in giving much space for alternative interpretations, other than making sure the ‘close interest’ group felt respected and were given attention that went above and beyond an average public group, like the American teachers.

In turn, the Goddess group did not seek access to the original materials, although some members of the group seemed to have a defeatist attitude when it came to interaction and interpretation. One group member recalled to me, for example, an instance when her colleague had made an artistic banner to be placed in the museum, but the banner currently sits hidden in a drawer in the dig house. When explaining this to me, the Goddess member recalled the banner with a positive tone and attitude, but then this memory led to a less positive discussion about the historical lack of inclusion of Goddess material and interpretations in the site's Visitor Centre. A Goddess group member told me that, in the original design of the museum, there was no inclusion of

any alternative interpretations, and so their group contested (particularly by writing comments in the visitor book). Their efforts and contestation were rewarded, and the archaeological team soon designed a freestanding interpretive panel, which then was placed on a temporary easel in the room (as opposed to on a fixture more permanently attached to the wall). The temporary nature of the freestanding display, rigged on the collapsible easel, was noticeable when I visited in 2009. In a later conversation with Shahina Farid, she confirmed this account, but added that when the team added the display panel in the museum after complaints, the Goddess community was still not entirely happy, since the team had used phrases like “Mother Goddess Worshippers” instead of apparently more appropriate terms “Goddess Community”. The team then corrected this mistake by printing the correct words onto white sticky paper and then physically sticking the new words over the old words on the panel.

For me, this account of sticky-taped words and banners hidden in drawers offered a tangible example of the physical/spatial dimensions of interpretive contestation involving site access. At Çatalhöyük, the archaeologists’ attempts to engage in multivocality with the Goddess community manifested in small-scale physical and mental power struggles. The Çatalhöyük team asserted its authority over both physical remains and interpretation in its restriction and accommodation of dig house space. They easily represented a paradoxical practical arrangement—they seemed to think it was “reasonable to abandon abstract objectivity and make trials of resistance commensurable...Talk to people, understand them, persuade if necessary; instead of patronising them by playing expert” (Shanks and Hodder 1995: 20). However, they did so by forcing a setting where the Goddess group felt respected, but where no one was foolish enough to think that the archaeologists were attempting to engage in a dialogue of commensurability or were not ‘playing expert’. In this setting, the lines were clearly drawn, and the archaeologists asserted their interpretive authority over material things and physical space.



Figure 7: Photo of the Painted Skull, taken by Scott Haddow, from the 2009 Archive Report (Çatalhöyük Research Project 2009: 127).

4.3.2.1 Public/Private Domains and the Narrowing of Access

In general, public space at Çatalhöyük was obviously separated from the rest of the working space in the dig house. A small barrier door not only divided the public from the experts, but also (whether intentional or not) singled out a status and division. Access to the dig house and archaeological material was physically narrowed by how much executive authority you had on the site. First, there was the barrier door, which only certain visitors were allowed past during working hours.⁵⁰ Allowed visitors included the Turkish locals who were involved in the community participation projects, the Goddess Community groups and touring archaeological teams from nearby sites (such as the project team under the direction of Nicholas Postgate), the teacher-student groups in university-level archaeology programs, and independent researchers in archaeology who were contemplating future research at Çatalhöyük. All of these groups, it should be stressed, needed to have previously scheduled appointments to access more private areas of the site beyond the barrier door. Casual visitors and other non-university-level teacher groups were rarely allowed access beyond the barrier. Even when the special interest groups, such as other archaeological teams or the Goddess groups, were allowed access beyond the barrier, they were generally kept out of the labs and stayed in the public living and recreation spaces, like the dining room, the veranda and the seminar room. Part of this control over space and territory was due to the

⁵⁰ Entertainment groups, such as visiting Whirling Dervishes, were allowed past the barrier door during non-working hours.

authority exerted by the Turkish government and the Turkish representative on site; however, another part of this regarded pure epistemic and executive authority held by the team members, who wanted peace and no one bothering them while they worked.

Even if you made it past the barrier door and held the blessing of the government, space and access on site still narrowed depending on who you were. Public/private access to *all* of the laboratories, for example, was the domain of only high members of the 'site hierarchy' like Farid or Hodder. For others, laboratories were tacitly restricted to workers in their own respective laboratory teams. For example, as an ethnographer, I was given a desk and space to work in the seminar room with the field excavators. I understood this to be my working domain, and this was the one laboratory that I had the authority to access without question or comment. However, when I would enter the Faunal or the Human Remains laboratory, it would draw significant attention, and I would need to state reasons for my intrusion, because I had no ostensible authority to be there.

When I asked members of the project team to explain their feelings about this kind of territoriality on site, most of them were initially reluctant to comment, or would begin commentary on *intellectual* territory and publishing rights. This reluctance did not come from sensitivity to territoriality, but rather the opposite—they did not notice the tight division of space and domains until it was pointed out to them, because there was an underlying assumption that this was simply the way space should be divided and operated in an archaeological operation.

4.3.3 Temporality

A final division of space that should be mentioned is that of *temporality*, which offered a very palatable division of authority at Çatalhöyük while I was on site in 2009. Temporality is a sense of space too: a day is divided by the timing of events, the movement of things and people, and moments of appropriate behaviour. The most relevant issue regarding temporality had to do with the duration or number of times that any given excavator or member of the public (like the Goddess Community) had visited the site. Longer duration or repeated visits to the site increased the executive and epistemic authority of any person. One excavator told me that she felt that status on site often "more or less divided by the people who have been here for a while and the people who haven't" (returning team member, personal communication 2009). Many of researchers, such as myself, were only on site for one field season and were new to the project community. Others had been with the project almost from its inception,

returning year after year, basing their entire careers on their annual Çatalhöyük research. It was clear that returning researchers, whether on their second year or their tenth, generally had more social and executive authority, as they had been able to gain social currency with more returning members, and they had greater experience with the rules and interacting in the physical project space. They also often had more epistemic authority as well, since their experience with the site and material was accumulative over time. While the team ‘lab heads’ were ultimately in charge of their own laboratory spaces, in some cases other mature and returning team members who were not official ‘lab heads’ seemed to hold almost equal authority and status on site. This was particularly the case with some members of the faunal and human remains labs. The correlation seemed to be that greater time at the site equalled greater experience, and greater experience led to greater expertise, and greater expertise led to greater epistemic authority, which in a scientific project like Çatalhöyük, equated to greater presence and executive authority to access social and physical spaces.

While the word ‘territoriality’ was sometimes debated in my interviews, the idea that there was a division of status and social order at Çatalhöyük based on presence—*permanence* versus *transience*—was not debated. Çatalhöyük is an unusually large operation, with as many as a hundred official team members drifting in and out of the dig house each field season, each with diverse and complex interests and reasons for being on site. The instability of so much diversity and movement has been commented upon before. In 2000, Shahina Farid wrote, “Instability within the project was seen to result from several factors: the constant change of personnel on a yearly basis, and throughout the season the arrival and departure of different teams working to their own schedule. Also the methodologies themselves” (2000: 27). In her commentary, Farid is critical of so much movement, arguing that the destabilizing “was found to be unnerving and unsettling” and that “The ‘fluidity’ in the written record, however, results in big differences in recording from one year to the next, requiring constant revision of previous seasons [*sic*] data and at some stage this process may become incompatible” (2000: 27). She points out that “Hodder interprets this as a good thing, arguing that: “...a lack of stability is necessary if a critical approach is to be taken and if the project is to remain responsive to a changing world around it” (2000: 27; Hodder 2000).⁵¹

Regardless of the implications for methodology or interpretation, the constant movement of new team members, who came and worked alongside the constant presence of others that had maintained a continuity working there, created a social

⁵¹ See Section 4.4.4 for further discussion on Hodder’s practical actions on the theory of instability in interpretive practice.

order and hierarchy based on permanence versus transience. The sense of permanence manifested in things as basic as cups in the sink of the dig house tearoom. Those who were returning to the site had personal mugs or cups, while new team members had to forage for a mug to have tea, and they had to quickly learn which mugs were off-limits because some team members were territorial if another team member used their personal mug. Eventually, new team members who stayed with the project long enough purchased their own mugs and kept them in the tearoom, claiming social space as their own. Metaphorically, this sense of physical territory and space operated in similar ways in the laboratory and in epistemological space. Returning team members had often previously staked claims to desks, methodologies and social hierarchies, and they had certainly staked experiential claims as to what 'went on before' and 'how things are done' on site.

Also, the instability of constantly reintroducing new team members to methods as well as social and work spaces resulted in a constant teaching/apprenticing process at the site. During excavation, the returning senior excavators had to constantly devote some of their time to training not only the untrained field school undergraduates from Stanford University, but also had to teach any experienced excavators who were joining the Çatalhöyük project for the first time how to excavate according to Çatalhöyük methods and protocol. While Hodder might have intended, or might argue, that this constant re-teaching of methods would enable constant team interaction with the process by which students were taught, therefore enabling reflexive dialogue with method, I found the opposite to actually be taking place. Instead, I found that the constant re-teaching of methods rather secured those methods firmly and authoritatively in place. The constant re-teaching solidified a process by which people said 'this is the way we teach newcomers' and 'this is what we do at Çatalhöyük', thereby blackboxing methods into stable 'ways things are done' rather than opening them to any reflexive consideration.

For methodology and epistemology, this manifested in two notable ways: one was the way that the laboratories were splintered into 'pods' with unique work cultures, and another was the way that time affected interpretation, both methodologically and on a final 'final product' level. Regarding the former, a good example is how the Faunal laboratory operated. In 2009, the Faunal lab was almost militaristic in detail, and very well organised. The team operated under strict operation procedures and rules. Boxes of new faunal remains would come into the faunal laboratory from the Finds Room, and then go through a rigorous scientific process of scrutiny and recording. They had a flow chart of appropriate protocol, with 'checking' moments when authorities (supervisors

or team leaders) were responsible for assessing whether or not procedure had been followed appropriately at certain levels, or whether interpretations by more junior members were accurate at certain points of the work flow [see red boxes in the faunal procedural flow chart, Figure 8]. This robust team structure operated much differently than, say, the Conservation laboratory, which was also a multi-person team, but which had a much more fluid and democratic procedure. At some point, Hodder suggested to me that the Faunal lab was structured this way because of the personalities who were involved, in all likelihood referring to the ‘lab heads’ who organised the Faunal lab authority and created the flow of practice.

Such a structure showed how returning members and longer durations of time spent with material cemented authority in a specific laboratory culture. I would argue that this authority accumulated naturally—repeated interaction with familiar material stabilised interpretations, and authority was gained through this time-garnered ability to identify material in the appropriate categories. Furthermore, I would argue that this militaristic structure and hierarchy of authority in the Faunal Laboratory developed somewhat naturally because of the *type* of material involved. The identification of faunal remains is entirely categorical—it involves the deliberate sorting of bones into pre-set groups, which are developed from an understanding of bones from known modern animals. The key to faunal identification is personal experience in recognising the difference between bones as similar as those of, say, a goat and a sheep (which is so difficult that at Çatalhöyük, they often get lumped into a ‘sheep/goat’ category if the specialist is unsure, or if the skeletal remains are less complete). This ability to understand the pre-set categories and the ability to accurately identify unknown, newly found remains develops with experience, and experience develops over repeated interaction with material over time. It is no wonder that the ontological setting in this scenario—the type of material, and the type of activity involved in being ‘faunal specialist’ in the Faunal Laboratory—directly enabled and constrained the way personal and institutional authority accumulated through *time* in this specific laboratory culture.

During my stay at the site, it was also suggested that ‘time’ (particularly relating to the extent of experience and duration of time at the site) also affected the interpretive process and the site philosophy as a whole. One example emerged from a discussion with a team member who had been returning to the site for a number of years. In one conversation, he mentioned that there were early attempts at Çatalhöyük for site ‘labels’ to be neutralised during excavation recording practice—in other words, if a team found a giant waste pit or found a fired cooking space, these areas were initially supposed to be called ‘pits’ rather than ‘middens’. A term like ‘fire installation’ was supposed to be

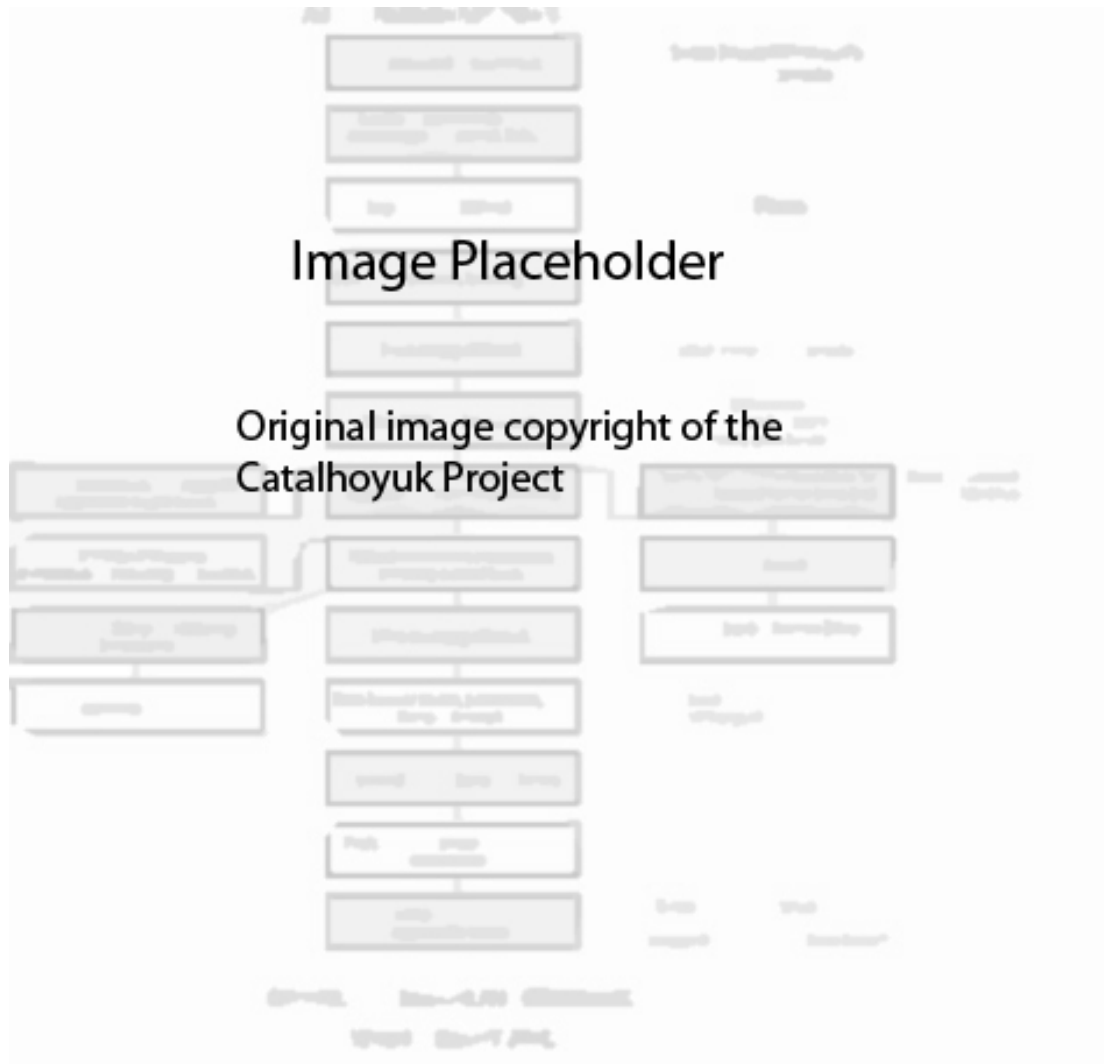


Figure 8: Faunal procedural flow chart, used by the Çatalhöyük faunal laboratory team. Note the rigid structure and the red boxes with 'checkpoints'. It is also significant to point out the notation on the side for the checkpoints: "ideal - more for new people". Familiarity with materials and methods breeds stability and authority in the knowledge production process. Screen shot courtesy of the Çatalhöyük Team database archive.

used rather than the term ‘oven’ or ‘hearth’, because of the strong connotations attached to the latter words. The idea was that, by using more open language, team members could cognitively keep associations between features and words open for greater interpretive flexibility and reflexivity. However, the team member argued that over time the site archaeological material had become repetitive and familiar, so that when the team ran across such features, they thought they were clearly middens and ovens. Because of their familiarity with the recurring material, the team had abandoned most of the ‘open’ categories and had collapsed back into using these specific categories like ‘midden’ or ‘oven’.

In this case, I would argue that the duration of time that the whole project had spent at the site had created a familiarity with the archaeology, and this had caused a fundamental shift in methodology and interpretive practice—a stabilising effect. While the open categories might have been a good experiment at the beginning of the project, the recurring physical properties of the material created a stabilising authority of the interpretations themselves. I would argue that any further use of ‘open’ categories in such a scenario would only become new terms for the same mental categories or interpretations. In such a case, the physical material directly limited or constrained any interpretive category that might be used or developed. Authority—in both the idea of interpretive categories and in the interpretive process—manifested through this stabilisation, where time and familiarity only further cemented an understanding of the physical remains.

Another example of how the authority of time duration at the site had affected interpretation involved an instance when the excavation field team was running through previous seasons’ work in their lab during the study season. When running through the previous year’s data and the Harris Matrix charts, one team member identified an opening in one of the Neolithic walls, which appeared to be an access leading out into an outdoor ground space. She was having difficulty explaining this opening without calling it an ‘access’ or a ‘door’. Çatalhöyük is famous for its narrative describing exotic Neolithic houses that were built with no streets or side doors, with the buildings only accessible from small openings in the roofs. On this occasion, the field excavators had an informal discussion about this mysterious opening in the wall. The area on the wall appeared to be built without bricks, but they could not agree that the space was a door—because (they kept insisting) Çatalhöyük had no doors. At one point, one of the excavators mentioned that Shahina Farid was “quite cross” at the mention of a possible side access door, because ‘Çatalhöyük culture had no streets’. This idea had been ingrained and established through many years of experience and fieldwork. It appeared

that Farid had said with authority that this was not a street, because there were no streets, because in the long history of the project no streets had never been found. The group ultimately used the word “heresy” to (jokingly) describe this debate, and after a period of momentary drama and humour, the issue was put to rest with different (and less controversial) terminology, and the Çatalhöyük record was spared of any further mention of streets or side doors. This example represented the authority of familiarity, the authority of repetition that the material had over interpretation, and the authority of those who had greatest executive control at the site because of their long duration of experience with that material.

In both of these cases, the interpretive outcome of scientific practice manifested from a network of operations between people and materials. Interpretations and accounts were stabilised by the authority of those who had experienced the site first-hand for a long duration of time, from repeated ontological interaction with archaeological material that was repetitive in nature, therefore allowing recognisability, and finally, from the negotiation of authority between the various team members who were assessing or interpreting that material. Higher status personalities (team leaders or other experienced returning team members) had authority that was often based in a longer duration of time and experience with the site, which resulted a strong presence and greater epistemic power over the production of knowledge.

4.3.4 Knowing Your Place: The Power of Space, Structure and Division at Çatalhöyük

At Çatalhöyük, people could establish a foothold of authority in three ways. First, a kind of pragmatic authority could be gained by quickly learning the routines of the site or place in a laboratory. By socially and ritually integrating, a person could build personal status and reputation as a competent individual, leading to greater authority. In daily practice at Çatalhöyük, everyone on site had a niche and a space, and they quickly learned the appropriate routines and language—at the risk of appearing ‘aimless’ if they did not perform. At one point in the 2009 season, a few of the Stanford undergraduate field school students were found to be ‘goofing off’ and avoiding work. One of the (authoritative and longstanding) lab specialists mentioned that this behaviour could affect the students’ feedback and recommendations by other team members. The specialist continued explaining that Çatalhöyük, like many excavation teams, often operated as a watchful, tacit social ‘panopticon’ (her word), where everyone is aware of everyone else at any given point of time, assessing their

trustworthiness and competence. While this might be a dramatic interpretation, in actual practice, authority on site did appear to operate in a watchful fashion: while on the one hand, team members with the titles like 'leader' or 'director' did elicit an appropriate authority and following based on their institutional positions, other members were simply regarded as more competent or able and had been consistent in gaining status and authority by virtue of doing the appropriate actions for a significant duration of time. In the 2009 season, the Stanford students who acted as enthusiastic and able apprentices gained greater authority over the course of the season, which manifested in how they were treated and what responsibilities they were given, while those who slacked off were often discussed as having poor work ethic, their authority and social status lessening over time.

Most importantly, such authority operated within the *physical* and *structural* operation of the site. Authority was most likely to be quickly accumulated by a team member who was consistently performing the appropriate behaviours of a competent archaeologist. Such behaviour legitimised their self-presence, because they were working correctly within the stabilised methodology at the site. Authority was even more quickly and widely gained if they handled material in ways that others at the site deemed was appropriate. Authority was less likely to be gained when a person undermined social structures, created new or innovative interactions, or tested boundaries. Any behaviour involving risk or change, especially when it involved the handling of precious archaeological material, was not well-received and would likely not raise the authority or status of a person on site.

People could also establish a foothold of authority by building a sense of alterity versus self. Alterity went beyond the categorisation of people as 'professional' or 'alternative' and involved the definition of space, persons, practices and authority on the site into inclusive/exclusive categories. Groups were divided by teams, specialisms, laboratories and sometimes even by nationalities. This happened often accidentally, but also intentionally. By accidentally, I refer to the way some age groups and professional groups were often formed by virtue of who one might routinely interact with on a daily basis, often a product of schedules that had happened to align, or work space that was randomly assigned to team groups. Returning team members (from previous years of excavation) often ate at the same lunch table because of friendships that had developed over time, and laboratory groups often started and stopped work at the same time, therefore bonding as a 'pod' and creating socially exclusive units. At Çatalhöyük, field excavators were mostly British, therefore a 'British group' was very present on site, as were the 'Stanford students' group from America who were united by age, nationality

and experience [see photo in Figure 9]. Again, groups like those of specific nationalities were often formed somewhat accidentally, despite the reality of their extant category and exclusive bond. The British field excavators, for example, were often asked to join the project because of pre-existing social/work networks, from a current team member's personal knowledge of a former colleague's competence and good practice. On a more deliberate level, sometimes entire Çatalhöyük teams intentionally stuck together in social and work settings, such as the West Mound Team. The West Mound Team would, for the most part, work together, eat together and socialise together, mostly distinct and separate from the rest of the East Mound Team. This division was created because of the very real geographical distance that separated the East and West Mounds in the Çatalhöyük landscape, and it impacted the social and interpretive exchange that occurred between these two groups. This kind of 'culture creation' is not unique at Çatalhöyük; Cornelius Holtorf records very similar scenarios at his excavation site at Monte Polizzo in Western Sicily, where he argues that "Learning such rules of the game, or tacit knowledge, can be of crucial significance" in your ability to succeed as an archaeologist (Holtorf 2006). At Çatalhöyük, the social and spatial interaction between such groups directly affected what persons or specialisms were present in any given physical space at any given time—and importantly—this interaction affected what ideas and intellectual materials were exchanged during social and work hours.



Figure 9: Darts with British and American national flags, and a Turkish beer. These symbols of recreation also represented some of the divisions behind groups that worked as tight units on site. Photo by Tera Pruitt.

Finally, there was a distinction in space and structure between those who were archaeologists, and those who were not—public versus private, expert versus novice, observer versus participant. Regardless of intent, various public groups were physically separated in space (see Section 4.3.2, above), which promoted *alterity*. Because the site was divided in public versus private spaces, it narrowed and could be limited in access. This led to a distinct ‘us’ versus ‘them’ feeling that permeated when public groups visited the site. The Goddess Community, for example, while welcomed and actively included in the site, were still part of an entirely different social and intellectual group. Both groups—archaeologists and the Goddess Community—stuck together and kept within their own boundaries when they visited the site, and only team members like Shahina Farid and Scott Harlow, who were scheduled to talk with them or who were specifically invited, attended them. After the departure of the Goddess Group, many of the other Çatalhöyük team members were excited to hear that the Goddess group had visited and were curious, but alterity seemed to keep the two groups from mingling at any other time. Such alterity and social boundaries extended beyond just subaltern groups like the Goddess community, extending even to professional groups that came to visit. One notable example was an archaeological team of a nearby excavation run by

Nicholas Postgate from Cambridge, which included many postgraduate and undergraduate students. Like the Goddess community, they were allowed back into the more private areas of the dig house, including the dining room and were given brief tours of the labs—and also like the Goddess community, their experience was controlled and heavily guided in space and time, and their access and duration at the site was limited. This was partially due to the strict rules and watchful eye of the Turkish government representative, but also due to the nature of the site as a working space where people did not want to be bothered. In some cases, this was also arguably because inner-circle team members enjoyed the fruits of being an academic whose work and site was worth witnessing, and who appreciated the hierarchical separation that comes from who is allowed to be a participant versus just a viewer in that setting.

A main outcome of my observation and fieldwork at Çatalhöyük was that space and the physical consent and structure of any experiential plane can greatly add or decrease individual or group authority based purely on who executively controls or narrows the access of that physical space. Control of space directly affects the production of knowledge. Who is allowed to get closest to material and who is allowed to engage with experts or non-experts directly affects what dialogues even have the opportunity to arise. Hodder himself has touched on this subject before, by recognising that “interpretation begins at the trowel edge” (i.e. that more direct and physical reach of the material in question breeds more ‘close’ and arguably more ‘accurate’ interpretation, lending the participant more authority). However, despite acknowledging this, a recognition of the way professional authority *is actually operating* has been little discussed. Outreach programs may be described and celebrated by the project in their newsletters and archive reports (Atalay 2009), but the basic fundamentals of *how* one person is physically enabled to touch material while another person is not, and how such a difference actually effects individual and collective authority in the construction of knowledge, I found to be a hazy and skimmed subject at the site. This lack of recognition of the *actual operation* of physical things, the material nature of interpretation, and the accumulation of authority was also present in the interpretive process as well as the methodological setup of the site. The next section discusses this in more detail.

4.4 Inscription, Translation and Blackboxing: Authority in the Solidification of Representations into Accounts

4.4.1 Authority through the Stabilisation of Practices

In archaeology, the production, exchange and consumption of academic messages involve a number of social processes—notably, *inscription*, *translation* and *blackboxing*—which affect the way knowledge stabilises into solidified, authoritative ‘final product’ versions of original fluid and processual ideas. Scholars like Bruno Latour (1987) and Michael Callon (1986) coined these terms from their observations of natural scientists at work in the field and laboratory. *Inscription* is the act of creating new material products that represent the actions and ideas behind the social production of knowledge. For example, in archaeology, the creation of site records, like elevation plans or GIS maps, formalise or inscribe a moment of excavation activity, thereby representing a four-dimensional source (material remains excavated at a specific moment of time) as a two-dimensional text or illustration. *Translation* is the process by which various actors engage with, negotiate and make choices about how to use an idea, artefact or a moment to benefit their own aims or advantage. For example, a Goddess Community member and an archaeologist might each individually view a female figurine found at Çatalhöyük and translate their own meaning and interpretation of that object for their own purposes. By translating the figurine to the advantage of their own view of the ontological and social world, they further advantage their own authoritative positions within their own social group. *Blackboxing* is the process in which methods and inscriptions become set as an authoritative standard or norm, a ‘way of doing research’ which goes unquestioned—until something goes wrong or contestation brings issues about the way a system operates to the forefront. This section expands the example of Çatalhöyük in order to address the way these three processes can operate in the discipline of archaeology. It also highlights where and how authority impacts the interpretation of archaeological knowledge through these methodological processes.

Andrew Pickering likens the social production of knowledge to an interactive struggle between human and material agents, where “scientists are human agents in a field of material agency...[and where] human and material agency are reciprocally and emergently intertwined” (1995: 21). It is through this reciprocal interaction—social practices involving the routines of examination, observation, data-collection, analysis, presentation and publication—that “things get performed (and perform themselves) into relations that are relatively stable and stay in place” (Law 1999: 4). Stabilising knowledge into authoritative accounts is an active and performative process, whereby

the fluid actions and relationships of scientific activity become stabilised into formal end-products. Like in the Archive Report front page photograph [Figure 10], where human and material agents are interacting within predefined space, it is through performed activity itself that knowledge is constructed.



Figure 10: Front cover photograph of the 2009 Çatalhöyük Archive Report. Knowledge is actively performed through the processes of inscription and translation (Çatalhöyük Research Project 2009).

4.4.2 Authority in Inscription and Translation: Solidification through Representation, Circulation and Mobilisation

The production of texts or representations is often referred to by Science and Technology Studies (STS) researchers as the process of *inscription*. In archaeology, textual or representative products, like museum displays or archive reports, are frequently the most stable outcomes of our knowledge production process. To paraphrase Law, *inscriptions* are the systems and performances that result in new materials. New materials are seen to be related to ‘the original substance’ of the scientific activity, but are seen to be things that summarise or ‘inscribe’ the original activities and materials into new forms (Law 2004: 20). These new forms are the ‘end products’ that emerge from scientific activity—the most notable of which are *texts*. The focus of much previous STS research has been on the scientific production of texts. As John Law writes, “Energy, money, chemicals, people, animals, instruments, tools,

supplies, and papers of all kinds, move into the laboratory. At the same time, people and (different) papers and maybe instruments, together with debris and waste, move out. Looked at as a system of material production, then, the major product of the laboratory turns out to be *texts*" (Law 2004: 19, emphasis in original).

In archaeology, like in natural science, we create texts from our scientific activity: site reports, scientific reports, scientific journal articles and books. In archaeology we also produce other material end-products to supplement or extend beyond our texts, such as maps and illustrations, site plans and elevation charts, museum displays and physical reconstructions, all of which are inscribed new 'products' that are based on original material, which inform 'knowledge' about the past. The archaeological practice of *inscription* regards "all the types of transformations through which an entity becomes materialized into a sign, an archive, a document, a piece of paper, a trace...They are always mobile, that is, they allow new translations and articulations while keeping some types of relations intact" (Latour 1999: 306-307). Pivotal activities of archaeological work involve the production of inscriptions like notes, drawings, images, texts and databases.

A classic example is the discovery of a pot in an excavation. After its discovery, the pot's dimensions are first drawn into a site plan and recorded on a context sheet by an excavator—inscribed into a new two-dimensional paper record and image. The pot may be removed and its context may be destroyed, but inscription remains as a material representation or 'knowledge' about that moment of time. In some cases, the actual pot may go into storage, while the inscriptions are studied in post-excavation work, with the only references to the original pot in a database record or GIS system—further inscriptions. Later, when the database numbers, photographs and GIS data of the pot are turned into descriptive text in an academic article, it is yet again inscribed in the new form of a text. The pot has turned from a material artefact into a virtual inscription; it is now a tangible text, but a virtual reality. Such inscriptions underpin the entire notion of what it is 'to do archaeology' and what is 'the archaeological record'. We have come to rely heavily on inscriptions for our everyday discourse and our interpretive practice (Bateman 2006). An inscription can be utilised for an array of different purposes that extend beyond the original material from which it is based. For example, a site plan captures a moment of excavation in time, recording in a more durable representation something that will soon be destroyed. A site plan is also something that is comparatively mobile, unlike, say, the original excavated Neolithic plaster floor that it represents. A plan can be copied and distributed to a far greater number of people—and

thus the value an inscription comes down to its representational and mobile importance.

The use, power and authority of inscriptions often comes down to their relationship to *translation*, an activity that often actively uses inscriptions. Bruno Latour explains that the concept of translation “refers to all the displacements through other actors whose mediation is indispensable for any action to occur...actors modify, displace, and translate their various and contradictory interests” (Latour 1999: 311). The term translation refers to the activity whereby actants (people, things, artefacts, machines, tokens, anything in a network) are changed so that they can work with or against one another, forge alliances and generally circulate. Translation is a process or activity through which executive and epistemic authority is effectively built, changed or undermined by various human and material actors.

In the 2009 field season, I observed two major types of translation at Çatalhöyük. On the one hand, there was the physical circulation and translation of things and people. I highlighted some of these negotiations involving space, structure and access in the previous section in this chapter. Through the translation of physical things—that is, through an individual’s negotiation of their own relationship to things, other people and their understanding of social and physical space—people at Çatalhöyük articulated the world around them, managing their own place within the site’s social orders and hierarchies, and manipulating artefacts and inscriptions to maximise benefit to their own aims and goals.

For example, in order for an archaeologist to gain authority, he would always handle—or translate—an artefact in such a way that it would maximise benefit to his own person. An undergraduate apprenticing student, for example, might simply make sure that he excavates an artefact in the most logical and safest way possible, handling it under the appropriate protocol and with care, then properly inscribing a record of the find in the site database before storing it properly in the storehouse. By translating an artefact in such an ‘appropriate’ way, this student gains authority. Others higher in the site hierarchy might note his skill and competence, raising his epistemic authority in the eyes of his peers, and eventually they may grant him more executive authority to access the site if he shows continued competence with artefacts. Similarly, the student’s trench supervisor would manage the artefact through (and above and beyond) the student, manipulating it in such a way that the record of the artefact was inscribed properly in the site diaries or was appropriately documented in the end-of-season Archive Report, which might be in her charge. Further down the line, a site specialist might physically study the original object, or perhaps just inscriptions (like site plans, photographs,

diagrams, etc.), and then reference the object in a report or journal article. The specialists' aim in this situation would be to ally themselves to the objects and to the inscriptions of those objects in order to create evidentiary support for a larger intellectual interpretive argument about the Neolithic past. By doing this, the specialist would translate the artefact into something bigger and more powerful: an interpretation, part of a larger account of the past, a 'contribution to knowledge'. Similarly, Ian Hodder as director may take the textual accounts of the artefact written by the specialists (he may sometimes also look at the original object, or sometimes only use the indirect inscriptions of that object) and make even larger 'meta' interpretations about the Neolithic past. These meta interpretations would, again, appear as inscriptions in 'final product' books or reports. The translation of the inscriptions from 'nothing' into 'something important' would maximise the authority of all of the materials involved: the original find, the evidentiary inscriptions, and the final product text itself. This act of translation would also place a great deal of weight and trust upon the archaeological methodologies and processes of inscription involved, lending status and authority to all of those individuals who handled the material, created inscriptions, and translated material along the way.

This latter point touches on the second major type of translation that goes on at Çatalhöyük—that of translating the archaeological site profile itself—maximising benefit to the project itself through a high degree of circulation and translation of what I would call 'Çatalhöyük as an Inscription'. What I mean by this regards the fact that Çatalhöyük and Ian Hodder both have a high degree of 'name brand' circulation in academia—specifically in academic arenas that debate how archaeology is theoretically and methodologically practiced at the site. This name recognition regards both James Mellaart's past sensational cultural-historical practice, as well as Ian Hodder's present postprocessual school. Because the site has such a high profile and high degree of circulation,⁵² the site itself has become a label or an inscription that has been utilised and translated by various academics for their own benefit and authority. Archaeologists working at the site gain authority through their exposure and ability to discuss 'what actually happens' with method at Çatalhöyük and can 'expertly' discuss the Neolithic material remains that they are now familiar with. Global archaeologists in the classroom also use the site—because it is high-profile and thus more easily known or

⁵² Oguz Erdur recounts in his PhD: "There are more archaeologists here per square meter than anywhere else in the world, it's been claimed. (Certainly mockery)... Envy and mockery accompany interest and attention... I myself was scoffed at by an elderly archaeologist: 'Oh dear! Why aren't I surprised? Seems like, everybody's going to *Everybody-knows-land* nowadays!'" (Erdur 2008: 557).

recognisable—as leverage in debates for or against postprocessual methods and theory. Such elevated attention around the site seems to have resulted in two things.

First the site, by virtue of a high profile and its insistence on greater methodological transparency, has generated a great number of different *types* of actors. Because Hodder invites anthropologists and general scrutiny, and argues for ‘new methods’ to be implemented in a postprocessual program that is claimed to be ‘more right’ than others, the Çatalhöyük project has attracted people who engage with the site material for different reasons and who inscribe things in different ways. For example, the PhD student Oguz Erdur attended the site in 2006 in order to understand Turkish identity and to write a critical anthropological diary of site activity (Erdur 2006; Erdur 2008), and Carolyn Hamilton attended the site to understand what she called anthropological ‘fault lines’ that ruptured between field excavators and specialists (Hamilton 2000). Meanwhile, in 2009 graduate students Marin Pilloud and Sheena Ketchum attended the site to study Neolithic human remains and clay remains, respectively, and were solely at the site to gain a doctoral degree and accreditation for their work on identifying and interpreting Neolithic material (Ketchum and Doherty 2009; Pilloud 2009). I myself attended the site in the 2009 field season to study the movement of people and things, with my own motivation to observe site structures and authority, and to gain doctoral accreditation for my own work. Other members like Shahina Farid works year round on the project to both manage the elaborate documentation and groups of people, as well as to dig as a field excavator to learn more about the Neolithic past (Baltar 2006: 122-123). Ian Hodder opened the site and continues to attend the project because it represents his practical-theoretical program of postprocessual excavation. This list represents only a fraction of the hundred or so excavators, specialists and members of the public who attend the site each season. This multiplication of people and purposes at the site has resulted in more people in attendance, more people translating the site for their own means and purposes, and the production of more inscriptions.

I found this situation to be somewhat problematic, because a second result of the site’s postprocessual method meant that there was also an explosion of inscriptions at the site, due to this encouragement of multivocal interpretations and instability, an active desire to interact with new mediums and methods. On the one hand, I would argue that the state of having many inscriptions can be positive. Any person wishing to find an inscription of previous material can easily find a host of inscriptions at the site on any one find—diary entries, database entries, textual accounts, photographs, illustrations, displays, etc. They can use a plethora of documents and records to examine

and then translate material according to their own aims and purposes. However, I would also argue that this plethora of ‘stuff’ is problematic. It has also resulted in ‘too much’ data or inscriptions, too many accounts for any one team member or even one whole team to get a handle on, fully digest or comprehend. As archaeologist Cornelius Holtorf has noted, the site has reached a kind of data saturation, where “More effort goes into managing the documentation than the site...People may spend more time watching videos of each other and navigating through huge archives than looking at particular features of the site” (quoted in Chandler 2002). While Hodder might actively encourage the activity of endless inscription because of his idea that “a lack of stability is necessary if a critical approach is to be taken and if the project is to remain responsive to a changing world around it” (Hodder quoted in Farid: 27), I would argue that a kind of entropy ensues.

While Hodder endorses instability within his team and his own site practices (Baltar 2010), he has ironically also argued the opposite point: that ‘having things’—that is, creating objects, artefacts and material things—breeds a kind of chaotic instability (Hodder 2009b). In his H.H. Young lecture at the Association of Social Anthropology in the Commonwealth Conference in Bristol 2009, Hodder referred to instability and things in the Neolithic. He argued that during Neolithic, people began making many things, and that this introduction of material possessions and objects seemed to breed a general clutter, seemingly making life more unstable for the inhabitants of Neolithic Çatalhöyük. I would ask Hodder, what is different from the Neolithic to now? Why would this principle not apply to humans working today, doing archaeology and creating knowledge? Why would having so many things *not* breed chaos today, as he suggests they did in the Neolithic, and why would having more instability and more inscriptions lead to more steady, stable and authoritative accounts of the past—as he seems to suggest in his argument that “a lack of stability is necessary if a critical approach is to be taken” (Hodder quoted in Farid: 27)? It can only be assumed that Hodder thinks that a ‘critical approach’ and ‘instability’ in this context refers to a kind of consensus and stabilization bred through critical peer review. However, by constantly breaking apart any consensus that does stabilize through peer review, by continually forcing instability over and over again, he seems to be undermining his own authority—and the authority of the site of Çatalhöyük as a representative of postprocessual archaeology. This is an argument that I will refer back to in the conclusion of this chapter (Section 4.5).

The question remains: with such instability of ‘too many things’, too many accounts and too many persons, what actually seems to be happening to interpretation at Çatalhöyük? Do more things and more accounts—more things and more entropy—

make authority more accountable? Does the instability of practice actually make the production of knowledge itself a more stable enterprise? The next section deconstructs these questions using specific examples from practice at Çatalhöyük. I argue that, while Çatalhöyük does make good on its word of creating instability and creating multiple pathways to knowledge, it seems to simultaneously blur or collapse the idea of *creating of multiple inscriptions* with the idea of supporting or even engaging with *multivocality*. What is in fact happening at the site is that while multiple inscriptions are being created, only one translation—or more correctly one series or one pathways of translation—is actually being actively used by the archaeological team, as regards an authoritative account of the Neolithic past. In other words, only one authority or authoritative pathway is present in a given ‘final product’. Hodder has not argued against such singularly authoritative pathways (in fact, he has argued very strongly for a kind of one-stanced authority amidst a sea of alternatives:

It does seem possible to argue for a certain authority but be involved in a plural, multivocal debate. It does seem possible to break down boundaries, and move to networks and flows, without losing impact and purpose. (Hodder 2000: 14)

But in the same breath, there seems to be little acknowledgement by Hodder or his team about whose ultimate authority is actually being staked and claimed in any one situation. There has been no acknowledgement of the fact that their plethora of inscriptions are so many and so great that they often get lost in a crowd of ‘too many’. This usually results in the team collapsing back into a more simple or streamlined accounting process, where they limit themselves to only certain inscriptions for ease of access, resulting in something of a ‘standard’ (shall I even say, ‘processual’) scientific production of knowledge. I argue that ultimately at Çatalhöyük, any one person relies on one convenient set of knowledge inscriptions and one pathway or voice when constructing their own personal understanding of the site data. This process of ‘pathway translation’, and a reliance on simple and direct authority, impacts the construction of scientific knowledge at Çatalhöyük.

4.4.3 The Translation, Production and Currency of Representative Things: The Example of the Plastered Skull Burial

An example of such ‘authoritative pathway’ translation in actual practice at Catalhöyük can be seen in the case of the Plastered Skull Burial.⁵³ This case study exemplifies how a wealth of inscriptions (documentations, photographs, reconstructions, textual accounts), based on material remains discovered in excavation in 2004, became an authoritative ‘final product’ account of the past.⁵⁴ The burial was brought to my attention in an interview with Ian Hodder in 2009. In conversation, he mentioned a specific exchange that had occurred between himself and a field excavator earlier that day in the seminar room, which he recognised as being an executive authority issue. According to Hodder, an experienced and competent excavator was examining data from previous field seasons and was unsure about how to interpret a singular instance of archaeological recording. The excavator was checking records from the 2004 season, preparing the material for final interpretation in the next series of major site publications. This field excavator was a highly trained professional but, according to Hodder, she seemed to lack the confidence in her own authority to interpret the past when the record seemed unusual or extraordinary. So she had called in Hodder and Shahina Farid to authorise her interpretation, to provide external confirmation and direction (although Hodder said to me that he thought her opinion and interpretation was equal to his own in this instance). In telling this story, Hodder seemed to be implying that he thought this case was of interest because of the way a number of personality issues—individual personalities, the level of personal security in one’s own interpretive ability, the personal need for validation by greater authorities at the site—could impact authority and the interpretive record. However, I thought this exchange was much more interesting because of the way in which authority and agency affected the translation, interpretation and reception of *inscriptions*, and in the way

⁵³ I was not able to personally witness the actual unearthing of these remains in 2004. However, I chose this example because of the wealth of *already inscribed* archaeological records of this find that existed when I was first introduced to it, as well as ‘final product’ published accounts of it that already existed in books and reports by 2009. I also had access to some of the original team members who excavated, inscribed and initially studied the material when it was unearthed in 2004, and who relayed their accounts of discovery to me in interviews during my fieldwork.

⁵⁴ By ‘final product’, again, I don’t mean to imply that any of the excavators or Ian Hodder ever thought that interpretations of this burial should be understood as an entirely ‘finished’, confident account or a closed book. But it was certainly translated and represented as a polished account in published books/reports in order to represent an authoritative and stabilised narrative.

negotiation of authority directly impacted the production of archaeological knowledge.

In order to continue this line of argument, I later met with the field excavator, who was still poring over the records. She told me that the problematic issue at hand regarded the exact placement of a certain burial—a skeleton holding the plastered skull, which was unearthed in 2004. While re-checking the 2004 Harris Matrix chart records, she had realised that the Harris Matrix and several other records from the 2004 season seemed to suggest that the female skeleton (found clutching a plastered skull, the only plastered skull ever found at Çatalhöyük) was buried first in a midden, and then the foundations of a house were built on top of the burial [see Appendix C for a sample of a Harris Matrix chart]. The excavator knew that this sequence was unprecedented at Çatalhöyük, because burials were usually cut directly into plaster platforms inside of houses that were already built—not cut into middens, or under whole house foundations. A midden cut with this unusual burial of a woman with a plastered skull meant this burial was a unique—or as the team later interpreted it, important—event. The field excavator checking the records wanted to make sure she was “getting the data right” before it became solidified in the record (personal communication 2009). Therefore she called in ‘higher authorities’ like Hodder and Farid to confirm and authorise her interpretation. This incident brought up a number of interesting points about the authority of stabilisation through inscriptions and translation in archaeological practice.

First, the main issue with the records was that details of the event itself were hazy. The burial was uncovered at the end of the 2004 field season, and because of time and financial constraints, the team was on a time crunch and so only the plaster skull was lifted and conserved in its entirety. The whole feature [1517]—which included the skeleton, the plastered skull, and a grave goods cluster with things like a leopard claw—was separated, and the plastered skull went to the museum. The original records, mostly written by field archaeologist Simon McCann, stated that the grave “appears to have instigated the building of platform F1501. Cut into midden deposits from the phase of building below this is a clear example of burial practice determining the construction and architectural erection” (Çatalhöyük Research Project 2004: Feature 1517, online database record). [See Appendix A and B]. When asked to recheck the data in 2009, a few interesting issues arose for the field excavator who was trying to stabilise the official records. First, the firm account of the platform burial as cut into a midden rather than a platform seemed accurate from the original records, primarily the Harris Matrix.

However, Simon's site diary from the next year's (2005) season dig, which finished the excavation of Building 42 (atop the burial), stated that:

"I was struck by the fact that all our original ideas about the burial with the plastered skull F1517, were that it had gone in pre layout of the platforms, F1501+2 and prior to any sort of activity within the house but we hadn't considered whether the house itself was built! So I checked the matrix to make sure I hadn't lost it, found that there was no direct strategraphic relationship between the burial and the eastern wall, they were both the first things to happen. (It is entirely possible that I may have got this wrong but lets just imagine for a while that I know what I'm doing)". [sic] (Çatalhöyük Research Project 2005: Excavation Diary Entry, online database record)

[See Appendix B]

This record shows that the official account of the burial under the foundation rests solely on (1) Simon's memory, which he admits is hazy and only stabilised one year after the event in 2005, and more firmly, (2) the Harris Matrix chart that he recorded in 2004. All of the textual formalisations of Simon's account seems to appear in 2005, a full year after the original recording and excavation that happened at the end of the 2004 field season. For an archaeologist like the field excavator in 2009, who was rechecking the records five years later, this seemed potentially problematic, hence her insistence to me that she "wanted to get the data right".

Interestingly, the initial account of the burial under the platform had already been stabilised in a number of documents that had been published before this field excavator's 'rechecking of data' in 2009. Her checking and questioning the records were only a secondary contestation, purely to settle the official account for the more authoritative site volumes that were to be published in the upcoming seasons. Two of the already published accounts bear special mention. First is an illustration by John Swogger, the site illustrator. As Simon McCann wrote in his 2005 site diary (same as above), the burial under the foundations suggested (for him):

...a public, communal event, possibly laying claim to that of a piece of real estate, or public due the importance of that person (skull, female or both). I mentioned this to John Swogger earlier today and he said that the reconstruction he did of the burial was without walls so perhaps we were thinking along the same lines? (Çatalhöyük Research Project 2005: Excavation Diary Entry, online database record) [See Appendix B]

While Swogger's reconstruction [Figure 11] did not address the foundation issue, it did formalise all of this speculation and fluid archaeological activity into a very striking and stable image of "what the burial looked like" at the time of inhumation. It decontextualised the burial away from houses and any other human activity that might have taken place.



Figure 11: John Swogger's illustrations of the Çatalhöyük plastered skull burial. On the left is the plastered skull, on the right is the skull as found in the full burial context. Illustrations online: http://myweb.tiscali.co.uk/jghsillustration/gallery_1.htm

A second and more notable published account was produced by Ian Hodder. In his book *The Leopard's Tale: Revealing the Mysteries of Çatalhöyük*, he stated firmly that:

The plastered skull was found held in the arms of a woman who had been placed in a pit as part of the foundation of a new building...This building (Building 42) was unusual in that it was built over a midden. The foundation deposit seemed to imply that if one could not erect a building over an ancestral building one could erect one over an ancestor. The way that the plastered skull occurred in a single pit/grave, and the way that it was held by a single individual, contrast strongly with similar rites in the Levant and southeast Turkey...We cannot be sure that the features resembled a specific historical person, although the shape of the nose seems highly distinctive. (Hodder 2006: 148)

These two accounts—especially the latter—are authoritative in their solidarity. They do not belie the underlying issues that the later field excavator seemed to have with the official site record, where the foundation account rested heavily on one inscription of the original burial placement (the Harris Matrix chart, which even the original excavator was relying upon to jog his memory about the original excavation in 2005). Thus, the Harris Matrix chart in this scenario might be called an Obligatory Passage Point. The next section explains what this means, as well as the “moments of translation” where this account of ‘foundation burial’ initially built authority and then finally cemented into an agreed-upon and stabilised authoritative account.

The term Obligatory Passage Point was coined in a study by Michael Callon on the ‘scallop and the fishermen of Brieuc Bay’ (Callon 1986). In this study, Callon cites

four “moments of translation” that can be discerned where actors in his case study “impose themselves and their definition of the situation on others”. These moments are:

(a) problematisation: the researchers sought to become indispensable to other actors in the drama by defining the nature and the problems of the latter and then suggesting that these would be resolved if the actors negotiated the ‘obligatory passage point’ of the researchers’ programme of investigation;

(b) intersement: a series of processes by which the researchers sought to lock the other actors into the roles that had been proposed for them in that programme;

(c) enrolment: a set of strategies in which the researchers sought to define and interrelate the various roles they had allocated to others;

(d) mobilisation: a set of methods used by the researchers to ensure that supposed spokesmen for various relevant collectivities were properly able to represent those collectivities and not betrayed by the latter. (Callon 1986: 196)

While these ‘moments of translation’ are very case-specific to Callon’s study of scientists studying scallops in Briec Bay in France, they offer a useful template for examining the translation of authority and stabilising of accounts in the case of the plastered skull burial at Çatalhöyük.

In the case of the plastered skull burial (to paraphrase Callon’s study), a single question—was the burial placed before the building of house foundations, or did it occur within a normal house plaster platform burial?—was “enough to involve a whole series of actors by establishing their identities and the links between them” (Callon 1986: 205). The various actors—the plastered skull burial, Simon, Hodder, Swogger, Farid, the Harris Matrix, the illustrations—became indispensable to the field excavator, who found herself caught between an original account and a potential contestation of that account. The field excavator also found herself in the uncomfortable position of being a ‘gatekeeper’, a person whose interests of all other actors lay in her admittance of the proposed research interpretation. Instead of embracing her position as an gatekeeper (as Callon seems to argue the scientists in the Briec Bay case were actively doing to further their authority), she sought further allies and confirmation of her position, allying some of the responsibility of the gatekeeper role onto another figure of authority. Hypothetically, as the diagram of this process [Figure 12] shows, “problematization describes a system of alliances, or associations, between entities thereby defining the identity and what they ‘want’” (Callon 1986: 206). In this case, each member of the group or actor has some kind of ‘road block’ or challenge in order to pass this Obligatory Passage Point question, and have a stake in the issue at hand. For the field excavator, her

'road block' and stake was her position of authority to confirm or deny the foundation account of this burial.

In Callon's original French terminology, the next 'moment of translation' comes in *interessment*—that is, the "group of actions by which an entity...attempts to impose and stabilize the identity of the other actors it defines through its problemization" (1986: 207-208). In the Çatalhöyük example, the field excavator attempted to join forces with all of the other actors in order to attain a certain goal: namely, "getting the data right". She enacted a process whereby she sorted through all of the previous records and inscriptions—photographs, site plans, site diaries, as well as consulted with other site authorities like Farid and Hodder—in order to corroborate the Harris Matrix chart and the hazy accounts made by Simon five years earlier. Like Callon's case of Brieuc Bay, "these interessment devices extend and materialize the hypothesis made by the researchers" (1986: 209)—in this case, the inscriptions were utilised to extend/materialize the hypothesis that the burial was cut into a midden and then house foundations were established on top of the burial, which was an unusual site activity. As Callon explains, "The interessement, if successful, confirms (more or less completely) the validity of the problematization and the alliance it implies" (1986: 210). In the case of the plastered skull burial, after negotiating the various records and inscriptions of the material, and after allying her own process with that of other authorities, the field excavator agreed with the validity of the original problematizing question.

The moment of 'enrolment' described by Callon is where "social structures comprising both social and natural entities are shaped and consolidated" (1986: 211), where various actors and materials align in 'roles' that are "defined and attributed to actors who accept them. Interessement achieves enrolment if it is successful. To describe enrolment is thus to describe the group of multilateral negotiations, trials of strength and tricks that accompany the interessements and enable them to succeed" (1986: 211). In the case of the plastered skull burial, when the field excavator negotiated the material and actors, every agent aligned in agreement with the hypothesis that the burial was placed before the foundation. She eventually confirmed the original account that "What can be said about this grave is that it appears to have instigated the building of platform F1501. Cut into midden deposits from the phase of building below this is a clear example of burial practice determining construction and architectural erection" (Çatalhöyük Research Project 2004: Feature 1517, online database record), an act of confirmation that then stabilised and, thus, *authorised* the account into an authoritative version. If she had further contested the hazy authority of this material from her review of the past record—again, it was only founded on the Harris Matrix and Simon's

memory, and the reliability of the latter was contested even by Simon himself in the 2005 site diaries—it might have created some further disruption to the authority of the published accounts and images that had already been produced. In this case, the moment of ‘enrolment’ or alignment of inscriptions might have played out an entirely different story, with some allies perhaps linking to the field excavator’s contestation and others not. For example, if she thought she had found reasonable evidence to suggest Simon’s memory was misguided, then Hodder perhaps might have listened to her contestation and backed her account, allying with her and the new accounts of the material. At that point the Harris Matrix and the previous illustrations and accounts would not be ‘enrolled’ or aligned as allies to the field excavators negotiation/contestation. Instead, the divide might have played out in something of a ‘battle of authority’ between those inscriptions and actors advocating the foundation account, and those actors like the field excavator and Hodder who advocated against it. In such a case, the actors with greater social weight and executive and epistemic authority, like the newly turned Hodder, would likely have weighted the authority on their side, with future publications advocating against the foundation burial, or dropping the account completely from future publications.

The issue of enrolment leads to Callon’s final ‘moment of translation’ called ‘mobilisation’, where he asks: “Who speaks in the name of whom? Who represents whom?...as with the description of intersement and enrolment, only a few rare individuals are involved” (1986: 215); thus, there is a *mobilisation* and authority of allies. Similarly with the plastered skull burial case study, representation is a key component of the second stabilisation of the foundation burial account. Like the scallops of Briec Bay, some actors are silent, while others speak or represent their interests. When the field excavator was poring over the records and negotiating the original account of the foundation burial, the plastered skull burial did not speak for itself, but rather the *inscriptions* and records of it were representative of the original event. Similarly, Simon as a person did not speak directly for himself to the field excavator in 2009, but rather his *inscriptions* or records (in the form of Harris Matrices, site diaries and other excavation records like context sheets and photographs) represented his memory and his account of the problematization. Because the field excavator was a contesting figure in this specific case study, she became the primary mobilising actor, upon whose account (which was expected to materialise through her ‘rechecking the records’ job in the post-excavation assignment in the 2009 study season) rested either the enrolment and intersement of the problematization, or the divergence and contestation of that and the mobilisation of different actors, a process which would then

create an entirely new set of allies that would align with a negation or alteration of the problematization. As Callon explains in his example, “To mobilize, as the word indicates, is to render entities mobile which were not so beforehand. At first, the scallops, fishermen, and specialists were actually all dispersed and not easily accessible. At the end, three researchers at Brest said what these entities are and want” (1986: 217). Similarly at Çatalhöyük, at the onset, the materials of the plastered skull burial—including the various records, the Harris Matrix chart, Simon, Farid, Hodder, as well as the various already-published stable ‘final product’ accounts of the burial—were mobilised by the field excavator and came together in the process of her negotiation with all of the material, and in her final acceptance of it ‘as-said’ by the Harris Matrix chart.

In this specific case study, the field excavator was uncomfortable with her ‘gatekeeper’ or spokesman role, and the authority that it entailed. Thus, she called in Hodder and Farid to help fulfil that role as ‘authorities’, who could, in part or in whole, take over some of the responsibility of verifying the problematization. A similar parallel does not appear in Callon’s account of the scallops of Briec Bay. In Callon’s example, he states that “Three men have become influential and are listened to because they have become the ‘head’ of several populations” (1986: 216). In the case of the plastered skull burial, it seems that the story is more complex than just a case of power or authority in the hands of a person in charge—it comes down to that person’s own negotiation of their position, and in cases like the field excavator and the plastered skull burial, she was not entirely comfortable the gatekeeper authority role she found herself in. In Callon’s example, the actors are entirely active in their attempts to garner and secure allies to gain authority, and in their attempts to gain the most active role as the ‘gatekeeper’ mobilising or representing agent. In the plastered skull example, the field excavator found herself in this role, but she instead mobilised others to validate the problematization and negotiated her own authority. In multiple instances, the field excavator felt that she needed to defer to Simon, “because he was the one who excavated the burial” (field excavator, personal communication 2009). It was Simon’s closeness to the material that lent him authority in the eyes of the field excavator, and it was his memory and account, his act of witnessing as well as his Harris Matrix records, upon which the entire potential contestation would rest. The field excavator seemed to feel that her own authority on this matter was undermined by her secondary relationship to this particular original find; she didn’t have Simon’s first-hand ‘I was there’ power. This is also why she decided to turn to Farid and Hodder, so that she would have ‘authorities’ as allies to step in and confirm or deny her own negotiations and interpretations. It was

this last-stage lack of confidence in interpretive positioning that Ian Hodder was critical of, for he implied that he thought the field excavator was equally qualified to make an interpretive judgement based on the records.

This example presents several lessons. First, along similar lines of argument as Callon in his Briec Bay study, “Translation is a process before it is a result...It also permits an explanation of how a few obtain the right to express and to represent the many silent actors of the social and natural worlds they have mobilized” (1986: 224). By examining translation in the case of the plastered skull burial, it becomes apparent that authority builds and accumulates around specific actors and specific arrangements or negotiations of ideas. In the case of the field excavator, she ended up in a powerful representative spokesperson or ‘gatekeeper’ position, with the authority to either confirm or invalidate Simon’s account of the plastered skull burial under the house foundation. But importantly, what this example demonstrates that goes beyond Callon’s Briec Bay study, is that in archaeology (1) *inscriptions* play an enormously important role in the production and translation of authoritative accounts of the past, and that (2) certain further gatekeeper authorities, like Farid or Hodder, can be drawn into an analysis to be gatekeeper spokespersons or representatives as executive and epistemic ‘authorities’, and their ‘authority’ positions affect the production or stabilisation of ‘final product’ accounts. Regarding the first point, inscriptions are so critically important in archaeology because the discipline is such a destructive process. Exact replication of an ‘archaeological process or experiment’ is never possible in archaeological methods. Because archaeology is such a destructive process, (and what material we don’t destroy, we heavily manipulate to turn into displays), we are often left only with inscriptions and representations of original excavations. This means there must always be something of an Obligatory Passage Point in the production of archaeological knowledge which involves the problematization of using inscriptions to validate accounts of the past. Authority in the discipline today is founded on this process.

Archaeology, as it is practiced now, forces objects to ‘be spoken for’, taking original material and turning it into inscriptions and representations, which are then negotiated by various actors. Most of the material actors involved in archaeology are dead or silent things, and they must be enlivened and enabled through their mobilisation. By uniting and comparing these inscriptions with other objects and inscriptions, this mobilisation can help create a more full and dynamic understanding of the past. The role of authorities is critical in this process of mobilisation, since various spokespersons make assumptions that (a) the past ‘should’ be or ‘wants’ to be spoken for, and that (b) the objects and inscriptions must pass through obligatory passage

points that the spokespersons (or gatekeepers) control. In the example of the plastered skull burial, the archaeologists involved in the knowledge production process make the automatic and immediate assumption that the material remains should 'be spoken for'. In Callon's example of the scallops in Brieuc Bay, the *scallops* "themselves express nothing. However they end up having, like the fishermen, an authentic spokesman" (1986: 215), which are the three researchers involved in the study of scallop development. Callon never questions the authority of whether or not scallops should be spoken for in the first place, nor why the three researchers were able to claim that authority in their roles as gatekeepers/spokespersons sitting at the bottleneck of the initial problematized question at the obligatory passage point. Similarly, at Çatalhöyük in the case of the plastered skull burial, the archaeologists claim an initial role of authority simply by performing the role of spokespersons for the past, and by physically controlling the material and the records upon which the question of the burial is based.

This leads to the next lesson from the plastered skull burial example: *not all spokespersons or actors are equal to others*. Not all actors are equal and committed. In the case of the plastered skull burial at Çatalhöyük, some actors and inscriptions are more active spokes-agents, who have power over more passive material culture or inscriptions. The Harris Matrix chart, for example, is one of the most active and powerful spokes-agents in this case, due to its stable role as the 'most reliable' witness to the event (after Simon admits his memory is hazy in 2005 and he himself relies back on the charts to reference the excavation events); it is an obligatory passage point through which all other actors must pass. Similarly, the field excavator becomes an active agent, because she sits in the key 'gatekeeper' role that decides what account is or is not 'valid'; all material must pass through her approval, and she will stabilise all of the fluid negotiation and contestation into a 'checked' and 'final product' account. Ian Hodder, also, is a very active agent, for he is drawn in by the field excavator to be 'an authority' who confirms or denies the material evidence, and he has motivation to keep the original account intact, since he had previously published such a firm account of it in his highly authoritative book in 2006.

Each of these powerful agents 'translate' the inscriptions, objects and accounts that they are committed to negotiate, and do so in a way that will benefit themselves or their own place in the system. The field excavator wants to make sure that she "gets the data right" and calls for other peer confirmation, because it is in her benefit to not have her authority questioned at a later time. It is theoretically in Hodder's benefit to do the same, because the foundation-burial account has already been formally stabilised in his own 2006 publications. Finally, it is in the benefit of the Harris Matrix inscription to be

confirmed as 'valid', because if its account of the foundation burial was 'invalid', then the matrix could be seen as a poor representation or 'wrong' inscription, opening a huge can of worms about the nature of the Harris Matrix chart as a reliable method, or Simon's ability to properly record excavation features. In all of these cases, these more powerful authorities or agents hold power because they sit in bottleneck, or narrow points of passage where inscriptions are negotiated, where they confirm or restructure accounts. This exemplifies how in many cases, archaeological authority is necessitated, and inherently a matter based upon, the setting of up bottleneck and obligatory passage point moments, where humans mediate for material culture, and inscriptions mediate for humans.

A final lesson from this exploration comes from Callon's question in mobilization, "Who speaks in the name of whom? Who represents whom?" (1986: 214), which is a question of ultimate authority. An initial response in the plastered skull burial case study is that the field excavator is speaking in the name of all of the inscriptions, the original material and the original excavators, as well as for the entire Çatalhöyük team when her validation of the account is published in the next series of official site publications (still forthcoming).

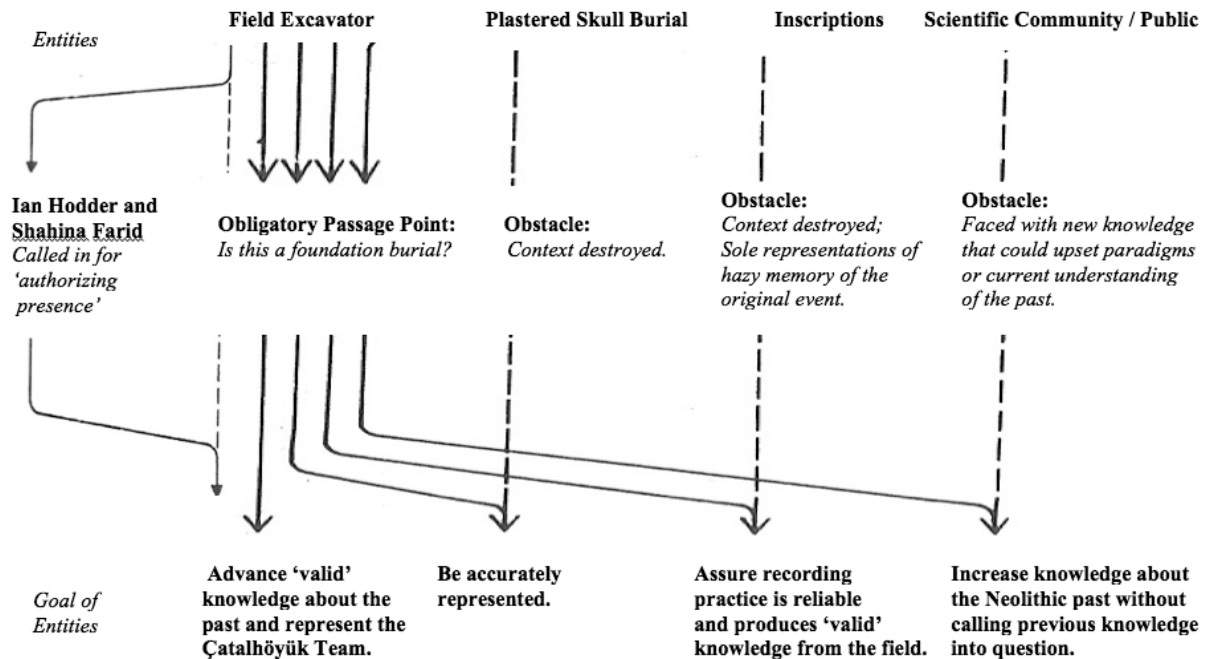


Figure 12: Diagram of the problematization of the post-excavation 'rechecking' process of the Çatalhöyük plastered skull burial. Note the 'gatekeeper' position of the Field Excavator and the Obligatory Passage Point, and note the 'authorising presence' of Ian Hodder and Shahina Farid, who were brought in as 'authorities' to confirm the validity of the foundation burial account. *This diagram is based on Michael Callon's representation of the scallops of Briec Bay (Callon 1986: 207).*

However, an even more specific and relevant answer relates through this question—who represents whom? In actuality, Ian Hodder represents the field excavator, and becomes arguably a more powerful authority and voice in this case, because he is drawn in by the field excavator to supplement, authorise and be an ally for her own spokesperson role. He is also the highest gatekeeper and holder of authority in the whole Çatalhöyük project, the representative of all the other representatives in this team effort, a fact which carries greater implications for how the accounts of the past are produced at the site. It is Hodder's penultimate account of the plastered skull that shows up in a glossy bound volume in 2006; it is his account (along with field director Shahina Farid) that first introduces the plastered skull find in the 2004 Archive Report—both accounts that appear in high-profile public media outlets—and it is this authority that most stabilises the account and lends it the most weight, authorising it as a 'final product' account of the past. What is happening at Çatalhöyük is that, while multiple inscriptions are being created and while multiple actors are engaged and necessary to

produce knowledge, only one translation—or more correctly, one pathway or series of translations through an obligatory passage point, and through more or less powerful gatekeepers—is most active and authorizing any account of the past. While interpretation may “begin at the trowel’s edge”, it can only end after passing through the appropriate processing, being lent the appropriate weight and status by an authorized source and spokesperson.

4.4.4 An Irreconcilable Contradiction? Direction versus Multivocality at Çatalhöyük

The authority of the Çatalhöyük project, as it stands today, rests on one critical tension. The postprocessual program promoted by Ian Hodder is based on the concept of transparency in the intellectual process: transparency of method, transparency of space and structure, transparency of the human and material networks and activities that produce knowledge in the practice of archaeology. However, too much control over that transparency at Çatalhöyük has had an un undermining effect on the overall authority of Hodder’s postprocessual program.

Hodder has argued, “We cannot impose an authority based on an objective science. Rather, we have to argue an authority in terms of a well-informed understanding of the data” (Hodder 2000: 14). During my 2009 fieldwork at Çatalhöyük, this authority of “well-informed understanding” most powerfully manifested in how much time an individual or team spent with the site and the material, and how close they could get to it, which practically affected the authority of persons and accounts. Perhaps even more important—which are often neglected in Hodder’s person-centric approach to archaeological interpretation—are the nonhuman and material actors and networks which create and stabilise authority through their own agency and constraints. In my observation of site activity at Çatalhöyük, these aspects heavily influenced the way material was handled and impacted interpretation. People who had been at a site for a longer duration and who had worked with material for longer amounts of time, or those who had more direct access to or experience of certain things, were assumed to have a more ‘well-informed understanding’ of the Neolithic past, an important leverage of authority at the site. Importantly, there was *stability* to these people, in the way their practices and understandings could collapse into familiar routines or settings. This stability emerged through their constant negotiation and interaction with routines, materials and ontological boundaries. Paradoxically this

stabilisation of space, material, people and authority seemed to bring up a conflict with the postprocessual approach at the site.

Hodder has consistently argued that structural instability should be present in order to maximise creative input and to challenge interpretations—a kind of ‘resistance and accommodation’ that he has theorized would lead to more thorough or accurate renditions of the past. The idea behind this is that, through constant consideration and renegotiation in the wake of unstable methods, some kind of stronger consensus will eventually arise. This at the outset is the argument for peer review—that multiple voices leading to consensus makes for a stronger or ‘more correct’ argument. However, Hodder has paradoxically continued to try to take this setup one step further by arguing that this consensus-forming is no good, and that “a lack of stability is necessary if a critical approach is to be taken and if the project is to remain responsive to a changing world around it” (Hodder quoted in Farid: 27). The paradox in this situation is inevitable—for, after consensus is stabilised through familiarity with material, then *forcing* it to become unstable again undermines the authority that has already accumulated and stabilised.

In terms of structural stability, Hodder has previously argued that “it is impossible to remain simply a service provider or a mediator...One is forced, then, to take a stand” (2000: 11), recognising the need for archaeologists to promote their own stable and unified accounts of the past based on material evidence, while still allowing other voices to create meaning for their own groups on their own terms. However, in the same breath, he argues that:

[T]he notion of ‘the site’ is one of the main building blocks of archaeological knowledge and archaeological authority. Archaeologists talk of ‘my site’; they say ‘come and visit my site’, or ‘what site are you digging at the moment’? There is some notion in these statements of ownership...But at Çatalhöyük we see the site disperse...varied groups, with their different interests and expectations approach the site, they construct different versions of it which are only partly rooted in the finds made at the physical location called Çatalhöyük ...The boundaries around the discipline are eroded, and the enclosed self-sufficiency of the academy is punctured. (2000: 10)

Hodder sees Çatalhöyük as a different kind of site, one that meets the challenge of opening transparency of method to both inside and outside challenge, allowing a contestation of authority and structural instabilities and divisions, in order to create a kind of strength that emerges from more peer review, which will elevate the site’s authority through multiple voices and contestation. Again, the idea is that constant multiplicity and instability will breed a kind of authority and better stability—a paradox. His main point is perhaps, “Rather than being decried as chaotic, this diversity is welcomed since it is preferable to a single perspective and monolithic approach” (2000:

9). Again, this is the argument of peer review, the argument that more agreement creates more accuracy or validity, and that disagreement or contestation makes practice and errors more transparent, which can then be contested and fixed through consensus.

But the question that emerges from this stance is, what happens when you force instability and multiplicity at a site that simultaneously stresses the importance of empirical authority? The postprocessual agenda at Çatalhöyük has been heavily consumed with the theory of multivocality and of reflexivity. So much so that Ian Hodder has pushed strongly for one new paradigm in archaeological thinking, and he has become a 'foremost figure in postprocessual theory' (Renfrew and Bahn 2000: 44), stressing this paradigm of multivocality, multiple voices, a lack of stability that is 'necessary' to archaeological interpretation. Have these ideals in theory have panned out in actual practice? Has practice at Çatalhöyük really encouraged transparency and multiple inputs, or does it really force or comply to just one trajectory, one gatekeeper or authoritative voice, one series of representative spokespersons who hold ultimate authority at the site? According to what I witnessed in terms of space, translation and structure at the site, I would say that transparency at the site is heavily controlled by this authoritative vision or voice—but not the authority of this actual practice.

This vision perhaps is no more obvious than in the most recent changes that have happened at the site, first announced in the summer field season of 2010, one year after my ethnographic study of the site. After speaking with the team leaders, Ian Hodder sent out a team-wide email that stated:

I feel strongly that the project needs new energy - that is new questions, new theoretical perspectives, critiques of what we have come to take for granted, new methods. Perhaps we could have achieved this without personnel change but I do not think that would have assured the new energy, the new windows into Çatalhöyük. (Hodder 2010a)

This commentary followed with the announcement that Hodder had fired all of his team leaders. Hodder's decision affected most of the specialists on the site who headed the various laboratory communities or 'pods', such as human remains, faunal remains, obsidian, ceramics, archaeobotany, and so forth. Field excavators were allowed to keep their positions, although according to one team member who asked to remain anonymous, some of the field excavators were considering not returning to the dig out of loyalty to their friends on the team who had been fired (anonymous team member, personal communication 2010).

According to Hodder's email, and in subsequent press, the reason he decided to fire his team was for purely intellectual, theoretical and interpretive reasons; he was not dissatisfied with the work of his team leaders, but rather, "it was time for a shake-up...It

has been a really remarkable team,” Hodder says, “I have felt over recent years that the project was getting comfortable with itself and so not challenging each other or me or the assumptions that we were all taking for granted” (quoted in Baltar 2010) [See Appendix D]. Hodder’s feeling that the project archaeology was becoming ‘more comfortable’ aligns with what I observed and discussed with various team members during my ethnography of the 2009 field season. Familiarity with the material at Çatalhöyük was stabilising into a more settled understanding of the past, and greater duration of time and familiarity with space and material also stabilised individual interpretations of team members, creating ‘authorities’ at the site (who were mainly returning team leaders and other specialists). However, from what I could gather from speaking with members of the team in 2009, longstanding members of the team might argue that they had earned their expertise and authority to, say, recognise a ‘midden’ from a mere ‘pit’ on the site, and an arbitrary opening or ‘access’ in a building from a ‘street’, through their years of experience. By his actions,⁵⁵ Hodder seems to think that the collapse of open interpretive categories was not, in fact, bred from familiarity or expertise at all, but rather “assumptions” and a “taking for granted” of categories by a team that has become disinterested in his postprocessual challenge to maintain instability in the archaeological method. His move seems to suggest that the only way interpretation can perhaps ‘work better’ is to bring in an entirely new set of people who have “new energy”.

According to the press, this was a sudden and abrupt decision, and “Many team members, some of whom have been working with the project since the mid-1990s, are stunned and confused” (Baltar 2010). One team member reportedly called it “the night of the long knives” (quoted in Baltar 2010). Because “Such a mass dismissal is highly unusual at long-running archaeological excavations” (Baltar 2010), this decision sparked a host of commentary within the public and the archaeological community. After the initial press announcement, online forums flooded the web with commentary like the Twitter comment: “Mass dismissals at Catal Höyük. Hodder wants new blood (himself excluded)” (Larsson 2010). On one news website which announced the initial online press release, people flooded the page with online commentary. Some of the more relevant selections reflect highly emotional opinions about Hodder’s use of executive authority [See Appendix D]:

⁵⁵ Here I stress that this is an assumption bred from the admittedly small amount of interview material currently available in the public domain about his decision to fire his team.

Hmm, maybe the director himself needs to step aside to let new blood in at the very top!

I think this move was brilliantly Machiavellian. Bravo!

...The guy at the top is responsible for leadership, if it isn't working then it is HIS fault, not theirs'. What sort of leadership do you provide? None, it would appear...

Regardless of what one thinks of Hodder's "intellectual courage" or his intentions, this strikes me as exceptionally poor leadership on Hodder's part. If he truly has "felt over recent years" that the team was growing complacent, then it was his job as director to motivate the team to challenge each other, Hodder, and their shared assumptions...I feel truly sorry for the team members whose hard work certainly bolstered Hodder's career.

...Has the religiosity of archaeology got so fervent that you can ignore the real-life impacts of sacking so many people? As obvious as it seems, Hodder imposes his agendas on all specialists so surely getting a proxy Grand Master to fill his own shoes is the more obvious answer? I am sure there are things beneath the surface here beyond theory, but if this is the paradigm dig that he planned, then surely the rest of us are bugged when it comes to recycling our staff every few years...

...Well if Ian himself resigned, and the project took on a new director, then new questions, perspectives and methods would be even more guaranteed, wouldn't they?

[sic] (Baltar 2010)

Most of these comments seem to be highlighting the fact that Hodder seems to be *forcing* new voices into the mixture of his own site structure. If his agenda is to open the site to new interpretation, he is undoubtedly making this happen by controlling which voices are to be present at the site by evicting other voices that he thinks are complacent or overly stable. This creates a conundrum. Some of the other comments on the same press release identify the other side of this coin:

I think it is a brilliant move. The point is to get at the truth of this site not prop up researchers. This work will be left to history. It needs to be exhaustive.

If the situation is as it is represented here, then Hodder is to be praised for his intellectual courage. However, events in recent years on other fields have shown that scientists are not immune to ulterior motives and 'hidden agendas.'

(Baltar 2010)

Commentary like this highlights the underlying question: what is archaeology really about if not encouraging better interpretation? If Hodder really thinks that interpretation is being undermined by complacency at his site, might not a mass eviction be justified? By firing his whole team, Hodder seems to think yes, his duty as an

executive authority is to encourage a more accurate interpretation of the past, and he seems to think that instability is the means to that end. However, when reflecting upon the *actual* reality upon which executive and epistemic authority of archaeological practice is based—which I have deconstructed in the previous sections—the whole situation creates a conundrum for Hodder.

While Hodder seems to be encouraging a *process* involving disorder, entropy and multiple lines of thinking, he is ultimately still aiming for one *product*: a solidified account of knowledge. Under the theory of multivocality, the idea is that by including many voices of challenge, the process of contestation will create better clarity, order and more robust accounts of the past through peer review—a process, I must point out, which fundamentally rests on stabilisation. Multiplicity and peer review should lead to a stronger consensus; they aim to solidify a ‘best interpretation’ of the available data, creating an authoritative vision of the past. The sort of radical multivocality that Hodder is seeking through his actions—forcing instability, firing and shaking up his team—run counter to his primary goal of empirical authority. As I argue in this chapter, empirical authority demands stabilisation. Individuals in the scientific production of knowledge gain authority by engaging in the ‘appropriate’ behaviours, by handling objects in ‘appropriate’ ways, and by following pathways of translation in the interpretive process. They increase their authority through time and familiarity with material that ontologically constrains their interpretations. The problem with Ian Hodder’s paradigm of continuous instability is that he is trying to force instability once again after authority has accumulated through this stabilisation of materials, inscriptions, translations and people. Regardless of his frustration with the process of stabilisation, and regardless of his desire to create new ‘mess’ and instability with a new team, it will inevitably stabilise again if the site is to continue to create authoritative accounts of the past. Thus, by continually trying to force instability after his team has already accumulated and stabilised things, people and interpretations, he is actually undermining his own authority—and undermining the authority of those persons and materials that worked hard at creating empirical authority in the first place.

4.5 Chapter Conclusion

4.5.1 Conclusions on Authority: The Importance of Non-Human Actors and Stability in the Production of Authoritative Knowledge

Science studies scholar John Law has argued that a major end-product of science is authority itself. He writes, “And the purpose of all this? It is to produce statements that carry authority, that tell about the outside world” (Law 2004: 27). This chapter has argued that, in the discipline of archaeology, authority manifests through the processes of stabilisation, inscription, translation and blackboxing. The production of knowledge in archaeology has a purpose: namely, to produce texts or other products like reconstructions or museum displays, which are weighty and authoritative, validating theories or trumping other texts. Authority is an ultimate end-goal of scientific activity, embedded in both the production and the consumption of texts and other scientific end-products. Authority is partially structural, and that structure comes from the negotiations and translations of material and people and ideas through space. Authority, as an outcome of social access and constraint as well as a matter of translation, impacts the way knowledge settles into stable, authoritative and authorised forms.

This chapter took the case study of Çatalhöyük and used Latour’s ‘translation model’ (Latour 1986: 266-269; also see Section 2.2.4 in this dissertation) to show how authority is an accumulated affect from many different actors, interactions and outcomes in a given network. This chapter made three linked contributions. The first is the identification and exposing of many underlying mechanisms through which authority is produced and maintained at an archaeological site, addressing the root causes and concerns of authority in the production of archaeological knowledge. Secondly, this chapter argued for the importance in acknowledging of the full range of actors that are instigated in authority. In most previous studies of archaeological authority, the only actors present in any debate are people. Past discussion over authority at a site like Çatalhöyük has followed human impact on human authority—contesting issues of human access, individual rights over interpretation, and local relations. However, as this chapter demonstrates, authority is a complex process that accumulates from the interactions of both human and nonhuman actors. The ontological world has as much impact, and places as much constraint upon, authoritative interpretation as the humans that interact with it. Social, physical and temporal dimensions of archaeological practice, like the division of space, durations of time and the handling of materials, impact the way authority is accumulated and translated by individuals. At Çatalhöyük specifically, Ian Hodder has long recognised the importance

of authority in the archaeological process, but he has conceptualised a site and a practice where the primary actors are human. Instead, I argue, the most influential actors in the production of knowledge are methods and programs of inscription and translation, which create both the necessary stabilisation for authoritative knowledge as well as 'authorities' who can claim expertise or power in epistemic authority.

On a methodological level, the nature of the 'Finds Assistant' role is a critical example of the importance of nonhuman actors in individual authority. At Çatalhöyük, the identity of a Finds Assistant—who has the rare power and authority to enter any laboratory or excavation site without much attention or question—centres around her role as a specialist who deals with 'finds'. As discussed in Section 4.2.2.3, all newly excavated material at Çatalhöyük is first taken to the Finds Desk, where the Finds Assistant then records all of the data from the artefact bags into the database. Then she takes the material in boxes and redistributes them into all of the appropriate laboratory rooms. Her role is to transfer a physical single context into the database, and then transfer the material on for more detailed study. The Finds Assistant's identity, access, accountability and authority at the site is entirely defined by the material that she interacts with. Her authority to enter all of the laboratories comes from her authority as a 'gatekeeper' of that material. She is watched by others and gains or loses status and authority based on her appropriate translation of this material, and based on her method in turning the original finds into appropriate inscriptions that go into the central dig house database—a technology and inscription that all team members rely upon. It is critical to note that the processual, nonhuman, physical, material, spatial and temporal aspects of her role—as well as her performative interactions in a network of both human and nonhuman things—are all mangled and interlocked in her identity and her authority as an archaeologist and a 'knowledge producer'. Similarly, on an interpretive level, the example above (Section 4.4.3) of the field excavator and her authority in the interpretation of the plastered skull burial shows the importance of material inscriptions on the production of a 'final product' account. In this case, the field excavator was involved in a kind of 'resistance and accommodation' (Pickering 1995) of humans and nonhumans, where the narrowing of interpretive access, the 'voice' of nonhuman actors like inscriptions, and the socio-politics of a site hierarchy played critical roles in the authority of final product accounts. Archaeological authority is necessitated by, and inherently a matter of, bottleneck and obligatory passage point moments of translation, where humans mediate for material culture, and inscriptions mediate for humans.

Finally, this chapter argues that empirical authority demands stabilisation. Using the case of Çatalhöyük, this chapter demonstrated that authority is created and maintained through the stabilisation of interpretations, which are both enabled and constrained by the ontological world. In 2009, the people at Çatalhöyük who held the greatest epistemic authority were those who had spent more time at the site, who had more familiarity and experience with repetitive material. In a scientific project like Çatalhöyük, this stability equated to greater presence and executive authority to access social and physical spaces. Interpretations and accounts were stabilised by the authority of those who had experienced the site first hand for a long duration of time, from repeated ontological interaction with archaeological material that was repetitive in nature, therefore allowing recognisability, and finally, from the negotiation of authority between the various team members who were assessing or interpreting that material. Thus, higher status personalities (team leaders or other experienced returning team members) had authority which resulted a strong presence and greater epistemic power over the production of knowledge.

This reality of stabilisation—and its important role in the authority of knowledge production—still goes unacknowledged by Ian Hodder, as he continues to seek out ways to create instability in his site structure. Hodder's current theoretical model relies on the argument of contestation as a means toward better transparency, on a model of multivocality that leads to consensus through peer review. However, by neglecting the nonhuman actors and methods that lend structure, repetition, familiarity and stability to the knowledge-production process, he is in essence undermining his own interpretive authority by continuing to unravel the very processes that created empirical authority for himself and his team in the first place. The most influential actors in knowledge production are the methods and programs of inscription and translation that create both the stabilities and authorities that he seems to resist. His model of radical multivocality runs at odds with his site's thoroughly scientific and stable methods. Hodder's own empirical authority comes from the way his methods and team are producing recognizable and defensible outcomes, and any empirical defence of his own theoretical and interpretive models must come from that stability. In short, what this case shows us is that authority is an outcome of complex social and physical factors, that nonhuman actors and processes play a critical role in stabilizing and establishing that authority, and that this sense of stability is central to the maintenance of authority over time.

4.5.2 Final Conclusions and Reflections on this Study

Finally, it is important to reflect on the successes and failures of this study of authority at the Çatalhöyük project. This study contributes original and distinct research to a host of previous Çatalhöyük ethnographic studies. But in some ways, it also fails to present a fully coherent analysis of authority at Çatalhöyük—largely due to the interesting nature, history and trajectory of the Çatalhöyük project itself.

First, it is important to address the distinctiveness of this thesis's argument in light of previous studies of the Çatalhöyük project. As discussed in detail earlier in this thesis (see Section 2.3.3, Section 3.2.1.2 and Section 3.3.2.2), the Çatalhöyük project has been something of a magnet for ethnographic and reflexive studies of its archaeological practice. Particularly as discussed in Section 3.2.1.2 and Section 3.3.2.2 of this thesis, previous studies contributed methodological and intellectual worth to my own research design. Notable similar studies include the work of Sharon Webb (2002), whose doctoral dissertation focused on multiple interpretations and museum displays at Çatalhöyük. The anthropological dissertation by Oguz Erdur (2008), whose literary 'site diary' stressed issues of Turkish nationalism and an outsider's perspective of archaeological practice, also offered interesting methodological and intellectual insight. Perhaps the most cited ethnographic study of Çatalhöyük is Carolyn Hamilton's analysis of 'fault lines'—rifts and conflicts between excavators and specialists on site (1996). Ethnographic attention has continued up until the present day. Since my study in 2009, two new studies of note appeared in the 2010 Çatalhöyük Archive Report: a study called *Evaluation of reflexive methods* by Björn Nilsson & Åsa Berggren, which assesses the success or failure of reflexive methods throughout Çatalhöyük's long history, and another study called *Practices of archaeological knowledge production at Çatalhöyük 2010* by Tonia Davidovic, which (like my own research) draws on SSK-oriented methods but focuses specifically on excavation practices at the site (Çatalhöyük Research Project 2010a: 158-159). In the 2010 Archive Report, these two budding studies represent 'things to come' on the Çatalhöyük research agenda, but they also represent merely the 'latest' in a proliferation of site ethnographies.

The fact that there have been so many ethnographies of Çatalhöyük practice in the first place is an important issue to consider. Ian Hodder has openly encouraged reflexive study at the site. Because he is so welcoming, many ethnographic researchers find the invitation and opportunity to study archaeological practice at Çatalhöyük almost irresistible. Because the site has a long history of ethnographic tradition, extending that work seems to be a unique opportunity. However, at the end of my own

work I find that I question: with all of these similar studies, what is really distinct in adding yet another ethnography to the pile? In answering this question, I find that my own study has its successful contributions, along with some noteworthy failures.

A distinct and successful argument in this thesis is that the construction of authority in archaeological practice is an inherently *messy, mangled and material* affair. This thesis demonstrates that authoritative knowledge relies upon the interrelations of deeply embedded, active, messy materiality as well as humans to construct archaeological knowledge. As argued throughout this chapter (climaxing in Section 4.4.3), archaeological authority demands stabilisation, which is amassed and solidified from a very messy and mangled interaction of humans, materials and processes like inscription and translation. This thesis distinctly argues that the ontological world intrudes upon human action and thought in archaeological methodology, and that the construction of authoritative knowledge relies upon the stabilizing material limitations placed upon human interactions and processes over time.

Unlike previous ethnographic studies of Çatalhöyük, this study distinctively and forcefully highlights the importance of *material* actors and processes of interaction in the production of knowledge in archaeology. Previous studies of Çatalhöyük have been far too focused on the agency of human actors, representing archaeological interpretation as a human-centric affair. By drawing on insights from other disciplinary methodologies such as SSK, this study argues that our focus should be reoriented to acknowledge the active processes of inscription and translation in our own practice. Much more thought should be given to the fact that we as humans operate in messy and complicated ways, in a mangled material world, where humans, materials, instruments, institutions and personalities all materially interrelate and interact to produce knowledge. Perhaps most importantly, it should be recognized that, in all of this messy reality, archaeological practice also accumulates the messy and amorphous by-product of 'authority'—a higher or lower status attached to the perceived and performed 'correctness' or power of particular knowledge and ideas (the performance and reception of authority is a topic more closely discussed in the next chapter, Chapter 5). No previous ethnographies of Çatalhöyük have so strongly addressed the importance of material agents in archaeological method, the processes and power of translation and inscription, and the unbridled messiness of the archaeological process that is not only inherent in the construction of knowledge but crucial to the production and sustainability of authority.

Unlike previous studies, this chapter also builds the distinct argument (climaxing in Section 4.4.4) that many previous reflexive studies of the Çatalhöyük

project have confused and conflated different concepts of multivocality. As discussed in Section 2.3.2., the theories of multivocality and reflexivity are central postprocessual themes of the Çatalhöyük excavations. These theories directly engage with the notion of authority, questioning who has the power to speak for and about the past. They ask, “how should we respond to the fact that so many groups want to tell different stories about the site?” (Hodder 2000: 4). They are a critique of taken-for-granted assumptions about what knowledge is and how it is formed (Hodder 2003: 58). With reflexivity, stress is generally placed on the act of self-examination or self-reflection of our own methods. With multivocality, the focus is on “changing practices and contexts so that disadvantaged groups have the opportunity to be heard and responded to. It involves trying to move away from the methods and principles that are attuned to the Western voice. It involves ethics and rights” (Hodder 2008: 196). However, as argued in Section 4.4.4, the authority of the Çatalhöyük project’s use of reflexivity and multivocality now rests on a critical tension. The postprocessual program promoted by Ian Hodder is based on the concept of transparency in the intellectual process: transparency of method, transparency of space and structure, transparency of the human and material networks and activities that produce knowledge in the practice of archaeology. However, too much control over that transparency at Çatalhöyük has tended to undermine the overall authority of Hodder’s postprocessual program.

This thesis departs from previous ethnographic studies of Çatalhöyük by making the distinctive argument that there is no real ‘multivocality’ happening at the site—at least, not in any sense of true commensurability or real ‘power sharing’ (see Section 4.3.2). While Hodder’s postprocessual program of reflexivity has succeeded (in the fact that he and many members of his team have actively stepped back to consider and reflect upon their own impact on the archaeology they produce, which has been a successful exercise), I strongly disagree that there is any program of commensurable ‘multivocality’ at the site. For example, ethnographies by the longtime site member Sonja Atalay have focused on conducting “community-based participatory research in archaeology”, or CBPR. In her 2010 article, Atalay writes that “The CBPR project in Çatalhöyük offers an excellent example of CBPR’s successful application to archaeology” (2010: 421) and that CBPR is about “democratizing knowledge” (2010: 426). Unlike such overly cheery and performative studies, this thesis argues (see Section 4.4.4) that ‘multivocality’ at Çatalhöyük has been a misused and conflated idea which needs to be opened and addressed in a more appropriate way. From what I witnessed on-site, team members at Çatalhöyük are not practicing any kind of real ‘multivocality’ nor are they really ‘democratizing knowledge’. Rather, they are engaging in *non-empowered*

Indigenous archaeology, a term coined by Donna Yates in her doctoral dissertation (2010). According to Yates:

In *non-empowered Indigenous archaeology* the archaeologist retains decision-making power. First, in this model, the archaeologist has approached the project with their own questions that address their own research agenda. The archaeologist makes the choice to contact the Indigenous community, but it is likely that excavation could take place without consultation. Nothing specific forces the archaeologist to look for Indigenous input, and if permission to excavate is denied by an Indigenous group, the archaeologist can choose to ignore the denial. The balance of power is not shifted, as some commentators seem to believe. (Yates 2010: 22)

In her thesis, Yates specifically criticizes sources by both Atalay and Hodder, particularly on the assumption that all local communities naturally *want* or *need* archaeologists to graciously ‘consult’ with them, and in the fact that they have yet to consistently acknowledge that the balance of power in a ‘consultation’ always sides in favour of the archaeologists, with no real democracy in decision-making (Yates 2010). Yates’s model of *non-empowered Indigenous archaeology* contrasts with the alternative model of *empowered Indigenous archaeology*, where local groups can assert significant control over both excavation methodologies and final interpretive outcomes. At Çatalhöyük, one can argue that Turkish stakeholders have perhaps forced archaeologists to interact more with the local community and consider their needs, and that Hodder’s “at the trowel’s edge” commentary has acknowledged the archaeologist-favoured power balance of any consultation; however, despite these departures from Yates’s model, I do think that it is fair criticism to argue that the community archaeology practiced at Catalhoyuk is *non-empowered*, in that it is solely powered by—and the result of—the research self-interests of individual Çatalhöyük team members.

The term ‘multivocality’ simply means including ‘multiple voices’ in archaeological practice, and indeed, this is what many community-based studies at Çatalhöyük are setting out to do (Webb 2002; Hodder 2003; Rountree 2007; Hodder 2008; Atalay 2009; Atalay 2010). But when I visited the project in 2009, I only found a cacophony of ‘multiple voices’ existing in parallel. Rather than finding any truly commensurable multivocality at Çatalhöyük, I instead found that interpretation and method was heavily controlled by one authoritative vision or voice. As detailed in Section 4.3.2, while previous literature has argued that it is “reasonable to abandon abstract objectivity and make trials of resistance commensurable...Talk to people, understand them, persuade if necessary; instead of patronising them by playing expert” (Shanks and Hodder 1995: 20), I instead found a kind of ‘parallel’ multivocality practiced at Çatalhöyük, where multiple voices were being ‘allowed’ or ‘sought out’, but

were not really integrated into final interpretations or methodology. As described in Section 4.3.2, the archaeologists ‘conducted multivocality’ by creating a setting where outsider groups, like the Mother Goddess community, felt respected and had the opportunity to add their voices to a general discussion. Neither side was foolish enough, though, to think that the archaeologists were trying to engage in a dialogue of commensurability or were not ‘playing expert’, or where outsider or alternative ideas would have deep impact on the archaeologists’ final interpretation of the material record.⁵⁶ In the specific setting I observed between the Goddess Community and archaeologists at the site in 2009, the lines were clearly drawn, and the archaeologists asserted their interpretive authority over material things and physical space.

Hodder has previously argued that “Subordinate groups who want to be involved in archaeological interpretation need to be provided with the means and mechanisms for interacting with the archaeological past in different ways” (Hodder 1992: 186). But as I argue in more detail later in Chapter 6 (Section 6.3.1), the very sentence structure of this comment reveals that Hodder and his team are in the authoritative position of *providing* subordinate groups with ‘means and mechanisms’ while subordinate groups are at the receiving and disadvantaged end of this process, dealing with whatever means or mechanisms they are allowed or allotted. The team’s intent to empower members of subordinate groups stems from a real desire to allow greater accessibility and freedom to archaeology, and I do think subordinate groups have felt empowered in some ways through their collaboration with the site. However, it must still be recognised that this empowerment is always controlled by those who are higher in the social hierarchy of archaeological practice. This is a point I revisit in more detail in Chapter 6. For now, I argue that it is time for the project to recognize the important distinction between two alternative uses of the terminology—‘commensurable multivocality’ versus simply ‘respectful or parallel multivocality’—and to address the merits and failures of its own idealism.

This brings me to reflect on some of the related shortcomings of my own research. In some ways my study fails in its aims to present a fully coherent analysis of authority in archaeological practice. I would argue that this has happened in part because of difficulties navigating the unique nature of the Çatalhöyük project itself. One

⁵⁶ Importantly here, I again stress the power of the material and ontological world that intrudes upon human interpretation. Archaeologists trained in scientific methods feel constrained by the ontological stabilisation of evidence, thus ‘multiple voices’ have much less of an impact on archaeologists as they are empirically trying to ‘interpret’ or ‘understand’ data. Archaeologists put great attention and stress on the material world that they study, which constrains and enables their interpretations.

failure of this study has involved the limited time that I had available for fieldwork. Simply because I spent less than a full season at the site, I had only a short time to ‘drop the bucket in the well to draw water’. Any time that a researcher spends only a short period of time doing fieldwork, particularly in the middle of a longer work season in the middle of a much longer multi-year project, the results will be necessarily constrained in scope. This work, then, is merely a sample, instead of a more complete vision of the detailed and intricate processes that contribute to the production of authority at Çatalhöyük. My limited stay at the site has also affected my study in a more subtle way: in some ways, this study fails in its aims to present a fully coherent analysis of authority because the Çatalhöyük project is a particularly complicated, disconnected, tangled, messy, scattered and disintegrated place.

As I argue in Section 4.1.2 of this thesis, the Çatalhöyük project has created an unusually large multiplicity of things and people—site diaries and database images, community forums and websites, experimental houses and virtual reconstructions, visual text and visitor platforms, a general explosion of inscriptions at the site—due to the encouragement of multivocal interpretations, the encouragement of instability in people and practice, and an active desire to constantly interact with new mediums and methods. While this plethora of ‘stuff’ allows any researcher to have a host of records at hand to examine and then translate according to their own aims and purposes, it also creates a sense of chaos at the site. While Hodder has actively encouraged endless inscriptions because of the idea that “a lack of stability is necessary if a critical approach is to be taken and if the project is to remain responsive to a changing world around it” (Hodder quoted in Farid: 27), I argue that a kind of entropy ensues. Two things result.

First, the large number of people who have access to the Çatalhöyük project, who speak for the project and the activities taking place, means that more ‘buzz’ or sense of worth and value has been generated around the project, compared to discussion around many other similarly sized excavations. Much of Çatalhöyük’s authority and the prestige of Hodder’s postprocessual program relies on a continuous discussion in academic literature and introductory textbook materials.⁵⁷ Ironically, this *stabilising* effect of continuous discussion that grounds Çatalhöyük’s academic authority, even though so

⁵⁷ I would also argue that, paradoxically, the overwhelming and slippery nature of ‘too many’ inscriptions might be the reason there has been much less ‘buzz’ and academic discussion about the project over time. The project arguably reached its peak of academic discussion in the late 1990s. Perhaps the decline of interest in the academic community is due to the ‘too many’ inscriptions and voices at the site, as it becomes more and more difficult to really get a sense of what is actually happening on site or who is actually contributing at any given time, and this generates confusion over how this model of chaotic method might be useful or helpful when extending this model to other excavating practices.

much of that discussion is about the project's purported instability. By having so many researchers attend the site, continuously conducting new research and speaking for the project, and by continually having the name of Çatalhöyük repeated and cemented in 'authoritative' introductory texts and classroom teaching, the project and its many central personalities become more and more concrete and stabilised in academic canon, thus creating and sustaining a sense of authority. It is important to note that this process itself is a messy, mangled and complicated affair, involving interwoven people, places, things, personalities, loyalties, texts, time and materials. I argue that the project's strongest moments of authority come when all of the mess and mangle stabilizes in just one authoritative voice—usually Hodder's—which rises out of the chaos, solidifying the inscriptions and messy method in one formal book or report narrative. Today, a new reader or visitor to the site is first confronted with an overwhelming instability of people and great confusion over 'too many' inscriptions. In the heat of this confusion and entropy, the reader then stumbles across the solid formal introductions in reports and the hardbound books published by Hodder or the core team, and these come as a cool relief. There is a strong sense of authority when one stable voice rises up out of the chaos, appearing to understand it all.

For my own research, I found that this constant practical chaos, this constant instability of new people and things tumbling in and out of the site while offering 'new ideas', has also created a strong *performance* of what postprocessual archaeology should 'look like' in the field. As a visiting researcher only on-site for a short period of time, I perceived a sense of showmanship at the site. This performance manifested most strongly when I observed outside archaeology groups visiting on what I called a 'pilgrimage' to see 'postprocessual archaeology in action' (see Section 4.2.2.3). These archaeology groups, often heavy with students, would come to see the Çatalhöyük excavations and laboratories; they would ask questions to the site directors with a hope to 'contribute', then depart without having much impact on the site specialists, who mostly just wanted to get on with their work in quiet laboratories.

This sense of performance further manifested as I went through my five weeks as a 'site ethnographer'. When I first arrived on site, I felt a sense of unease when I was immediately labelled as 'another ethnographer' and my work was, at times, quietly resisted by archaeologists who were tired of being watched and studied (although it must be said that my questions were never dismissed or rejected, and people warmed to me the longer I stayed at the site). For example, many of the returning members expressed a slight sense of exasperation and humour when they first met me: 'yet another ethnographer showing up for duty'. During my time there, I got the sense that

many of the Çatalhöyük team members were simply a ‘performing for the anthropologist’. In my field notes on Day Two, I wrote:

Really interesting conversation I just had [with] one of the human remains specialists who is sharing a room with me. She mentioned jokes that went around about what exactly I’m doing – and asked directly, almost bluntly, what *exactly* it is that I would be doing here?: would I would be walking around the site with a notepad and clipboard looking at everyone as if they were monkeys? She said that she and a few others were in the showers this morning, and then suddenly there was no water. They joked that maybe they should all go out in the veranda with buckets of water and splash it on themselves—then the ethnographer could come and watch the primitives ‘doing their thing’.

On the same day, my field notes relate a separate conversation with another disillusioned specialist who told me that the team specialists really just felt like “middle management” working away on archaeological details day in and day out, while “higher powers” watched and made commentary. The specialist said that many members of the team often “just felt like amoeba in Hodder’s Petri dish”, since the director continued to disrupt the site by “inviting controversy for the sake of his next paper”. This sense of disillusionment, which was rife at the site when I visited in 2009, no doubt contributed to Hodder’s decision to fire most of his team to bring in “new energy” (see Section 4.4.4).

This reality means that my own study has certainly failed in some ways to fully pin down the complex mangle of authority at the site of Çatalhöyük. I argue that this archaeological project is perhaps *overly* scrutinized: it is *too* studied, *too* observed. It produces too many voices, which are never fully integrated, because there is too much instability and too many inscriptions to manage. As I argue in Section 4.4.2, because of this confusion and instability, the team usually collapses back into a more simple and streamlined accounting process as they interpret data, where any one person necessarily relies on only one convenient set of inscriptions or one set of voices when constructing their own understanding the site data. Out of all of the chaos and ‘too many things’, emerges just one authoritative voice for simplicity’s sake—and it is this relief in the stability of one strong authoritative voice lifted above chaos that the true authority of the project lies.

Thus, in the same continued vein, my research has simply contributed yet another inscription to the mess and tangle of the Çatalhöyük project. I, too, have been forced to rely on only one set of inscriptions or limited series of events to stake my own arguments. In this way, my own study is undermined because of the limitations in trying to get a grasp on ‘too much’ data. The site is so studied, so scrutinized, so inscribed that any comprehensive account of the project’s authority through time and space would be a mammoth undertaking, requiring an enormous amount of time and familiarity with

the site—which perhaps goes above and beyond anything a new researcher might be able to perform. Perhaps only a longstanding, stable and returning member of the site, like Shahina Farid, could offer something remotely close to a comprehensive discussion on the construction of authority at Çatalhöyük.

Finally, I would argue that my research is ultimately undermined because it is only a study of a performance. People and things at Çatalhöyük operate in a complex web of practice that—to any new researcher—is merely a performance of an idea of what methods and spatial setup ‘should be like’ at the site, and not what is actually happening. I do think that this chapter has in some small way scratched the performative surface of Çatalhöyük and begun a discussion on the project’s authority, but I also think that its results might be compromised by the fact that I have been studying people who are overly aware of my observing eyes, overly trained to ‘deal with’ being observed, and who have simply performed ‘postprocessual archaeology in action’. Because of these difficulties undertaking research at Çatalhöyük, this chapter does in some ways fail to present a fully comprehensive analysis of authority. However, it contributes a solid illustration of just how truly messy and mangled archaeological practice can be, and it strongly argues that authority in the entire discipline rests on the stabilizing of *material* performances and interactions of things and people. The next chapter, which focuses on the case to the Bosnian Pyramids, extends this discussion of performance and participation in the construction of authority in archaeological practice.

CHAPTER FIVE:

Authority in Politics and Performance: The Bosnian Pyramids as a Case Study

“In arguments for hypotheses, as against textbook expositions of findings, the best scientists sound like honest, intelligent lawyers and like principled, mutually respectful people engaged in political controversy.” (Miller 1987: 155)

“The conceptualisation and representation of the past is fraught with difficulty, not simply because of the paucity of the data, but because the construction of history, written or oral, past or present, is a political act.” (Whitelam 1996: 11)

5.1 Introduction

5.1.1 Introduction: Authority from Context, Institutions and Socio-Politics

Using the case of Çatalhöyük in the previous section, I explained how authority manifests through the processes of inscription, translation and the stabilisation of fluid, complex scientific practices. Authoritative things, people and accounts in such a case are first negotiated in localised arenas, in the translations of people working with the physical world and under social institutions of scientific practice; however, authority in the production of archaeological knowledge is yet more complex. In some cases, individuals or collectives are often drawn to charismatic leaders and social movements in the hope to attain some measure of authority or benefit from authority. People in search of or ‘in possession of’ authority can turn into powerful consumers and producers of authoritative goods. Authority can also be mimicked and performed, and people often make deliberate choices in how to perform, seek out, or undermine authoritative people, things or knowledge.

The latter points bring up the specific question that drives this chapter: what is happening in a case like the Bosnian Pyramids? In Bosnia, a group of people (and in particular, one individual) has successfully promoted an image of archaeological authority, even though their interpretations of excavated material have no ontological significance. The amateur Bosnian Pyramid project has held a dominant or ‘authoritative’ position in popular culture, over more ‘justified’ accounts of the past promoted by professional archaeologists. Archaeological authority, then, fundamentally

rests on external social contexts which affect the reception of accounts with the general public. The issue of authority in archaeological practice goes beyond just how actors might translate material and 'gatekeep' power in localised arenas of practice. This chapter argues that in cases like the Bosnian Pyramids, archaeological authority is drawn from *performative* and *participatory* acts that are contextual in nature. Socio-politics plays a crucial role in the way authority can be created and translated by archaeologists, as well as by amateurs and members of the general public, and in the way accounts are successful at garnering authority in public arenas. This study demonstrates that, by drawing on institutions of social authority and science as a master discourse, epistemic and executive authority can be constructed and maintained on the basis of performance and participation.

The first section of this chapter introduces the idea of authority behind the act of classification, the power in dividing what is authentic, authorised and authoritative from what is not in a scientific discipline like archaeology. The second section uses the case of the Bosnian Pyramids to illustrate the role of socio-politics and institutions in the translation of authority, and it argues that politics have a major impact in the construction and maintenance of archaeological authority, especially relating to the general public. The third section argues that scientific authority is, in large part, due to appropriate performance, and the success or failure of authority can come down to how one draws on the appropriate scientific acts, institutions of legitimisation and the idea of science as a master discourse. This last point, regarding science as a master discourse, is fully expanded in the final section of this chapter, using the specific case study of the radiocarbon results presented at the 1st International Scientific Conference of the Bosnian Pyramids.

5.1.2 Case Study Parameters: Relevant Project Background

In 2005, a Bosnian-American businessman and alternative historian named Semir Osmanagić made international news headlines when he announced that he had discovered the largest and oldest man-made pyramids in the world. These ancient pyramids, he claimed, are located in the small town of Visoko, Bosnia-Herzegovina, 20 miles northwest of Sarajevo [Figures 13, 14]. Osmanagić has identified five pyramidal-shaped hills located in the Visočica river valley, which he has claimed are technological

feats of a Paleolithic⁵⁸ Bosnian supercivilisation (BosnianPyramids.org 2006; ICBP 2008). The largest of the purported pyramids, Visočica Hill, is 185.5 metres high. If genuinely man-made, this would make it the largest pyramid in the world, as Khufu's pyramid in Egypt is only 146.5 metres. Osmanagić renamed all of the pyramidal hills in the valley with titles like 'Pyramid of the Sun' and 'Pyramid of the Moon', because they supposedly resemble the Maya step pyramids in Mexico. According to Osmanagić, the three largest pyramids purportedly form a perfect triangle, and the four sides of the largest 'Pyramid of the Sun' align to the four cardinal points of the Earth's compass. His hypothesis also claims that these pyramids are connected by an intricate underground tunnel network, and the walls are adorned with the world's earliest writing and letters that resemble ancient Nordic runes. Osmanagić has associated two other sites with Visoko: a hypothetical 'rock quarry' site in the village of Gornja Vratnica, and a river ravine near Zenica filled with ancient 'mysterious stone balls' (Osmanagic 2007c; Osmanagic 2007a).

These sensational claims are a bit of a two-headed Janus: on the one hand, Osmanagić and his team stress that their project is scientific, based in ontological reality and physical truth. On the other hand, Osmanagić and his team consistently connect the project to new age mysticism, fringe beliefs, alternative archaeologies and esoterica. For example, the project releases "Scientific Reports" as well as media coverage of the project as a genuine scientific archaeological enterprise (Osmanagic 2007b; ICBP 2008; Pazdur 2008) [See Appendix H], while simultaneously presenting itself as a site with 'mystical' and 'mysterious properties with connections to energy beams, cosmic forces and geological anomalies (Coppens 2008a; Coppens 2009). Despite the fringe associations, the scientific and physical reality of the Bosnian Pyramids is by far the most prevalent narrative pushed by Semir Osmanagić and his team, and it is arguably the 'scientific' and 'empirical' account of the site that holds sway and authority in the eyes of the general Bosnian public.

Semir Osmanagić is originally from Sarajevo. He holds a Masters degree in politics and economics, and in 2009, he defended a PhD from the University of Sarajevo on unconventional fringe theories about the Maya (Osmanagic 2007b; Osmanagic 2009). Osmanagić settled in Houston, Texas before the Yugoslav Civil War (1992-1995) and now owns a successful metal construction business that oversees 100 employees—an

⁵⁸ The exact dates for these 'pyramids' have varied over time by pyramid supporters, with little consistency. In some cases Osmanagić also refers to the pyramids as having a Neolithic date or as being built by the Illyrian civilisation around 12,000BC (Coppens 2009); however, in this thesis I use the term 'Palaeolithic' to reflect the radiocarbon dates of around 34,000 BC that have been heavily promoted by Semir Osmanagić and the Bosnian Pyramid Foundation (see Section 5.5.2).

accomplishment reflecting his considerable skill as an entrepreneur. Regarding his archaeological background, Osmanagić claims to have studied pyramids around the world in his free time over the past 20 years and is the author of several works of ‘fringe’ archaeology (Foer 2007) [See Appendix E]. His book *The World of the Maya*, for example, suggests that the Maya were descended from aliens from the Pleiades, “inherited knowledge from their ancestors at Atlantis and Lemuria (Mu)”, and that “pyramids erected on these energy potent locations enabled the Maya to be closer to the heavens and to other levels of consciousness” (Osmanagic 2005c; Osmanagic 2005b: 70). Most of Osmanagić’s alternative history works espouse the same genre of ‘fringe’ ideas (Osmanagic 2005a).



Figure 13: Map showing the location of Visoko in Bosnia-Herzegovina. Map by Tera Pruitt.



Figure 14: This iconic image of Visoko was taken in 1973, and it is widely distributed online, in pyramid brochures for tourism and ‘scientific studies’, and on tourist postcards and other souvenirs. This is the most stunning, straight-lined side of Visočica Hill (renamed Pyramid of the Sun). Incidentally, this is also the most photographed angle of Visočica Hill. This is a freely distributed image.

Osmanagić’s pyramid theories quickly gained local and national attention and support, including support from the international alternative history community (Foer 2007; Coppens 2009). Most professional archaeologists, however, have since agreed that Osmanagić’s theories are not supported by any evidentiary material found at the site, despite Osmanagić’s claims to the contrary (Bohannon 2006a; Rose 2006b). Most mainstream archaeologists define the site as ‘pseudoarchaeology’, an act of amateur archaeological practice that “invokes the aura of scholarship without being scholarly in fact and blurs the distinction between real scholarship and ‘alternative’ output” (Jordan 2001: 288-289). In spite of the negative professional academic reaction, Osmanagić’s project has continued to operate and thrive through to the year 2010, with continued backing from the Bosnian public, media and government (Pruitt 2007; Woodard 2007a).

Use of mass media has been the single most important reason that information and support for the pyramid project spread so rapidly. Print news first released and distributed Semir Osmanagić’s story, and television and Internet media fanned the debate between supporters and opposition. Debra Spitulnik writes: “Mass media...are at once artifacts, experiences, practices, and processes. They are economically and politically driven, linked to the developments in science and technology, and like most

domains of human life, their existence is inextricably bound up with the use of language” (Spitulnik 1993: 293). Interactions that Osmanagić’s team, the general public, politicians, academics and other groups have had with the media have created a complex web of performance, contribution, theatricality and distribution.

Mark Rose of the Archaeological Institute of America writes of the initial press interest: “The story has swept the media, from the Associated Press and the BBC, from papers and websites in the U.S. to those in India and Australia” (2006b). Most of these initial reports demonstrated support for the project. According to Rose: “Every major media outlet that initially covered this story got it wrong. It’s clearly crackpot stuff, but apparently nobody bothered to check the story” (quoted in Woodard 2007b). Eventually bigger news outlets started checking the story and released more sceptical reports; however, local newspapers, “don’t have science desks...Bosnian archaeologists dismiss the majority of local journalists as ill-educated. Hence April’s *Avaz* headlines like “The pyramid will be visible by the end of the year” (Kampschorr 2006: 27).

Television media was the most influential in spreading supportive information to a wide audience (Osmanagic 2007c). Woodard reports, “Federation television, the largest Sarajevo-based network, provided extensive coverage, and soon thousands of people were visiting Visoko every day” (2007b). Local media stations also arranged for ‘face-offs’ between Osmanagić and mainstream archaeologists and distributed many supportive campaigns for his site (Osmanagic 2006). Foreign television networks like ABC advertised excited programs that would “travel to Bosnia to follow this modern day Indiana Jones” (ABC 2006). Osmanagić was quick to use his new clout with the press, travelling around the world—to places like Easter Island, Peru, England, and Jordan—with Bosnian TV to create documentaries that boosted his site’s profile (Osmanagic 2007c) [See interview transcript in Appendix E]. In the meantime, other private groups released professional documentaries about the Bosnian Pyramids (BBR 2007). Local newspapers relished the attention from foreign press, exaggerating foreign interest: “all local television news shows trumpeted the presence of CNN, AP, Reuters, and the BBC—without mentioning that most outlets covered it as a cute human interest story” (Woodard 2007b). With international media attention fuelling the local media, excitement and positive press spread the story like fire. Almost overnight, Osmanagić became the mastermind and poster boy of a national sensation. [Figure 15]

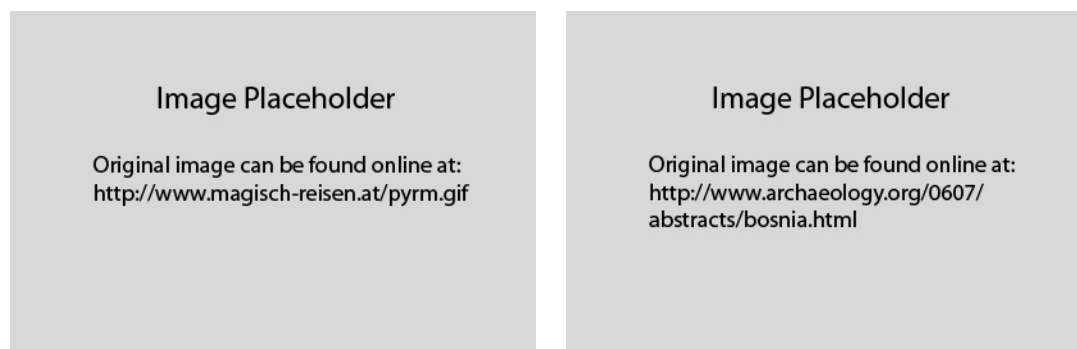


Figure 15: Osmanagić courts both the local (left) and foreign (right) Television Press.

(Image courtesy of Gabriele Lukacs:
<http://www.magisch-reisen.at/pyrm.gif>)

(Image courtesy of Beth Kampschorr:
<http://www.archaeology.org/0607/abstracts/bosnia.html>)

Mark Rose writes, “one might have thought that the Ice Age Bosnian pyramid story would collapse like a bad soufflé, but no. Mainstream media has become somewhat more critical of stories emanating from Visoko, but much of the real work in dissecting the claims has appeared on blogs and message boards, such as *The Hall of Ma’at*” (Rose 2006a). While the Bosnian Pyramid project gathered force and popularity through print and television formats, Osmanagić’s bad archaeology was exposed mostly in online formats. The Internet has become the biggest media for those who oppose the pyramid project, undoubtedly because of its interactive and dynamic format. Anti-pyramid websites come in three types: independent websites devoted to anti-pyramid sentiments, blog postings and commentary on personal websites, and forum commentary attached to previously established websites (Feagans 2007; Reece 2007; Irna 2010). Websites like *In the Hall of Ma’at* operate a general list of articles and forum discussions that dispute alternative history stories for the general public. *Ma’at*’s developer, Katherine Reece, says she built the site to “help those people who were searching for the truth about history to have an easily accessible ‘mainstream’ counter to these ‘alternative’ claims” (2006:103). Her forum has featured heated and emotional debate about pseudoscience at the Bosnian Pyramids site. Other websites and blogs like IRNA (Irna 2010) continue to release frequent bouts of news, information and evidence-based arguments against the pyramid project.

In 2006, Osmanagić established an officially registered ‘Archaeological Park: Bosnian Pyramid of the Sun Foundation’ (referred to this dissertation as simply ‘the Foundation’), establishing a fully-fledged business and administration centre. His team of 35 to 80 individuals, depending on the season, is mostly composed of amateurs with an interest in history, but also includes PhD holders from countries such as Egypt and Russia (Osmanagic 2007c; ICBP 2008). The Foundation has maintained that its ultimate

goal is to establish Visoko as a major tourist attraction and get the pyramids listed as a UNESCO world heritage site (Kosmo 2009). The team runs fully invasive and extensive excavations in Gornja Vratnica, and at Visočica and Plješevica Hills in Visoko ('Pyramid of the Sun' and 'Pyramid of the Moon', respectively). International professional archaeologists have particularly criticized the Foundation's haphazard and destructive excavation methods. Osmanagić's amateur team has damaged genuine medieval and iron age archaeological remains in the Visoko hills in their search of 'proof' of ancient pyramids (Rose 2006b). Supporters and opponents alike have compared Osmanagić to Heinrich Schliemann: his supporters praise Osmanagić's determination for pursuing his vision despite objections from the established academe (in reference to Schliemann's background as a passionate amateur). His opponents reference Schliemann's penchant for destroying all archaeological evidence—from medieval to Roman—that stood between him and his sought-after Trojan stratigraphy. [Figure 16]

Osmanagić and his Foundation publish voraciously: everything from scientific reports aimed at a general public audience to tourist brochures aiming to boost business in the region. Osmanagić has lectured at Bosnian Embassies throughout the world (Osmanagic 2007a), has hosted his own sizeable international scientific conference (ICBP 2008) and has made frequent appearances in local schools and on television (ABC 2006). The pyramid phenomenon in Bosnia was initially seen as an overwhelming success, bringing in important positive economic changes to the post-war town of Visoko (Foer 2007; Woodard 2007a; Woodard 2007b). Much of the enthusiasm behind the project has involved the money it brings to the region through tourism. Bosnia experienced a great deal of suffering in the recent war (1991-1995), which divided the country ethnically and politically, leaving its citizens very insecure and its government politically disjointed: "Fears, hatreds, memories, grief for the dead, nostalgia for the lost native places and homes, shattered dreams, insecurity, disappointment, pessimism are continuing to haunt everybody" (Zhelyazkova 2004: 17). In this context, the pyramid project has provided a positive, unifying symbol for post-war Bosnian nationalism, holding significant authority in the region because of its useful role in a national and ethnic dialogue (Pruitt 2007).



Figure 16: Excavation site at Plješevica Hill (renamed Pyramid of the Moon). Photo by Tera Pruitt.

The questions that emerge from this situation are difficult. Who has the right to Bosnia's past? Who has the authority to use Bosnia's past? This project is undoubtedly helping Bosnia's economy. On the other hand, it is undoubtedly disrupting, and perhaps destroying, genuine archaeology in Bosnia. This scenario forces us to ask distressing questions: might an imagined site like the Bosnian pyramids be *worth* more than real archaeology? Who has the right to put a value on it? Who has the authority to own or excavate archaeological space, or to construct narratives based in archaeology (or at least in the notion of what 'archaeology' might entail)? This site is an economic and social asset to different groups in Bosnia, with different values for different reasons. For many politicians and members of the public, the question is not whether or not the pyramids are real, but rather if people will come to see it, spend money in the tourist shops, and use it as a cultural and economic artefact. For others the site's very existence questions fundamental ideas about government, personal control and academic authority.

This case study also raises important questions about the power of representation and performance, and the appropriate 'presence' and 'presentation' of archaeological accounts. The performative aspects of this case, coupled with the participatory involvement by members of the public, offers an interesting contrast to the idea that 'facts' and 'validity' are objective concepts that might exist outside of a social

context, which involves politics of ‘convincing’. Instead, we are forced to involve complex arenas of authority such as performance and display in order to explain why the Bosnian Pyramid account of the past has been so successful and accepted. While my previous work on this case study for my MPhil dissertation (Pruitt 2007) focused on the socio-political heritage concerns that this project has raised, this thesis is primarily concerned with the questions raised by the site’s construction and maintenance of epistemic and executive authority in Bosnia.

5.2 Authority Behind Categories and Alterity

5.2.1 The Authority behind Classification and Boundaries: Archaeology as a Knowledge-Producing Culture

Archaeology derives its social identity from the way specific people, things and actions are classified as being *archaeological*. As a discipline, archaeology gains authority from its classification as a knowledge-producing culture; people and things within the discipline hold authority from their status within this category. As Bowker and Star write, “to classify is human...a classification is a spatial, temporal, or spatio-temporal segmentation of the world” (1999: 1-11). As humans, we categorise the world, often tacitly, by sorting activities and materials into classification. By doing so, we create social and moral order out of the world we experience. Categories are defined, created and sustained by their social reproducibility. The identity of archaeology as a descriptive category—and a discipline—is maintained, upheld and recreated moment by moment by the social re-enactment of its method and meaning. Archaeology is identifiable as a subject by the acts that society deems are archaeological, by the spaces and the materials that are deemed to be archaeological, and by the tangible products of the system that are deemed by general social consensus to be appropriately archaeological. The identity of archaeology as a category can change or evolve, but only through legitimate means, and only through consensus by the majority of people who accept changes to archaeology as an appropriate category. If there is no social consensus on a category, or if the legitimate means are contested, power struggles may arise—as exemplified in the case of the Bosnian Pyramids. Such a case raises questions about the very nature of categories, consensus, and who has the authority to pick and choose what

is or is not 'appropriate', which can lead to debates about who has the executive authority to access or alter physical space or ground.

The act of identifying and classifying archaeology is important: the very act of creating a classification or naming things or people within the category is inherently a 'diving practice'. 'Dividing practices', conceptually popularised by Foucault (c.f. Foucault 1965; Foucault 1979; Foucault 1982; Rabinow 1984: 8-11), involve the construction of inclusive lists of things and actions which orient, structure and define what it is to be or to do something—say, 'to be' an archaeologist is a definitive category that necessarily excludes everything that is 'other'—say, what it is 'to not be' an archaeologist, or to be a pseudoarchaeologist, for example. As Foucault has argued, 'dividing practices' have an essential power/knowledge relationship. The act of classifying sets up categories of inclusion/exclusion, creating relationships of asymmetric power. The very nature of dividing objects and acts as appropriately or inappropriately under a classification creates an immediate imbalance of authority: on the one hand, what is classified as archaeological has the power of definition, and on the other hand, all the excluded activities of the rest of the world have the powerless state (relative to the category) of being simply 'other'.⁵⁹ Thus, there is a great deal of power vested both in *the state of being* classified and in *who has the power* to name or choose the categories.

A case like the Bosnian Pyramids is innately tied to the authority of categories. Scholars like Reba N. Soffer have argued that, "in the long run, the success of a discipline is not determined by its powers of protection or patronage", but rather "successful professions have maintained a monopoly over a special body of knowledge and skills...of a real benefit to the public" (1982: 801). When an 'alternative' case of archaeological practice like the Bosnian Pyramids clashes with 'professional' practice, it can provoke hostile reactions from those who see themselves as protecting the boundaries and reproducibility of the discipline. That is especially so when an alternative case, though it may lack fidelity to the truth, is nevertheless arguably "of a real benefit to the public". A site like the Bosnian Pyramids challenges the social authority that lies behind the boundaries, control, influence and territory of the discipline of archaeology.

⁵⁹ This is not to say that all the rest of the 'other', non-archaeology things have no power under other names, only that in the immediate instance of classification and naming, they have less power than the things identified in the named category.

5.2.2 Challenging Categories: Professional Authority and Alternative Archaeological Claims

⁶⁰Competing ‘alternative’ archaeological claims—claims that sit outside of the generally recognised category of ‘archaeology’—have existed since the beginning of archaeology’s professional development (Feder 2002). Many of these claims, however, have been neglected by mainstream archaeology as insignificant side issues, only noteworthy as examples of bad archaeology or laughable enterprises. This neglect has been critically challenged in the last few years. Archaeologists have begun to see the value, and perhaps necessity, of studying alternative claims to the past. Influences from Marxism to postmodernism, indigenous rights and values, and heritage institutional accountability to public funding have led the field to be aware of pluralistic interpretations about the past and forced archaeologists to recognize the historical contingency of their own profession (Trigger 1989; Skeates 2000; Merriman 2004). The study of ‘alternatives’ has most thoroughly developed regarding indigenous values and notions of the sacred (Goldstein and Kintigh 1990; Downer 1997; Wallis 2003). However, many archaeologists feel that other alternative archaeologies—such as nationalistic manipulations of history, imagined reconstructions, or pseudoscience—are also relevant to mainstream archaeology. According to these arguments, alternative claims challenge the authority and the very fundamentals of learned archaeological research. The study of alternative claims helps us to understand and justify reasonable archaeological interpretations, and to separate them from irrational speculations ranging from the misguided to the intentionally malicious (Schadla-Hall 2004; Fagan 2006a; Renfrew 2006). Furthermore, it is becoming more apparent that alternative claims are not as one-sided, simplistic and dismissible as many professionals are prone to think. Complex alternative claims contest the authority of professional archaeology, and they highlight underlying questions about the nature of authority in scientific disciplines—addressing the way performance and socio-politics can directly raise or lower the status and authority of interpretations about the past.

This thesis uses the case of the Bosnian Pyramids to illustrate issues of authority that emerge from this developing professional debate. Most archaeologists have dismissed or simply acknowledged the Bosnian Pyramid case as cut-and-dry pseudoarchaeology. It seems to fit securely within any diagnosis of fabricated science,

⁶⁰ Sections of this text have come from my MPhil (Pruitt 2007) research. Some text remains intact from my original work, but it has been substantially edited, updated and integrated into this doctoral thesis.

leaving no question as to how mainstream archaeological professionals should define and categorize it (Fagan 2006). But a closer look shows that this type of case study is much larger and more complicated than simple labels like 'real' or 'pseudo' can characterize. The Bosnian Pyramid project and many of its individual team members have held a great deal of authority with the Bosnian public, while also garnering support from a number of accredited professionals and institutions. However, from the beginning, it has also held no valid authority with most professional ('mainstream') archaeologists.

The site is not a hoax, or a forgery, or entirely 'unscientific'. The Foundation has engaged in many genuine and authoritative scientific methods; it has previously employed accredited professionals (along with many more unaccredited amateurs) and has found a number of objects that can be arguably called 'archaeological' (along with many more 'non-archaeological' finds). The site holds a kind of executive and epistemic authority, yet not *credibility*. What does this mean? What are the implications of such a complex, messy site in relation to the professional discipline, and to the scientific authority of archaeological inquiry? An essential power behind this project lies in the way it serves different symbolic, socio-political and economic purposes on local and worldwide scales, and how it is intimately attached to, and working within, larger conditions of politics and performance. In essence, this case draws its authority from much larger issues than just archaeology. Its 'authoritative knowledge' is created and sustained through contextual social arenas.

5.2.3 Categorising Alterity: Pseudoarchaeology

The term 'alternative archaeology' refers to a wide and amorphous range of claims about the past. Indigenous spiritual and reburial issues, malicious manipulation of history for propaganda purposes, pseudoarchaeological claims about supercivilizations, and even some professionally interpreted archaeological reconstructions can all be included under a blanket category of 'alternative'. The Bosnian Pyramid case study can be generally categorised as pseudoarchaeology. Mainstream archaeologists frequently define the term 'pseudoarchaeology' by explaining what it is not: mainstream archaeology, hoax or myth. Mainstream 'archaeology' is defined as the discipline that focuses on the *scientific* "recovery, analysis, and interpretation of the physical remains of past human activity" (Fagan 2006: 24). Pseudoarchaeology, unlike archaeology, does not master a logical chain of thinking or analysis; it is "not a set of serious archaeological principles...designed to gain the

confidence and support of professional archaeologists. The aim is to propose a set of alternative principles and alleged records of sites that will attract and hold the interest and belief of the general public and the popular media” (Flemming 2006: 68). The Bosnian Pyramid project fits this definition of pseudoarchaeology. It is not a hoax like the Cardiff Giant or the Piltdown Man, which were tricks designed to fool academic and non-academic audiences alike. Nor is it a myth based on ignorance of data, like the so-called myth of the Moundbuilders (Feder 2002). Semir Osmanagić’s project, again, “invokes the aura of scholarship without being scholarly in fact and blurs the distinction between real scholarship and ‘alternative’ output” (Jordan 2001: 288-289), a classic case of pseudoarchaeology.

Following the notion that there is a ‘classic’ type of pseudoarchaeology, academics such as Fagan (2006), Flemming (2006), and Lefkowitz (2006) have developed something akin to rubrics that map out qualities of pseudoarchaeology. Fagan (2006: 30-42), for example, “diagnoses” pseudoarchaeology as maintaining the following characteristics:

1. Dogged adherence to outdated theoretical models
2. Disparaging academia
3. Appeal to academic authority
4. Huge claims
5. Selective and/or distorted presentation
6. The “kitchen-sink” mode of argument [multi-disciplinary]
7. Vague definitions
8. Superficiality, sloppiness, and grossness of comparison
9. Obsession with esoterica
10. A farrago of failings [logical fallacies]
11. Expectation of a reward at quest’s end

The Bosnian Pyramid site exactly matches such formal definitions. Mark Rose, with the AIA, referred to this case: “this kind of tale is a staple of the pseudoarchaeology or fantastic archaeology genre” (Rose 2006b).

However, simply defining or categorising this type of site as ‘pseudoarchaeology’ does not satisfactorily characterize the complexity and breadth of the situation. Although attention has been turned towards the issue, which is a step in the right direction, cases of pseudoarchaeology are ultimately social processes within larger socio-historical contexts, and they need to be recognized as such. Wiktor Stoczkowski, from The *École des Hautes Études en Sciences Sociales* in Paris, writes that:

What is at stake is rather our capacity to grasp the cultural dimension of pseudoscience. In fact, once we have shown that it is inferior to academic science (which is a truism for most of the scientists and their public), we still have done nothing to understand pseudoscience as a social phenomenon. (2007: 472-473)

This argument—that complex contexts and conditions allow for alternative archaeology to become preferred accounts of history—is key to understanding how authority plays out in the development, defining and categorising of what is or is not appropriate in any scientific discipline. It also qualifies what makes ideas authoritative or marketable, and offers insight to how the play of socio-politics in any given case of archaeology can walk a fine line between something that gives meaning to the study of the past, and something that overwhelms and unethically takes control of history.

5.3 Socio-Politics and the Reception of Archaeological Authority

5.3.1 Introducing Socio-Politics and the Case of the Bosnian Pyramids

This section examines the way socio-politics can directly affect the production and reception of archaeological messages. In the case of the Bosnian Pyramids, ‘facts’ have been constructed for personal and political gain. This section argues that scientific authority may be positively or negatively received in a situation entirely governed by politics, without regard to ontological validation. This section first gives the structural and executive context of the pyramid project, and it explains the important role of the media in propagating and authorising the accounts of the past produced by the pyramid project team. It then identifies the deeply rooted socio-political processes involved in the case and exposes the ways in which various people and groups invest meaning in an account of ancient pyramids in Bosnia. After explaining the context of places and materialities, and ethnic claims and divisions, this chapter argues that four types of politics create meaning around the site: national identity, ethnic claims, politics of money and politics of academics. This chapter argues that socio-politics affect how receptive an audience may be to an account of the past, and that in many cases, issues of validation, fidelity and ontological significance matter far less than individual or collective social values in the way a public initially receives or promotes archaeological authority.

5.3.2 The Power of Politics, Places and Materialities

Laurajane Smith writes: “Heritage is about a sense of *place*. Not simply in constructing a sense of abstract identity, but also helping us position ourselves as a nation, community or individual and our ‘place’ in our cultural social and physical world” (2006: 75). Historically, Bosnian culture has intertwined materiality and place with ethnic and religious identity: “the physical and social landscape of a region is more than a palimpsest of long-term settlement features; it is an imprint of community action, structure and power on places” (Chapman 1994: 120). Places in Bosnia are more complex than just backdrops and settings. They are intimate features of social life, power and politics. Archaeology and heritage play a key role in this embedded cultural-spatial landscape, where identity “is forged through association with the monuments and artifacts of past ancestors, for there was often strong residential and manufacturing continuity in towns and villages from late medieval to modern times” (Chapman 1994: 120). All Bosnian towns have a long history closely associated with their ethnic-religious populations. Visoko, for example, is considered a primarily Muslim Bosniak town and has a long history of Islamic influences since the medieval invasion of the Turks (Malcolm 2002).

Especially in post-war Bosnia-Herzegovina,⁶¹ nothing goes without an identity of place and ethnicity. Layton and Thomas remark that many people from the former Yugoslavia “had always thought of themselves as Yugoslavs rather than Serbs or Croats. As Yugoslavian unity broke down, however, so many found it increasingly expedient...to secure a national identity” (Layton and Thomas 2001: 15). Today, the main ethnic groups within Bosnia are trying to cling to both a sense of national identity and a separate ethnic one, which segments the country into different religious-ethnic material cultures. Every thing, person, and place is tensely divided: Bosniak, Croat, Serb. Every individual, town sector, market, or heritage site has its respective religion:

⁶¹ Bosnia-Herzegovina has often been called “the microcosm of the Balkans” (Malcolm 2002: 1). The current country is divided and identified by ethnic and religious groups of people who associate themselves with different nationalities, notably: Bosniak Muslims, Croatian Catholics, and Serbian Orthodox Christians. The same mixed ethnic racial groups, which inhabited Bosnia-Herzegovina more or less peaceably for hundreds of years, developed into national identifications with the countries of Bosnia, Serbia, and Croatia in the nineteenth and twentieth centuries under Austro-Hungarian rule. These groups were momentarily unified after World War I under the single Balkan state of Yugoslavia.

Serbia, however, held ambitions for Yugoslavian dominance when the state began to collapse in 1989. The resulting Yugoslav civil war in Bosnia (1992-1995), was a violent, international mess. The Serbian army besieged the capital of Sarajevo, killing many civilians. Bosnian Serbs, Croats, and Bosniaks were divided, and the country became a three-way ethnic battlefield between Bosnia, Serbia and Croatia. Although atrocities were committed on all sides, Bosniak Muslims were the most targeted and victimized ethnic group. The country experienced the largest genocide in Europe since the Holocaust; it is estimated that 150,000 people died, mostly Muslims, and half the population was left homeless or fled the country (Clancy 2004: 47; Kampschorr 2006: 24).

Muslim, Catholic, Orthodox. The Mostar Bridge is considered Bosniak Muslim, for example, the old Bas Carsija market of Sarajevo is Muslim, and the pilgrimage site and city of Medjugorje is Croat Catholic. Heritage sites such as these and hundreds of others were deliberately shelled by combating ethnic armies during the recent war. Most were targeted for their material culture associations with an opposing ethnic identity (Chapman 1994: 122; Barakat, Wilson et al. 2001: 171). Ideologically, “the deliberate destruction of mosques, churches, museums, civil records, monuments and artefacts in the Balkans suppresses the evidence of a culturally diverse and hybrid past, in favour of a mythical ‘golden age’ of ethnic uniformity” (Layton and Thomas 2001: 12). Each ethnic group has a history of trying to claim that vision of a ‘golden age’ as their own. It is within this climate of material identity, of post-war ethnic “tolerant hostility” (Zhelyazkova 2004: 17), that Osmanagić’s golden pyramid hills have inevitably become deeply entrenched in the politics around them.

5.3.3 Constructing Authority through Nationalism and Identity

From the early stages of its development, the Bosnian Pyramid project has been attached to national identity politics. Semir Osmanagić has made a brave attempt to construct and claim the site “for everyone,” of all Bosnian ethnicities, as a site of monumental importance because it transcends ethnic quibbling and—for once—can represent Bosnia as a national whole. Osmanagić insists that his site is a matter of national pride, “something that can unite people instead of dividing them” (quoted in Foer 2007). Osmanagić maintains that, “Bosnia and the Adriatic pool is the second oldest oasis of life in Europe, with 27.000 years on uninterrupted presence of intelligent man” [*sic*] (BosnianPyramids.org 2006). He continues that, “Bosnia is a source of civilization of Europe and that is a reason enough that Bosnians should be proud of their heritage” (BosnianPyramids.org 2006). These bold statements suggest that not only is Bosnia the origin of all the country’s ethnic groups, but it also is an origin of Europe as a whole. Pyramid-unifying nationalism is even visually identified: the Bosnian Pyramid of the Sun Foundation logo is a yellow pyramid icon attached to an inverted top blue triangle and stars of the Bosnian national flag. [Figure 17] Such visual propaganda makes the pyramid literally *part of* the national flag, strongly stating that the pyramids and Bosnian nationalism are one and the same. Thus, the visual message is that to believe in pyramids is to believe in Bosnia, and to not believe in pyramids is to be a traitor to unity and nationalism.

This is doubtlessly why some Bosnian professionals who oppose the project have been called national “traitors” in the country. Foreign academics have been “treated to abuse and ridicule” and told that they should stay out of business they do not understand (Harding 2007: 43). Members of the public have recognised that, “[a]ny criticism over such pseudoscientific approach in Bosnia-Herzegovina is stamped as an unpatriotic act while critics are stigmatized as traitors in public, since the pyramid project has since its beginning been identified with a ‘national interest’” (Stultitia 2007). Project opponents are often explicitly identified and condemned. In one letter, for example, Osmanagić accuses specific professionals of trying to divide the country politically:

The group of anti-pyramid opposers like Blagoje Govedarica, Zilka Kujundzic, Svetozar Pudaric, Mirko Babic, Gavrilko Grahovac, Ivan and Dubravko Lovrenovic, are working hard to debunk the pyramid research project, spreading voices that the project is supported only by ‘Bosniak ambiences’. They are trying to destroy the project by transforming it in a sad story in three pieces about the Bosnian national and religious reality. Those persons intentionally ignore the fact that the Foundation always underlined that this project has nothing to do with single nations, religious beliefs, but that it belongs to an ancient past about which all should be proud off. Thus, becoming an integrative factor that should unite, not divide. [*sic*] (Osmanagic 2006)

Some academics have responded to such propaganda with anger, contempt and pleading. Bosnia’s foremost prehistoric archaeologist, Zilka Kujundzic-Vejzagic, received threatening letters for speaking out against the project (Foer 2007). Nevertheless, many academics both in Bosnia and abroad launched several unsuccessful campaigns to try to stop the program, sending out petitions (Archaeology.org 2006; NoPyramid 2006), and even appearing opposite Osmanagić on television programs.



(Image courtesy of Archaeological Park: Bosnian Pyramid of the Sun Foundation)



(This is a freely distributed image)

Figure 17: Official logo of The Bosnian Pyramid of the Sun Foundation (left). The logo incorporates an inverted Bosnian flag. Compare with Bosnian national flag (right).

Osmanagić also endorses a political unity campaign through national Federation politicians and parties. Although some of his networking is undoubtedly for financial gain (see Section 5.3.4), Osmanagić also seems to be genuinely promoting a sense of

national pride through political support. In an online interview, Osmanagić says, “We all agree? Well you see, it is possible! Bosnian pyramids have united all levels of government showing political maturity starting with Visoko municipality” (BosnianPyramids.org 2006). High-level political support is abundant; important politicians like the former President Chairman Sulejman Tihic have approved the project. The President Chairman publicly announced to Montenegro that they and “all other regional presidents as well as the media [should] come and see the pyramid remains” (HINA 2006). And when Osmanagić’s project faced an uncertain future when its permits were pulled in June 2007, the Federation’s Prime Minister Nedžad Branković stepped in, restored the permits, and voiced support for Osmanagić. Branković firmly stated, “The government will not act negatively toward this project” (Woodard 2007a). Speaking to reporters, he asked, “Why should we disown something that the entire world is interested in?” (Woodard 2007a). Supporters seem absorbed with the prospect of achieving international recognition—or at least appearing to have it—and much of the authority behind the project comes from the prestige of simply being high-profile in the media.

Bruce Trigger writes of nationalistic archaeology: “The primary function...is to bolster the pride and morale of nations or ethnic groups. It is probably strongest amongst peoples who feel politically threatened, insecure or deprived of their collective rights by more powerful nations” (Trigger 1984: 360). This description certainly applies to Bosnia, which experienced a great deal of suffering in the recent war, leaving its citizens in a state of “tolerant hostility” (Zhelyazkova 2004: 17). In this context, the pyramid narrative provides a positive symbol of nationalism, and it is hardly surprising that so many members of the public and national politicians have supported the project. Tangible, visible symbols, like the Foundation logo, as well as the monumental and striking pyramidal hills in the landscape, are material reminders of ‘great things’ that could have happened in the past and might happen again the future. I would argue that much political support for the project has emerged because people have been grasping for more tangible, rooted symbols of their newfound nationalism. The material nature of the pyramid ‘archaeology’ means that a rebuilding nation has something sturdy and identifiable to reach out for; the nonhuman and material aspects of this case are as important as the socio-politics that are contextualising them. I would argue that the inherent materiality of the project—which has been created through physical interactions with the landscape, and deliberate manipulation of iconography and logos—is central to its authority in political arenas.

5.3.4 Authority through the Politics of Money

Politics of money are also intimately attached to the success and authority of the project. In depressed post-war Bosnia-Herzegovina, money is a sensitive issue. The country is still rebuilding and stabilising, struggling against high levels of unemployment and a lagging economy “due to the fact that there are no strong institutions or political stability” (Zhelyazkova 2004: 14). Regarding the Bosnian Pyramid project, there are two sides to this coin: the first is the argument that, regardless of its pseudoarchaeological nature, the project has already demonstrated real economic benefits to the region. Secondly, there is the argument that the money spent on the project would be better spent on post-war restoration efforts, or at least on ‘real’ professional archaeology. Much of the site’s high-profile status and presence has emerged from this financial debate.

Those who have argued that the pyramids will bring social benefits have already seen results. The project has pumped money into Visoko and the broader country through tourism, and it offers hope of more to come. By 2007, Visoko had already changed dramatically from its dilapidated post-war state. Before the pyramids, the town received around 10,000 visitors a year. In 2007, it reported having that many visitors in a single day. The project attracted 250,000 tourists to the town in 2006, bringing in a flood of new money and an economic boost (Monaim 2007). Visoko residents initially welcomed this change as something of a miracle. When interviewed by a foreign reporter, Esref Fatic, the owner of a souvenir shop in Visoko, emphatically insisted, “something will be found under the hill” and thought that “any kind of discovery means a lot after so many years of nothing...people will come here and spend money and that would mean our youth has something to do” (Zimonjic 2006).

Most of the town’s population still enjoys an influx of people. In 2006, the main hotel in Visoko changed its title from “Hotel Hollywood” to “Motel Piramida Sunca”, which translates to ‘Pyramid of the Sun Motel’ (Bosnian-pyramid.net 2006). Craft stores sell tee shirts and pyramid souvenirs, and cafés serve coffee with pyramid-stamped sugar packets and pyramid-shaped pizza (Economist.com 2006). One child I interviewed, a ten-year-old local boy, now makes more money than his parents by waiting alongside the road and offering tours to visitors. Local volunteers, like this boy, also employ much of their free time by excavating with ‘Mr. Semir’ and the other volunteers (local interview, personal communication 2007). Another local resident I interviewed pockets a good bit of money by selling homemade pyramid crafts from his house garage (local interview, personal communication 2007). In his spare time, he takes visitors to a new restaurant that was built just to accommodate tourists, which

advertises by way of a pyramid made of bricks decorating the lawn [Figure 18]. The resident insisted to me that these changes were just the beginning of the town's development: in summer, when visitor numbers are highest, the town roads cannot handle the traffic, so, he said, the city has plans to widen the roads and pave the dirt ones the lead up the hill [Figure 19] (interview with local resident, personal communication 2007). Pyramid hype also extends outside of Visoko. Tourist Agencies in Sarajevo and neighbouring areas—even as far as Croatia—have started advertising organized pyramid tours (Maestral 2007; Negra 2007). Brochures line the tourist information desks in the capital city of Sarajevo. More than one professional archaeologist, knowing nothing about the site beforehand, has been lured to Visoko to go see the archaeology listed in the brochures. In these tourist brochures, the site is often listed as a highly respected, authorised and genuine archaeological project (interview with Ezra Zubrow, personal communication 2010). The authority of the project is latent in the streamlined and professional logos on the brochures, and in the authoritative displays of the magazines set out on tourism counters.



Figure 18: New businesses, like the one above, were built in Visoko to accommodate the influx of tourists. This restaurant sits near the entrance to one of the pyramid tunnels, outside the main city streets. The business advertises with a large brick pyramid on its front lawn. *Photo by Tera Pruitt.*

From the beginning, Osmanagić and the pyramid Foundation have had their eye on tourism. In 2006, Osmanagić announced plans of “research activity” that would be “opening more areas of the Pyramid to tourists”. He claimed that his “main research focus from 2008 onwards will be the provision of more tourist facilities” (Piramidascunca.ba 2006), insisting that Visoko would eventually have over a million tourists a year. Volunteers and local residents have seen pyramids as a way into the future: “The pyramids will help us speed the development of the economy, and when we have done that the EU will accept us” (quoted in Economist.com 2006). The idea that a grand archaeological site could boost political authority of a small country and launch it onto the world stage alongside bigger powers like the European Union is tantalising. These outsized hopes also explain why political parties interested in the site for its economic potential have engaged in “outright political posturing” (Foer 2007). Haris Silajdzic, a Bosniak member of the rotating presidency, publicly stated, “these enthusiasts are getting people excited and interested in something positive and are helping the economy of a poor part of the country” (Woodard 2007b). Many of these interested politicians have used the site as a campaign strategy, patting Osmanagić on the back and smiling at the camera. [Figure 20]



Figure 19: Tourism is new to Visoko. Makeshift souvenir shops, like the this garage-turned-business, are now common. Local residents, like the boy in the foreground, can make money giving tours to visitors. Plješevica Hill (Pyramid of the Moon) can be seen in the distance, behind the garage shop. Photo by Tera Pruitt.



Figure 20: Semir Osmanagić poses for the camera with Ivica Saric (left), Sarajevo's Minister of Culture in 2006. A large number of volunteers can be seen excavating in the background. Image courtesy of John Bohannon: <http://www.johnbohannon.org/NewFiles/bosnia.pdf>

These campaign strategies usually operate as external factors, pumping up the authority of the site beyond Osmanagić's control. One notable Sarajevo radio presentation in 2006 exemplifies how stunned Osmanagić was to hear how he was used in a campaign:

ANCHOR: Have you thought about.. that the whole idea of pyramids in Visoko could be used for preelection purposes?

OSMANAGIĆ: [...] My wish is, in fact, that this project has support of all political establishments, because I think that is in the interest of this country ... and it will not interfere with political.. uhm.. elections [...]

ANCHOR: But what if political elections interfere with the Foundation?

OSMANAGIĆ: How?

ANCHOR: By Sulejman Tihic coming to kiss you [...] do you think that this kiss will not be worth, I don't know, a thousand votes in Visoko tomorrow? Because you're not popular only in Visoko, but in that region, have you thought about that?

OSMANAGIĆ: No.

[sic] (Radio-202 2006)

The creation and promotion of the site has gone beyond just the control of Semir Osmanagić. Many politicians seem to realize that Osmanagić's excavation is pseudoarchaeology, yet they have continued to promote the project because of its economic potential. On whether or not the project should be shut down, President Haris Silajdzic said, "Let them dig and we'll see what they find. Besides, it's good for business" (Harding 2007). A spokesman for the foreign Federation representative in charge of

Bosnian Affairs, Christian Schwarz-Schilling, supported the project, calling it “the world’s first victimless pyramid scheme” (quoted in Foer 2007).

But those who oppose the project see plenty of victims. Many people, especially foreign academics, have said that the social and economic gains are probably only short-term and that the money spent on the project would be better put to use in post-war reconstruction efforts. Ahmed Khattab, Egypt’s ambassador to Bosnia-Herzegovina, says the pyramids “should not be a top priority. This digging will require millions and millions, and meanwhile artifacts are being damaged in the museums for lack of heat. Bosnia is a poor country, and there have to be different priorities” (quoted in Woodard 2007b). The project’s actual figures are daunting. In 2006 alone, the Bosnian Pyramid of the Sun Foundation raised about \$500,000, not counting in-kind donations such as estate cars and free loans of bulldozers and transportation. Osmanagić personally contributed about \$100,000 (Foer 2007; Woodard 2007b; Harding 2007; Foundation interviews, personal communication 2007). These figures are staggering in post-war Bosnia, which is still littered with damaged cultural property that suffers for lack of reconstruction funds, such as the damaged National Museum and the National Library, which still sits as a burnt-out shell in downtown Sarajevo (Chapman 1994; Barakat, Wilson et al. 2001). Archaeologists such as Anthony Harding of the University of Exeter have expressed distaste at the amount of money going into the Bosnian Pyramids project: “it adds insult to injury when rich outsiders can come in and spend large sums pursuing their absurd theories...instead of devoting their cash to the preservation of the endangered genuine sites and monuments in which Bosnia-Herzegovina abounds” (2006).

The politics of money add a crucial dimension to the project. Once again, it is the *tangible* and *material* results of the project that matter as much or more than the abstract conceptualisation of the archaeology as ‘fact’ versus ‘fantasy’. The value, acceptance and authority of this case rests fundamentally on its physical presence, which can be pointed to by politicians and the public alike as something that materially benefits people and places.

5.3.5 The Politics of Experts and Expertise

5.3.5.1 The Authority of Credentialed Experts: The Egyptians

Along with his own amateur archaeology work, Osmanagić has also engaged the authority of ‘authorised’ or credentialed scientists and institutions to back his project.

Notably, he has enlisted a number of “scientific experts” to support his work (Piramidasunca.ba 2007). Although he initially engaged in “a naughty habit [of] announcing project support from foreign archaeological authorities who either weren’t supportive or weren’t authorities” (Foer 2007), Osmanagić did later employ a number of professionals on his team who do hold some level of credentialed authority within the mainstream discipline.

The most notable academic supporters have been a group of Egyptian geology experts who came to Visoko with a passionate desire to help support Bosnia after the war. Among these are Dr. Aly Abd Alla Barakat, a geologist from the Egyptian Mineral Resources Authority, and Dr. Mohammed Ibrahim Aly, who has reportedly taught Egyptology and other subjects at the University of Cairo. The latter is reported to have visited Visočica Hill (Pyramid of the Sun) and said the site was “extraordinary, definitely not made by nature” (Piramidasunca.ba 2007). Perhaps the most publicised Egyptian supporter is Dr. Nebil Swelim, an Egyptologist from Cairo, who claims three doctoral degrees (Swelim 2010a), and whom I discuss in more detail below. The fact that these scholars are from Egypt and have only tenuous knowledge of Bosnian archaeology has not seemed to faze supporters. For many in the general public, the idea of ‘pyramids’ is so intertwined with the identity of ancient Egypt that many have seemed to have taken the authority of these Egyptian geologists and Egyptologists at face value.⁶²

Dr. Nebil Swelim’s participation with the Foundation is a particularly interesting saga of authority and expertise. In the public eye, Dr. Swelim has been promoted by the Foundation as one of the most prestigious—and perhaps one of the only ‘archaeological’ as opposed to ‘geological’ or ‘independently researching’—academic supporters of the pyramid project. His name and authority has been exploited by Osmanagić and the Foundation in strategic ways, such as naming Swelim the (ceremonial) President of the Foundation and President of the ICBP Conference. By naming a ‘triple doctorate expert’ the ceremonial head of a controversial organisation, Osmanagić shifts the burden of

⁶² This connection of the Bosnian ‘pyramids’ to the Egyptian pyramids has also resulted in a great many Bosnian Pyramid publications with a heavy hyperdiffusionist slant. Osmanagić claims to have visited pyramids around the world, implying that this makes him ready to identify and study archaeology in Bosnia if it appears in pyramidal form. In general, significant controversy about the appropriateness of ‘pyramid’ qualifications has followed Osmanagić, as well as many of the Egyptian team members. In an interview [see Appendix G], former Foundation team member Andrew Lawler said that, “Apart from Aly Barakat, [the Egyptians’] role was little more than that of tourists. I know that some...felt they were being used as promotional tools” (Foundation member, personal email communication 2010). In the same interview, the former team member said that Dr. Nebil Swelim, unlike some of his colleagues, relished being in the limelight. This suggests that Swelim had personal and political motivations to support the project, since Swelim’s supportive reports in favour of the Bosnian pyramid site were written “after spending under 2 hours on Visocica”.

authority and expertise to Swelim, who can be promoted as a more established and senior foreign expert. For the public, when a multi-credentialed expert with connections to ‘other pyramids’ is advertised as a project leader, the pyramid narrative appears to be backed by more substantial institutions than just one celebrity in an Indiana Jones hat. This strategy is what Bruno Latour calls ‘bringing in allies and support for the argument’, a classic “argument from authority...it creates a majority to impress the dissenter even though the dissenter ‘might be right’” (Latour 1987: 31).

Swelim has consistently defended his interest in the project as his way of offering support to post-war Bosnian people, and he has thrown his full support into Osmanagić’s version of quasi-archaeological science. Swelim’s support has surprised some of his personal friends. In an interview at Cambridge, Dr. Seif El Rashidi, the coordinator of the Durham World Heritage Site, called his friend Swelim a “serious, no-nonsense kind of man” with sincere academic interest in archaeology (personal communication, 2009). This account of Swelim’s personality contrasts with those of Semir Osmanagić and other core members of the Foundation, who have employed a considerable degree of whimsy in their approach to the past, with their constant references to conspiracy theories, alien encounters, new age wisdom and paranormal activity. Since Swelim has never excavated at the Bosnian Pyramid site, and has only published lengthy ‘reports’ about what he argues is the ‘nature’ of a pyramid (which boils down to the practically simple and unoriginal argument that pyramids are artificial structures with large bases and pointy tops), his support of the Bosnian project might be seen as politically motivated (Swelim 2007; Swelim 2011).

While scholars like Swelim seem to have good intentions, they have given no real evidentiary justification for their support. In response to a number of articles and emails published by opponents who criticise his role in the project (Irna 2008b), Swelim published a variety of reports about Visočica’s ‘pyramid’ status:

These arguments led to 5 conclusions: 1. The pyramid hill Visočica is a new introduction to the local scenarios of pyramid science. 2. Visočica is justified for a pyramid nomination. 3. The main subjects to understand the pyramid hill Visočica are geological. 4. Perhaps our present wealth, technology and recourses are not capable. [5.] The true measure of a pyramid expert is his output on pyramid science. (Swelim 2010b; Swelim 2011)

Swelim’s insistence on the existence of something called ‘pyramid science’ is telling in and of itself. By extracting a ‘pyramid’ or any archaeological object or structure out of its cultural context, you make it virtually meaningless—pyramids in ancient Egypt were constructed for a multitude of cultural reasons. Those reasons would have nothing to do with pyramidal structures built in the Bosnian past, supposing such pyramids existed in

the first place. Swelim is attempting to culturally compare ancient Egypt of 2600 BCE to an alleged culture in Bosnia at 34,000 BP (see radiocarbon dating, Section 5.5.2). This comparison across thousands of years and miles is meaningless without some justification—and none is given.

In his most recent report, Swelim concludes by commenting on what makes the authority of a scientific ‘expert’:

Some scholars gain a reputation of being “pyramid experts” by occupying a post or an administrative or a teaching position for some time. Others develop a charisma and become stars on TV documentaries; unfortunately some of what they claim is received without any verification or checking. The true measure of a pyramid expert is his output on pyramid science. (Swelim 2010b)

Such a statement is somewhat at odds with the current situation in Bosnia. Most of Osmanagić’s experts seem to lend authority to the site by simply occupying a title or position, or through credentials claimed by having ‘looked at pyramids for some time’. In public arenas, Osmanagić himself has become seen as an ‘expert authority’ through his media personality, charisma and celebrity status from TV documentaries. Osmanagić, Swelim and the other members of the team have not been able to publish in peer-reviewed journals, where their work would be ‘verified’ and ‘checked’ before public release. The important point is that, to the public, these ‘official reports’ and ‘strong statements of authority’ that are published online by Swelim and Osmanagić lend authority to the project, not only because Swelim takes such a simple, hard-line and confident approach to what he believes is ‘right’ or ‘wrong’, but also because of the language used: they talk of reports, publishing, pyramid science, output and credentials. To many members of the public, these arguments sound much like ones that are fair and justifiable. Indeed, they sound just like the arguments voiced by the professional archaeologists who oppose the project (Sarajevo interviews, personal communication 2008).

I observed further controversy around the Egyptian authority in the project during my attendance at the 1st Scientific International Conference of the Bosnian Pyramids (ICBP) in August 2008 (more discussion on this conference in Section 5.5.3, below). A large group of Egyptian professors and students from the Library of Alexandria and the University of Cairo were invited to participate at this ‘scientific conference’. Both sides—members of the Bosnian Pyramid Foundation and members from the Egyptian attendees—quietly criticized what happened at the conference. One Egyptian hydrogeology expert I interviewed said that in his opinion the whole landscapes of Visoko and Zenica (where the stone balls were found) were naturally formed mountains and stone, made by glacial, hydrogeological processes. This geologist

implied that he was attending the ICBP conference to socially support post-war Bosnia, as well as to enjoy a free trip to a “beautiful country” (participant interview, personal communication 2008). During the conference, another Egyptian geological expert, Dr. Mohamed Ibrahim El Anbaawy, viewed the excavations on the first day, then disagreed sharply with Osmanagić’s pyramid hypothesis. For the rest of the conference, he spent considerable time trying to teach basic geological principles to the conference attendees and the pyramid team, arguing that hydrogeology could explain all of the formations that the pyramid project had excavated and uncovered. The Foundation members were unhappy with his criticism, and more than one Foundation member at the conference expressed their frustration with his opinions. At one point, when Dr. El Anbaawy tried to point out natural geological stratigraphy to a group of pyramid supporters, tensions mounted to raised voices and yelling [Figure 21]. On the other side of the divide, some members of the pyramid team also (quietly) expressed frustration and dissatisfaction with the Egyptians, complaining that Osmanagić had paid for the Egyptians’ trip to the conference and many of them were more interested in shopping than in validating pyramid archaeology.



Figure 21: Dr. El Anbaawy lecturing to members of the Foundation and the ICBP conference participants, arguing for a natural and geological origin of the supposed pyramids. (He is the man the grey shirt: on the left photograph, he is standing and gesturing on the right side of the crowd; on the right photograph, he is seated with a notepad and trying to give a geology lecture to a crowd of pyramid supporters. Photos by Tera Pruitt.

Such interaction is clearly fraught with politics, and this critical and messy interaction between the Foundation and their own ‘supporters’ has not been published for public scrutiny in any meaningful way. During the conference, the dissenting geologist Dr. El Anbaawy was on the final panel for drafting public conference conclusions, and Dr. Swelim—who voiced utmost support, but was also of the opinion that the hills are, at least at base, natural formations—both insisted the *geological*

significance of the hills needed to be included in the conference conclusions (much to the chagrin of Osmanagić and other alternative theorists on the panel). The compromise by the Foundation was the inclusion of the phrase ‘*geo*-archaeological’ in the final press releases, which I would argue (after observing the whole of the ICBP interactions) was primarily the result of the week-long contestation by Dr. El Anbawwy. The final publications and press conferences of the ICBP conference simply included the line that the Bosnian Pyramid project was “important *geo*-archaeological and epigraphical research that requires further multidisciplinary scientific research” [*sic*] (ICBP 2008)—meaning the site was debated as being *geo*-archaeological, and some participants of the conference thought the ‘pyramid’ conclusion was far from clear-cut. To the public however, this strong statement ‘blackboxes’ all contestation, belying any empirical debate and projecting a robust and authoritative tone.

I would argue that contestation and exchange at the ICBP conference represented some genuine academic engagement, at least on the part of Dr. El Anbawwy, who successfully critically engaged the pyramid supporters and shifted some of the conference conclusions to include the terms like ‘geological’. But all ‘backstage’ contestation was ultimately ‘blackboxed’ in the final press releases made for the public [Figure 22]. Instead of referencing any contestation or genuine nuance in the empirical record, the public release lent the appearance of validation by a long list of ‘academic heavyweights’ with PhDs. The conclusions were professionally edited, were broadcast on TV and were shiny-looking, a performance which lent authority to the much simpler account of “pyramids in Bosnia”.

5.3.5.2 The Authority of Credentialed Experts: Team Members

In addition to outside experts like Swelim, a handful of Foundation team members have had academic degrees behind their names. Two accredited archaeologists were briefly employed to excavate for Osmanagić’s team, although both have now quit the project. One was an archaeologist named Rafaella Cattaneo, who only briefly joined the project. Later, an archaeologist named Andrew Lawler, who graduated with a BA in archaeology from the University of Cambridge in 2006, spent significantly more time at the site. After working with archaeological field units in the UK, Lawler, who had a general desire to work in

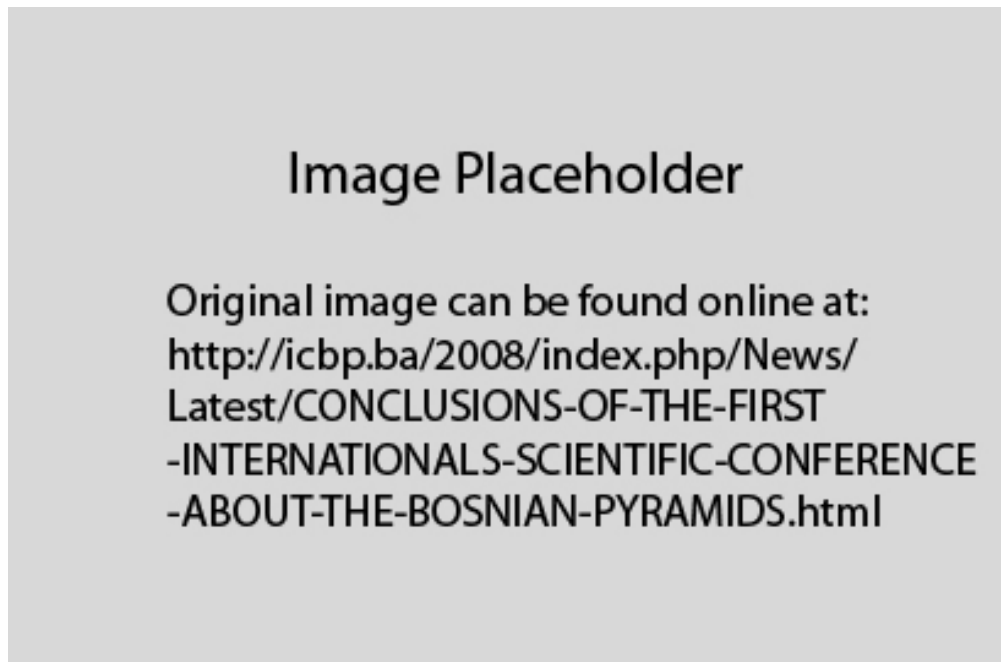


Figure 22: Conclusions from the ICBP Conference, which 'black box' almost all of the debate and contestation that occurred during the conference proceedings. *Conclusions online at: <http://icbp.ba/2008/index.php/News/Latest/CONCLUSIONS-OF-THE-FIRST-INTERNATIONALS-SCIENTIFIC-CONFERENCE-ABOUT-THE-BOSNIAN-PYRAMIDS.html>*

Bosnia, joined Osmanagić's team. During his time with the project, Lawler kept a low profile and did not openly discuss his negative opinion about the 'non-archaeological' nature of the site (Lawler, personal communication 2009). Noting the project's lack of organisation, recording and trained archaeological methodology, Lawler instituted a field guide manual, an artefact organisation system, a stratigraphic recording system, context sheets and other standard archaeological methods. His primary work area was on Plješevica Hill (Pyramid of the Moon) site. In an interview I had with Lawler after he left the project, he explained:

"Nearly everything was fantasy during my time there. Only the burnt stones from the Moon pyramid were real and older than the war. At KTK tunnel, an abundance of 19th and 20th century stuff was coming out, but most of disappeared, and I guess since I left the rest has been disposed of. When I reorganised the artifact store, about 10% of what was in there was real. The rest was fossils or 'pretty stones'. There was some Neolithic and medieval pottery, a flintlock, an iron knife (presumably medieval) some animals and glass, and 10-20 animal bones, along with some bone fragments." (Lawler, personal email communication 2009) [See Appendix F and G for interview transcripts]

Lawler also took radiocarbon samples and sent them off to various radiocarbon labs, like Oxford and Kiel, at the request of Semir Osmanagić. While Lawler did institute more professional standards at the Moon Pyramid site, he was not in charge of any other excavation location, such as Visočica Hill (Pyramid of the Sun), Gornjia Vratnica (the 'rock quarry' site), Zavidovići (stone balls near Zenica) or any of the tunnel sites. These sites, he says, were simply dug with backhoes and shovels by volunteers in the local community on their own time. While Lawler did record data in methodologically appropriate ways, none of his interpretations of the data ever became part of the official record on the site. Osmanagić was in charge of publishing reports and books on the project, producing almost all of the project's 'final product' accounts of the past. When Lawler presented his report on the radiocarbon samples from the tunnels at the ICBP conference, he (and his unmodified report) suggested that there were natural/geological causes for the organic debris that had been dated. Some of the paragraphs on the natural origin for the radiocarbon material were later edited by Osmanagić before he put the report on his website, in order to promote the supposed artificial/human origin for the organic material (Irna 2008c; Irna 2008a; Lawler 2008). Lawler quit the organisation soon after he presented this material to the ICBP conference (Lawler, personal communication 2008; 2010).

In terms of leadership and accreditation, Semir Osmanagić has (only recently) achieved recognised degrees and accreditation for himself, in the field of archaeology. When Osmanagić began the project in 2005, he only held a Masters degree in economics,

and his credentials solely rested on experience travelling the world and looking at pyramids from different cultures, along with his authoring of books like *The World of the Maya* which argued for extraterrestrial origins of the Maya culture (Osmanagic 2005b; Osmanagic 2005c). This changed in 2010, when Osmanagic obtained a PhD degree from the University of Sarajevo, in the Faculty of Political Science under the supervision of Prof. Hidajet Repovac, *History of the Civilizations* (Osmanagic 2009). Osmanagic's doctoral thesis on the ancient Maya included a number of controversial claims, such as the argument that they were responsible for the creation of advanced science, which was justified with dubious artefacts like crystal skulls⁶³ (Sax, Walsh et al. 2008; Osmanagic 2009). In his dissertation abstract, Osmanagic immodestly references his own doctoral work:

There is no scientific precedence that could serve as an example of this pioneering research and analyses...Assertions that the Zapotecs (or Olmecs, depending on the author) were the cradle of all other cultures (including the Maya, Toltecs and Mistecas) are no longer valid. The archaeological evidence shows that the Maya are the oldest civilization in this region. (Osmanagic 2009)

The fact that Semir Osmanagic now has full doctoral accreditation from the University of Sarajevo, a widely respected university in Bosnia-Herzegovina, is meaningful. At the beginning of the project, when Osmanagic held only unrelated degrees, professional archaeologists used his background to try to undermine his authority. Bosnian archaeologist Enver Imamovic, a former director of the National Museum in Sarajevo, was quoted as saying, "This is the equivalent of letting me, an archaeologist, perform surgery in hospitals" (Rose 2006b), implying that Osmanagic did not have the appropriate expertise, training or degrees to excavate. However, with official accreditation, the weight of authority shifts in Osmanagic's favour, at least in appearance. Regardless of whether Osmanagic's PhD may be attributable to his high-profile celebrity status in the country, or whether he earned his degree by crafting a genuinely strong thesis for his controversial claims, the fact remains that his use of expertise and accreditation is central to Bosnian Pyramid project's continued status and high degree of authority in the country.

I talk in detail of the role of experts and expertise in the project because of the vital impact that their presence has had on the authority of the project as a whole. Accreditation and institutionality—at least the discourse around it and the appearances of it—have been some of the main ways the project has bolstered its own authority and

⁶³ Crystal skull artefacts, like the Mitchell-Hedges Crystal Skull which Osmanagic has previous associations with, are asserted to be 'fakes' by academic scholars, who argue that they are modern creations (Sax, Walsh et al. 2008).

clout. By attaching itself to scientific institutions and methodology, and by promoting connections to apparent credentials and peer review, they are engaging in a classic “argument from authority” strategy. This—it should be stressed again—is meant to create the appearance of “a majority to impress the dissenter even though the dissenter ‘might be right’” (Latour 1987: 31), and in the case of the Bosnian Pyramids, the strategy works well to construct the appropriate performance of scientific support.

5.3.6 Contestation and Academic Authority

International professional archaeology has responded to the project in waves. Following the media’s initial portrayal of Osmanagić as a serious amateur archaeologist, professional archaeologists expressed interest. Dr. Bruce Hitchner at Tufts University initially stated, “My impression is that they may be monumental elite tombs from the pre-Roman period” (Blogger.ba 2007). Zahi Hawass, former Head of Egypt’s Supreme Council of Antiquities in Giza, initially said, “It is quite possible there are pyramids in Bosnia” (Blogger.ba 2007). The Archaeological Institute of America (AIA) even hosted a blurb about the Bosnian Pyramid excavation on its fieldwork opportunities website, advertising Osmanagić’s request for field volunteers (Rose 2006b).

But as Osmanagić’s unsubstantiated claims and ‘fringe’ background became fully apparent, this congenial reaction soon turned to cynicism and scoffing. The AIA fieldwork advert was quickly withdrawn. Archaeologist Anthony Harding, who was then the head of the European Association of Archaeologists, was one of the first objectors to respond: “In most countries of Europe those with wacky theories about ‘hidden mysteries’ on presumed archaeological sites are free to propound them but not to undertake excavation...it adds insult to injury” (Harding 2006). Zahi Hawass retracted his previous speculations and issued a public letter stating that, “Mr. Osmanagić’s theories are purely hallucinations on his part, with no scientific backing” (Hawass 2006). This cynicism soon turned to panic when it became apparent that the pyramid frenzy was not subsiding, that it was actually growing. Major publications like *Archaeology Magazine* (Kampschror 2006; Rose 2006a; Rose 2006b), *Science Magazine* (Bohannon 2006a; Bohannon 2006b), *British Archaeology* (Harding 2007), *Discover Magazine* (Bohannon 2008) and *Smithsonian Magazine* (Woodard 2009) published sombre, warning articles. Today, most professional archaeologists recognize the site as pseudoarchaeology. Richard Carlton, archaeologist at the University of Newcastle, despairs: “Support of this raft of nonsense has only increased. I have no idea what to do other than to continue to present reasonably argued opposition” (Bohannon 2006b).

During this initial reaction, one group of academics entreated politicians to force Osmanagić to stop excavations on Visočica Hill (Pyramid of the Sun), citing the importance of the medieval fort that sat on the summit, and giving evidence that Osmanagić had already destroyed some genuine medieval and Neolithic sites in the surrounding area (Archaeology.org 2006). In 2007, the Bosnian government restricted Osmanagić from excavating anywhere near the top of Visočica Hill near the medieval fort. Meanwhile, professional archaeologists from the National Museum in Sarajevo were granted permits to excavate the medieval fort themselves, starting in 2008. However, attempts to restrict Osmanagić from excavating at the base of Visočica Hill (Pyramid of the Sun) or the nearby Plješevica Hill (Pyramid of the Moon) ultimately failed. This has resulted in one professional team excavating on the top of one hill, and one amateur pyramid team excavating at the bottom of the same hill—neither team communicating, hardly acknowledging one another. Osmanagić's project is still by far more popular, more supported, and holds more authority than the professional project in the eyes of the general public, despite the fact that the medieval fort played a critical role in Bosnian national history, as once the seat of the Bosnian independent medieval kingdom (Malcolm 2002).

It is constructive to contrast this post-war state of affairs in Bosnia with a nearly-identical pre-war case of pseudoarchaeology, which started like the pyramid project but had a different outcome. In the 1980s, a Mexican hotel owner named Salinas Price announced that he had found evidence that Homeric Troy was located in the Bosnian town of Gabela, in the Neretva River valley (Stultitia 2007). At that time, Bosnian archaeologists exercised their authority to stop the pseudoarchaeological dig, making sure that Price could not get excavation permits (Kampschror 2006: 26). The state of affairs is considerably different now in post-war Bosnia, where any person can take action on his pseudoarchaeological claims due to political instability. Enver Imamovic, an archaeologist at Sarajevo University and former director of the National Museum of Sarajevo, thinks "our system is to blame, our institutions, which are not doing anything" (Harris 2006). Bruce Hitchner, professor at Tufts, thinks that "the scam is made possible by the lack of effective central authority" and that Osmanagić has "exploited that weakness" (Kampschror 2006: 27).

5.3.7 Socio-Politics as Integral to Scientific Authority

I would argue that Osmanagić has indeed exploited the weakness of an unstable country, by gathering momentum through political support, and by using his own networks and connections in government (for example, his father was once the Secretary for Industry, Energy and Trade in the former Republic of Bosnia). But it is also equally true that Osmanagić and his pyramid project has also been exploited by that very same system. International professional academics have responded to the project as if it was a top-down program directed by a maverick, whose claims to authority can be snuffed out by appropriate rational and empirical arguments. In reality, the project's authority is much more complex. The success of the project has resulted from material desires and material results, some of which have been driven by Osmanagić himself, but many others which have been actively performed into existence through the participation of an audience eager for a material symbol of economic success and nationalism. They are translating the project into something that goes beyond archaeology—a tangible symbol of nationalism and money.

This project is an economic and social asset to different groups in Bosnia, and the project is deeply ingrained in national and ethnic Bosnian history. Eric Hobsbawm writes:

‘Invented traditions’ have significant social and political functions, and would neither come into existence nor establish themselves if they could not acquire them...the most successful examples of manipulation are those which exploit practices which clearly meet a felt—not necessarily a clearly understood—need among particular bodies of people. (1983a: 307)

Such a *need* for pyramids clearly exhibits itself at Visoko: the pyramid site satisfies specific socio-political needs. It offers a world-class monument that outstands every other major national monument in the world, right there in “little Bosnia.” It offers politicians a diversion from unstable government problems and offers a campaign strategy. It gives a war-struck town a thriving economic boost. In short, it fulfils serious social needs. For many members of the public and politicians, the question isn't whether or not the pyramids are real, but rather if people will come to see it, spend money in the tourist shops, and use it as a cultural and economic artefact. For others the site's very existence questions fundamental ideas about government, control and academic authority.

Archaeologists who have been desperately trying to ‘knock sense’ into people about the true nature of the site have seemed to be unmindful of these issues. Telling a supporter that their pyramids don't exist is futile when people are *praying* for the site to

be found: Visoko local Rasim Kilalic, who turned his weekend home into a café, said “Please God, let them find a pyramid,” [while] rushing to serve crowded tables” (quoted in Sito-Sucic 2006). Kilalic and those like him are not concerned with arguments about what ‘is’ or ‘is not’ authentic archaeology. When people feel it necessary to pray for pyramids, when they have a stake in making sure the notion of pyramids survives, then there are larger considerations in play than unerring fidelity to ontological truth. Such active, participatory inventing is exemplified in one quote by a local Visoko resident: “If they don’t find the pyramid, we’re going to make it during the night. But we’re not even thinking about that. There *are* pyramids and there *will be* pyramids” (quoted in Foer 2007). This is exactly what the participating public, media and Osmanagić are doing: *constructing* pyramids through their participation. Osmanagić is only able to invent his heritage and sustain his authority through the continued participation from a supportive audience that allows his ideas to gain momentum and security. The site and members of the Foundation—particularly Osmanagić—have been crafting a complex performance of executive and epistemic authority through the use of institutions and expertise.

5.4 Performing Science: Gaining Authority Through Appropriate Performance

5.4.1 Making Realities: Authority Created in the Bosnian Pyramid Project

John Law writes that, “The practices of science make relations, but as they make relations *they also make realities*” (Law 2004: 29). Here, Law is referring to the fact that facts are created through the practice of science, and that facts are by definition: “Something that has really occurred or is actually the case...a particular truth known by actual observation or authentic testimony, as opposed to what is merely inferred, or to a conjecture or fiction” (OED 1989). The key concept here is that of *authenticity* in observation and testimony, a reliance of representation on ontological truth, which raises questions about the nature of epistemic authority. In a discipline like archaeology, what makes an account of the past authentic or faithful to what actually happened in the past? How do you begin to classify experiences, observation and testimony into categories of the ‘actual’ and ‘authentic’? How does this play into the scientific methods of ‘fact-finding’, excavation and the publication of archaeological knowledge?

In archaeology, facts are created through the interactive process of excavating, post-excavation recording, publishing and display. Actors create categories in the process of ‘doing archaeology’, but the process itself can also create actors and categories. This is the ‘factual construction of social agents’ whereby, for example, an untrained student who goes on fieldwork *becomes an archaeologist* through the act of excavating (Van Reybrouck and Jacobs 2006). A student gains status as an archaeologist through his appropriate behaviour and performance, and he accumulates authority by performing appropriate actions in the category of ‘archaeology’. In such cases, the performative aspect of what it means ‘to do’ science and ‘to be/become’ a scientist—at least in terms of the authority of appearing so—can be almost as important as the validation of data. Facts are constructed equally through the *performance* of authentic observation and testimony, as they are in the politics of category-making and meaning making. This section offers a discussion on the performative aspects of authority in the production of knowledge, highlighting how the performance of scientific practices can construct powerful new realities.

5.4.2 Actualities and Virtualities

In studying how nonexistent material can become an extant ‘reality’ for so many people in Bosnia, it is useful to explore what might be theoretically framed ‘actualities’ and ‘virtualities’. In “Theorizing Heritage” (1995), Barbara Kirschenblatt-Gimblett retells a story of a travel writer who visited the historic site of Cluny church in France:

Last year 700,000 tourists came to see Cluny and the church that isn’t there... A museum dedicated to the church stands a few feet away from the excavation. Inside, I look at an animated, three-dimensional computer re-creation...Back outside, I stare at the void. The computer model is still so fresh in my mind that an image of the enormous edifice seems to appear before me. I’m not alone in this optical illusion: everyone else leaving the museum seems to do the same double take outside. It’s as if we’re having a mass hallucination of a building that no longer exists. (quoted in Kirschenblatt-Gimblett 1995: 15)

Kirschenblatt-Gimblett offers this example as “virtualities in the absence of actualities. It produces hallucinatory effects. On the basis of excavation and historical reconstruction and in collaboration with visitors, the museum openly imagines the site into being—in the very spot where it should be still standing but is no more” (1995: 377). The museum has a mediating effect which (re)invents a virtual site, where “we travel to actual destinations to experience virtual places” (1995: 377).

The Cluny church and the Bosnian Pyramids share a common feature: the ‘inventing of a site through the blurring of what Kirschenblatt-Gimblett terms

“actualities” and “virtualities” (1995: 375). In the pyramid case, media communication (using language, images, and a combination of performance and participation) acts as a medium in which Semir Osmanagić and others collectively create the pyramids. The notion that the ‘virtual’ is opposed to the ‘actual,’ and the idea that the two can become blurred or that the former can replace the latter, is not new in literature. Eric Hobsbawm, for example, argues that there is an underlying and genuine custom in which traditions come to be invented and then exist (1983a: 2). Scottish kilts, for instance, were largely artificial traditions that later merged with and ‘became’ Scottish custom (Trevor-Roper 1983), and many nationalistic traditions, such as national holidays and festivals, were mass-invented in state-led generations in Europe between 1870-1914 (Hobsbawm 1983b). These invented traditions were in a sense ‘virtualities’ that became ‘actualities’ in pre-existing custom.

Jean Baudrillard goes further with this notion of the ‘virtual’ as opposed to the ‘actual’ in his philosophical work *Simulations* (1988). Baudrillard specifically discusses ‘simulacrum’, a Latin word that essentially means “to put on an appearance of”. According to traditional philosophers like Plato and Nietzsche, a simulacrum is an unsatisfactory reproduction of something existing in reality, something like a Roman copy of an original Greek statue (Nietzsche 1990; Plato 2004). However, Baudrillard departs from Plato and Nietzsche, arguing that a simulacrum is not a copy of the real, but rather something virtual that becomes truth or replaces truth in its own right, something that is ‘hyperreal’ (Baudrillard 1988). The ‘hyperreal’ characterizes the inability to distinguish between the ‘actual’ and the ‘virtual’. For example, if media radically shapes and filters an event and a viewer’s reality becomes enmeshed in both facts and invented/altered information, then his reality is ‘hyperreal’.

This discourse of ‘simulacrum’, and the ‘actual’ and the ‘virtual’, is a useful lens to view the way pyramids are being constructed at Visoko. Kirschenblatt-Gimblett’s Cluny church “hallucinations” and Semir Osmanagić’s pyramids can be seen as cases of ‘simulacrum’, where ‘virtual’ imaginings are created through a mediating factor (the museum is mediating reality in Cluny, and various media sources mediate reality in Bosnia). In both cases, viewers experience the ‘hyperreal’, where imagined understandings of history merge with an ‘actual’ site in reality. The Bosnian pyramids do not exist as Semir Osmanagić and his followers say they do. The hills are simple geological formations, and no matter how hard Osmanagić may search, he will not produce real evidence of a supercivilisation. One can distinguish the ‘actual’ from the ‘virtual’ at Visoko, just like visitors to the Lascaux Caves in France “could easily be made to understand how they, let alone an art historian, can tell the difference between the

real and a fake” (Butler 2002: 114). Osmanagić, however, does claim that pyramids exist at Visoko, he performs science as if he is in the act of uncovering them, and he has more or less devout followers who support his project, acknowledge his epistemic authority and claim to see what he sees.

This situation, I argue, is occurring because Osmanagić is successfully creating a simulacrum of the site and performing a hyperreal history, primarily by using authoritative mass media outlets as the medium to disseminate his ideas [Figure 23]. Osmanagić is presenting a ‘virtual’ (irrational and invented) image of ancient pyramids through various communication networks, in the same way that the museum at Cluny provides a ‘virtual’ (rationally argued for) image of the inexistent Cluny church. The major distinction is not in how these two images are presented, performed or in the ostensible authoritative support behind their claims. Rather, the distinction rests on the fact that the church at Cluny actually existed in the past and there is ontological evidence behind this reality, and the Bosnian Pyramids did not exist in any ontological sense outside of a hyperreality based on smoke and mirrors. This process of performative inventing, the importance of hyperreality as a means to authority, and the questions that these concepts raise are expanded upon further in the next section.



Figure 23: The Pyramid Project is a performance, and Semir Osmanagić is in the spotlight.
Photos by Tera Pruitt.

5.4.3 Method to the Madness: Inventing Authority through Performance and Media

In 2006, the television station ABC Houston 13 broadcast a special story about Osmanagić and his pyramids.⁶⁴ This story exemplifies how Osmanagić's performance and his use of communication networks construct and authorise the idea of ancient pyramids by creating the idea, or the simulacrum, of pyramids:

[Image: logo brand of a pyramid with the words: "Houston's Indiana Jones"]

DESK ANCHOR: Travel to Bosnia to follow this modern day Indiana Jones and his search for Bosnia's great valley of pyramids.

[Footage of Semir Osmanagić walking at the Pyramid of the Sun, wearing a khaki shirt and trousers and an Indiana-Jones style hat]

OSMANAGIĆ: You are enjoying the most beautiful place on the planet.

ANCHOR: You don't know Semir Osmanagić, but to the people of Bosnia, he is a national hero. *[Cut to a scene with school children clapping for him].* Congratulated, applauded, and loved wherever he goes. *[Cut to scene of more children presenting Osmanagić a pyramid-shaped cake].* This is a land which has been torn by war and civil conflict, but resurrected in a way by one man [...] Indeed, his story, if true, could change the history of the world.

OSMANAGIĆ: *[walking at the Pyramid of the Sun; where the site appears to be excavated professionally]* We are going back thousands of years from the ancient times and the Roman and the Greek.

ANCHOR: As a history buff, a sort of living Indiana Jones, he travels the world, exploring mysteries [...]

OSMANAGIĆ: All you need to do is disregard the trees, the greenery, the soil, and you will see the object, clearly in your mind. [...]

ANCHOR: Semir used satellite, thermal, and topography analysis on tens of thousands of hills in his search for pyramids [...] If a person could look back and just visualize this place as you see it, eight thousand, ten thousand years ago, they would see a massive stone city.

OSMANAGIĆ: What they would see would be the most magnificent city ever built on the face of the planet. (ABC 2006)

The transcript above vividly illustrates how Osmanagić and his supportive media have performed a 'virtual' pyramid site onto the landscape in Visoko: the story invites the viewer to "disregard" the site as it stands today, consider the work Osmanagić has done, and "visualize" a "magnificent city". This evocation of simulacra—images not only of that city, but of the genuine scientifically accredited archaeological project that found the city—occurs in a number of ways, elaborated further in the sections below. The first is

⁶⁴ I find this example of the ABC 13 broadcast particularly appropriate, since Semir Osmanagić has often played this same media clip during many of his own public presentations (notably, his presentation at the Bosnian Embassy in London in 2007, and at the ICBP Conference in 2008). The fact that the Bosnian Pyramids have made it onto the well-known American network ABC has often been leveraged for authority and legitimation.

Osmanagić's self-representation: language and images provoking associations with pop-cultural icons. The second is Osmanagić's deliberate narrative establishment of a villain (mainstream archaeologists and political opponents) that helps to root the pyramid story as a cause 'for good'. The third is through the Foundation's penchant for logos and branding, rooted in modern 'pop culture' and stereotypes, and which actively establish the project. The last is the performance of 'doing science' and the creation of an appearance of methodology through the appropriation of scientific manners, outsourcing of genuine scientific results, and the mimicking of scientific documents and utilising the rhythm of scientific language.

5.4.3.1 Self-Representation: Icons and Personalities

In his work, Osmanagić references several specific icons of self-representation that lend authority to his own image as an expert on the past (c.f. Holtorf and Drew 2007). First and foremost, Osmanagić represents himself as an adventurer. Osmanagić builds on a prevalent archaeological icon from media and literature: the khaki-wearing adventurer, who knows that "anyone is capable of discovery and the non-professional may participate in the grand adventure" (Ascher 1960: 402). Osmanagić fully endorses this image, always wearing rugged khaki and rarely appearing in public without his wide-brimmed Indiana Jones-style fedora. [Figure 24] Osmanagić describes his work with adjectives like 'dangerous', 'brave', 'exotic', and 'mysterious'. His tone is dramatic, alluding to 'secrets', 'mysteries' or 'treasures' of the past. The ABC Houston transcript above, for example, claims that he is a "living Indiana Jones, he travels the world, exploring mysteries" (ABC 2006).

Osmanagić has offset this adventurous image with two perhaps contradictory self-representations: the hardworking academic and the cool socialite. He asserts that his time is dedicated "to the intensive research of certain enigmas of the past" involving cultures such as the Maya, Assay, and pre-Illyric cultures in Bosnia (BosnianPyramids.org 2006). He claims he has "read 40-50 books a year" (BosnianPyramids.org 2006). On many occasions he has emphasized that the Foundation has dedicated over "300,000 man hours" to the pursuit of evidence, many of which are presumably his own (Osmanagić, personal email communication 2008). Somewhat paradoxically, Osmanagić has also been initiated into the artsy, 'just plain cool' side of popular culture. His excavations have been launched with concerts of popular rock groups and pyramid-themed art installations. He has even appeared in a music video (Harris 2006; Dedic 2007). In interviews, members of the public who have

watched Osmanagić on TV have told me that they see him as a “famous” person and a “celebrity” who has charismatic authority because he is so present in popular culture (Sarajevo residents, personal communication 2010).

Osmanagić also represents himself as a hero-crusader on a quest for truth, attempting to save a war-torn land. The ABC show above, for example, explicitly calls him a “national hero” who will “resurrect” a war-torn country (ABC 2006). The humble public servant image is not far behind. In one interview, Osmanagić recognizes that he is in the spotlight of his project, but says “affirmation of the project on the world wide scene and of course the contact with the media, are all a part of this process. However I will slowly move away from the center of the attention as more people get involved in various activities” (BosnianPyramids.org 2006). Osmanagić’s image as the modest public servant and dedicated martyr coexist in statements like: “I was aware the in this initial period there would be critics who will publicly or privately, speak out, insult and challenge this vision. That is why I did not want to put anyone else forward, but instead I answered to all provocations with the culture of dialogue and scientific arguments” (BosnianPyramids.org 2006).

With these various and often conflicting personalities, it is perhaps surprising that Osmanagić has achieved such a successful authoritative media image. But he has, for two reasons: first, these images are stereotypes, seemingly drawn from a collective understanding of what is to be an archaeologist (from pop-cultural icons like Indiana Jones, to academic notions of public servitude and intensive research) (Holtorf and Drew 2007). The second reason is that he establishes one solitary opposite force: the villain. Osmanagić creates a solid base for his own authority by juxtaposing his various self-images against one antagonist.

5.4.3.2 Narration of Villain

Garret Fagan writes of pseudoarchaeology, “There is another powerful storytelling feature in this genre, one usually lacking in good archaeological television: a villain. For many pseudoarchaeology shows, the villain is archaeology itself” (Fagan 2003). Vilification “is a kind of symbol-making that groups engage in under certain conditions in order to...build consensus and morale for certain kinds of social actions” (Klapp 1959: 71). Osmanagić has successfully established mainstream archaeologists as the primary villain to his cause. It is through this move of opposition that he has been able to maintain his own narrative.

Like a classic hero, Osmanagić has consistently kept up a performance of ‘good guy’ versus ‘bad guy’ with the academic establishment, saying that “every new idea has opponents in the beginning. The bigger the idea, more aggressive the opponents [*sic*]. But, it does not influence my goals and determination for an inch” (Osmanagic 2007c). Osmanagić has used the instability of the post-war academic establishment to his advantage, saying that archaeologists are incompetent and lax in their work (BosnianPyramids.org 2006). Osmanagić has also accused Bosnian archaeologists of “longtime carelessness” [*sic*] and cites foreign scholars as “clueless about the real situation and state of Bosnian Cultural Heritage” (BosnianPyramids.org 2006).

Osmanagić has represented mainstream academics as insulting, fearful groups who conspire to attack his higher truth. On one website, Osmanagić has directly politicised and polarised his academic opponents: “convinced about their conservative views, [they] promptly attacked the hypothesis and tried to debunk it’s author. Some of them, showed a typical bosnian [*sic*] propensity, by launching labels and insults from behind the scenes” (Osmanagic 2006). He has also used forceful language to depict mainstream scientists as afraid, jealous and small-minded: “Are they afraid about the material evidence that will make collapse their world views?” [*sic*] (Osmanagic 2006); “The trades like geology and archaeology will be the last to accept [the pyramids], because it’s a revolution” (quoted in Foer 2007). Like every good crusader and public servant, Osmanagić refers to his opponents in a tone of ‘humble citizen’ versus the ‘corrupt establishment,’ conjuring a crusader image of fighting for truth against all odds.

A prime example of such behaviour is a letter that Osmanagić addresses to “Professors, Museum Councilors [*sic*], Member of Federal Committees and Journalists” (Osmanagic 2006). The letter explicitly entreats academics to help a cause that will improve the country, a cause that intends to give sublime hope and goodness to the world and will stand (and has already stood) the ‘tests of time’. However, the letter seems to imply that the antagonistic archaeologists are endangering a ‘good’ cause that represents an ‘underdog’ country, trying to disunite ethnic groups and take sides, and fighting economic growth and development in the country:

The pyramids will survive all of us. In One Hundred Years, nobody will remember our names. But, those collassal [*sic*] stone structures, located in the small, but proud country called Bosnia, will radiate a positive energy out into the world. Please, let me invite you once again to unite the modest Bosnian potentials...In five years, one million of tourists [*sic*] will visit the Bosnian Valley of Pyramids. Our wish is that Bosnia and Herzegowina [*sic*] becomes a lively place where explorers, students, professors, volunteers of lightened faces exchange their international scientific knowledge. Tourism will develop the market, the economy will raise and infrastructures will be built. (Osmanagic 2006)

While drafted as an open letter to opposing archaeologists, this document actually appears on a fanatically supportive public website that mainly draws advocates who are looking for confirmation of the pyramids (Bosnian-pyramid.net 2006, poll data). The letter, therefore, is not really directed at the indicated professionals, but rather toward a supportive general audience. The actual intended reaction, it can be assumed, is not to convert the putative addressees. Rather, Osmanagić seeks to make his general public audience see the great benefit of the project and to collectively rally against the dispassionate and antagonistic academics. As propaganda, it does a great deal to reduce the authority of mainstream scientists while simultaneously elevating Osmanagić's own authority.

5.4.3.3 Drawing on Institutions, Logos and Branding

Osmanagić creates the image of a villainous establishment of scientists, with professional archaeology being a small-minded enterprise. However, he simultaneously uses the authority of logos and branding, drawing on scientific institutions when it suits his own means to an authoritative image. He does this in several ways, from the promotion of cultural assumptions about foreign academia, to the use of brand names and signage. He uses media, which by nature, “[enable] marketers to project brands into national consciousness” (Muniz and O’Guinn 2001: 413). For example, Osmanagić never fails to mention that he has been living and working in Houston, Texas. According to some Bosnians, living and working abroad (especially in places like the United States or the European Union) is considered an attractive and authoritative feat in its own right (Sarajevo resident, personal communication 2007). Along with his American label, Osmanagić builds his self-image on prevalent pop-cultural icons. His “sort of modern-day Indiana Jones” image is his own personal logo (ABC 2006). Headlines brand him as “Bosnia’s Indiana Jones,” “Houston’s Indiana Jones,” or “Indiana Jones of the Balkans” (ABC 2006; Hawton 2006). This self-branding provides enough drama and assumption to give Osmanagić a look of amateur authority, and he is an easily recognisable celebrity icon in media contexts. [Figure 24]



Figure 24: For years, Semir Osmanagić rarely appeared in public without wearing his signature, iconic hat.

As well as branding himself, Osmanagić also seizes every opportunity to promote other people with official political labels or degrees behind their name. Along with encouraging national political sponsorship and his own Foundation supporters, Osmanagić courts international professors or students who give his project an appearance of authoritative, scientific presence (see Section 5.3.5 on experts and expertise, above). At the excavation sites, this courtship is full of friendliness and hospitality. However, casual visits by curious academic professionals have more than once been later spun as support for the project's authority, when in reality, no such support existed. For example, in July 2010, Dr. Ezra Zubrow from the University of Buffalo SUNY travelled through Sarajevo and saw authoritative-looking blurbs about 'archaeology in Visoko' listed in tourist brochures. Unaware of the site's academic controversy and project's lack of peer review, Zubrow visited Visoko. Within a short span of time, he found himself at the centre of attention, surrounded and courted by Osmanagić, cameras and other team members. When a video camera appeared at lunch, he jovially made comments about how archaeological sites should go on "unfettered" by politics. He left Visoko without having seen much of the site, and with the impression that Visoko was full of hospitable local people. Later, he was surprised to read news headlines that boldly stated: "U.S. Professor Gives Thumbs Up To Bosnian Pyramid Find" (Osmanagic 2010). Zubrow felt that his visit was grossly misinterpreted and manipulated to read as 'expert consensus' and 'proof' of pyramids (Zubrow, personal communication 2010).

In another instance, Dr. Robert Schoch, a controversial academic in his own right from the University of Boston, travelled with Dr. Colette Dowell to the Bosnian Pyramid site to see what the fuss was about. They were both courted and then manipulated for

press interest by Semir Osmanagić and members of the Foundation. Dowell narrates the event:

Television, news papers and websites...announced our arrival in Bosnia as the "American Superstars," who would credit the claims of Semir's pyramids and Bosnia would receive its glory. It was a terrible position for us to be placed in. Semir would make a point of introducing us to investors and politicians and have us all stand around posing together for our pictures. (Dowell 2007)

Another example of the Pyramid Project's fondness for authoritative labels has manifested during Semir Osmanagić's public presentations. For example, at the Bosnian Embassy in London in 2007, Osmanagić opened his lecture by saying that his "excavation team includes an Oxford university archaeologist" (Bohannon 2006b; Osmanagic 2007a). Osmanagić showed a brief video clip of a young man at the Pyramid of the Moon stating that he felt "convinced that there's certainly some kind of large-scale man-made structure" (Bosnianpyramid.com 2006). Peter Mitchell, an Oxford archaeologist, told *Science Magazine* that the boy in the video was only an undergraduate student and "does not have any expertise and in no way represents the university" (Bohannon 2006b). Nevertheless, months after the event, Osmanagić continued to promote this 'Oxford archaeologist' video on his website, undoubtedly because of the weight the 'Oxford' name carries.

The Bosnian Pyramid project has also drawn heavily on the names of policy institutions to gain and sustain the project's authority. Along with the links made to the University of Oxford, the project has also made more substantial links to the Library of Alexandria in Egypt, the Russian Academy of Natural Sciences and—notably—UNESCO and the World Heritage List. In an article headlined "Alexandrian Archaeologists Impressed By The Scientific Approach Of The Bosnian Pyramids Research", the Foundation describes how the president of the University of Alexandria "expressed his willingness to closely cooperate with the Foundation in the future" and how "[a]fter the successful presentation, Osmanagić as offered a membership to this prestigious institution which he accepted with much pleasure" (The Archaeological Park: Bosnian Pyramid of the Sun Foundation 2009). Both accounts are true: the Egyptian group did induct Osmanagić as a member. The group members such as Dr. Nebil Swelim and Prof. Monna Haggag, who support the project for deeply personal Islamic and socio-political reasons.

Osmanagić has similarly been inducted in the Russian Academy of Natural Sciences. This organisation is not the same as the famous Russian Academy of Sciences (which is limited in number to 500 full members, including multiple Nobel Laureates); it is entirely independent. Osmanagić gained his induction through Dr. Oleg B.

Khavroshkin, a geophysicist from the Schmidt Institute in Moscow and member of the Russian Academy of Natural Sciences. Khavroshkin also spoke at the ICBP conference. His high-profile name and scientific-appearing presentations quickly led him to be very much relied and drawn upon during the ICBP conference. As a member of audience, I watched Dr. Khavroshkin present on the “Seismic-Physical Structural Model Of Pyramids”, which included opaque PowerPoint slides full of seemingly meaningless formulas and diagrams [Figure 25] (Khavroshkin and Tsyplakov 2008). Dr. Khavroshkin presented geophysical results from tests he had taken at the Bosnian Pyramids site; however, he used his conference presentation time to drift off topic and bolster his claims that life on Earth is extraterrestrial in origin (ICBP conference 2008). Dr. Khavroshkin’s actual ‘scientific contribution’ to the pyramid project bordered on the nonsensical; however, his name, degrees and institutional background lent the appearance of a supportive “scientific heavyweight” (Coppens 2008b). Osmanagić has drawn authority for himself and his project from such experts who support him politically and socially, who have been able to induct him into establishments with names like the “Russian Academy of Natural Science” and the “Library of Alexandria”, which sound weighty and foreign.

Semir Osmanagić and the Foundation have also drawn on the brand and the authority of the United Nations and UNESCO, simply through a discussion and promotion of the UNESCO World Heritage List as an eventual aim of their pyramid tourism plan. From the project’s inception, they have explicitly aimed to “install a plaque declaring the site a UNESCO World Heritage Site” (Piramidascunca.ba 2006; Wikipedia 2010). In 2006, members of the professional community wrote a petition to UNESCO, signed by a large number of academics with doctorates and positions at authoritative establishments. The petition argued that Osmanagić’s project should be halted and not seriously considered by UNESCO (Archaeology.org 2006). In response, UNESCO officials released an official statement saying that they did not intend to send a mission to Visoko (Woodard 2007b). Political supporters in Bosnia were unmoved, and the project continued to endorse its UNESCO World Heritage List hopes to the public as their vision of a way to get ‘little Bosnia’ on the map. In June of 2010, the Bosnian Pyramid Foundation released an article headlining: “Bosnian Pyramids in United Nations”. This article states that the United Nations held the Ninth Permanent Forum on Indigenous Issues on 16 June 2010 in New York, and that during this session, one member of a non-governmental organisation (called the Ecospirituality Foundation from Italy) urged for a number of European sites to be protected by the UN. The Bosnian pyramids was included in their list of sites (Piramidasunca.ba 2010). This very weak connection

between the UN and the Bosnian Pyramids is apparent. However, the headline's unabashed connection of the pyramid project to the United Nations is enough to lend weight and status to the Bosnian Pyramids, through the simple and sustained mention of an institution as powerful as the UN.

Finally, there is authority stemming from modern concepts of using logos and establishing brand identity. At the most obvious level, Osmanagić's penchant for logos and brand names appears in the way he has trademarked the Foundation: a shiny, official-looking logo that directly references the power of government [Figure 17]. In 2006, he successfully trademarked the individual names of the pyramids and 'The Archaeological Park: Bosnian Pyramid of the Sun Foundation' (Schoch 2007). In Visoko, official government signs point toward the pyramids, and an array of formal professionally manufactured Foundation signage mark the site [Figure 26]. This obsession with logos and branding creates the feeling of establishment and authority, a point that also emerges in the way Osmanagić tries to represent the site as 'scientific'. This point is expanded in the next section.



Figure 25: A sample slide from the PowerPoint lecture of Dr. Oleg B. Khavroshkin, titled 'Seismic-Physical Structural Model of Pyramids' (Khavroshkin and Tsyplakov 2008).



Figure 26: Example of the authoritative, professional-looking signage that marks the Bosnian Pyramid excavation sites. The red signs with the official Foundation logo give tourists interpretive information. This photo also shows a professional Foundation poster advertisement (hanging below the red sign) which advertises the upcoming 1st International Conference of the Bosnian Pyramids (ICBP). Photo by Tera Pruitt.

5.4.3.4 Scientific Representation

In his self-representation, Osmanagić has moved seamlessly from performing as a 'modest people's adventurer who despises elite academics', to the completely contradictory performance of 'visionary amateur scientist who leads a team of elite experts and carries out intensive scientific analyses'. Historically, Osmanagić has carefully manipulated images and language so that his methods appear scientific, while actually having no basis in real evidence or accepted methodology.

Osmanagić has always argued that he has conducted serious academic work dedicated "to the intensive research of certain enigmas of the past" involving cultures such as the Maya, the Assay, and the pre-Illyric cultures in Bosnia (Bosnian Pyramids.org 2006). He continues to stress that his research in Visoko is a controlled and extensive scientific experiment. In 2007, he released a document called *Scientific Evidence about the Existence of Bosnian Pyramids* [see Appendix H], which states:

Discovery of Bosnian Pyramids was not simply an ad-hoc affair, but required combination [*sic*] of classic geo-archaeological methods with modern geophysical and remote sensing technologies.

The Archaeological Park Foundation believes that only a multi-disciplinary approach, with serious scientific argumentation on internationally recognized level [*sic*] will yield a successful realization of the Bosnian Pyramids project.

The team, therefore, includes not only archaeologists, but also geologists (mineralogists/petrologists, hydrologists and sedimentologists), geophysicists, paleontologists, speleologists, anthropologists, mining engineers as well as anthropologists. Each one of these experts brings a new element of problem understanding and integrate their qualifications and expertise into the project with a great enthusiasm and collegiality. (Osmanagic 2007b: 1) [See Appendix H]

Such language intentionally connects his project to mainstream scientific work and methodologies. Consider the language used in this example article in the 2004 Çatalhöyük Archive Report:

[The project] aims to understand this sequence at a landscape scale through multi-disciplinary research that includes fieldwalking, surface collection, survey, excavation, archaeobotany, archaeozoology, ceramic analysis, geomorphology, micromorphology and soil science. (Mills 2004)

Despite the similarities in language, considerable differences exist between the professional work done by archaeologists like Steven Mills at Çatalhöyük and the claims made by Osmanagić in his scientific report.

While Osmanagić's language intentionally connects his project to mainstream scientific work and methodologies, none of his statements (including his long list of team experts) are ever documented or supported with any real evidence. His scientific reports usually have short paragraph entries with intricate titles such as "Apparent thermal inertia measurements" or "Geodetic topographic contour analyses". His data, however, usually boil down to nothing but simple statements that "geospatial anomalies" exist (Osmanagic 2007b: 2) or only reveal vague generalizations, such as "the sides of Visočica/Bosnian Pyramid of Sun are exactly aligned with the cardinal sides of the world (north-south, east-west), which is one of the characteristics often noted with the existing pyramids" (2007b: 3). These 'data' entries each have corresponding images, which at first glance appear to be technical and evidentiary; but on closer inspection, the images and their accompanying legends are meaningless. [Figure 27]

These reports vividly show that *what* Osmanagić says is less important than *how* he says things. The reports mimic language patterns of professional archaeological documents, drawing on the established institution of science, creating a tone of authority. This tone, coupled with colourful, technical images give the project a feeling of weight and worth. The official Foundation website and logos are formatted to appear formal and official, yet inviting and inclusive for a wider public. In this case, Osmanagić and his team are, through mimicry, performing authority. The next section in this

chapter expands on this point by discussing how the Bosnian Pyramid project deliberately connects to science as a master discourse.

5.5 Authority from Science as a Master Discourse

5.5.1 Drawing on Science

Historically, most of the academic debate in this case study has revolved around what material evidence has (or has not) been found by the pyramid team, arguing for or against the validity of Osmanagić's grand interpretations about the 'greatest civilisation in the world'. As discussed in the section above, most of Osmanagić's scientific documents engage in mimicry of scientific methods, with little meaning or message behind their presentation. However, the story is yet more complicated, as Osmanagić and his team have proven themselves to be adept at constructing an 'authoritative' presence, and have constructed accounts of the past that have been received as 'authoritative' by many in the Bosnian public. A main reason behind this success and authority, I would argue, is drawn from their use of genuine scientific methods, in activity that I call an "outsourcing of scientific ethics" (see Section 5.5.4, below). In some instances, such as the use of radiocarbon data and testing, the team have accurately sampled and received results from prestigious labs.

Osmanagić's team uses accredited professionals to take samples of genuine organic material, sends them off to get tested by accredited laboratories, and gets accredited persons to present accurate results. But they do this activity based on inaccurate assumptions about the source material, and they draw illogical interpretations from the results. By relying on credible scientific sources and discourses, the team has outsourced its own accountability and authority: it has used a sprinkling of 'scientific' data based in fact, but has ultimately taken this data out of context to yield outlandish interpretations. This translation creates a complex web of performance, authority and accountability. The following section explains this practice and performance in more detail, focusing on the radiocarbon dating data presented at the 1st International Scientific Conference of the Bosnian Pyramids (ICBP).

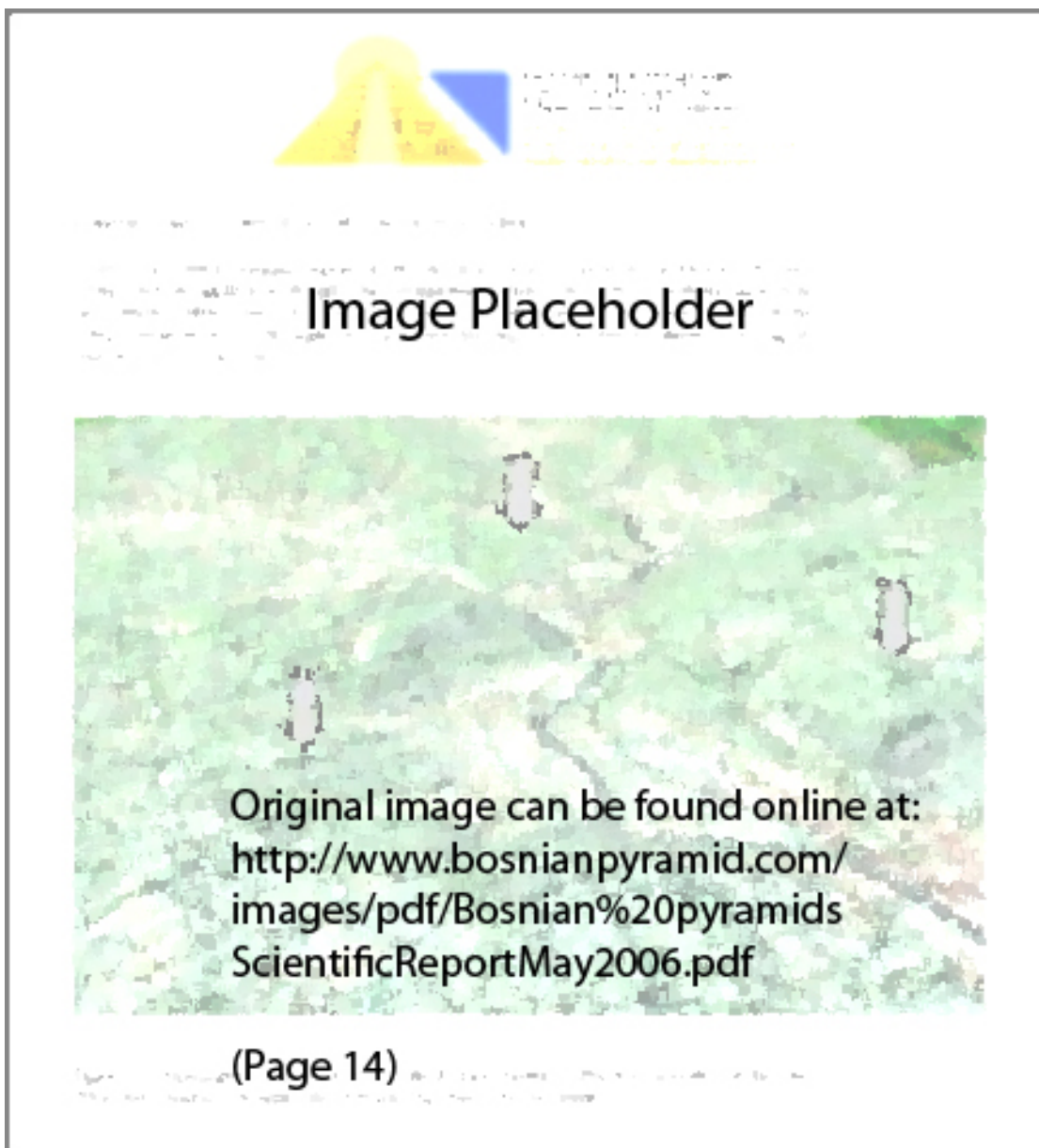


Figure 27: This is a sample page from Osmanagić's *Scientific Evidence about the Existence of Bosnian Pyramids* report. Three arbitrary arrows and scientific jargon on a topographic map are supposed to represent 'data', but when examined closely, they are empirically meaningless (Osmanagic 2007b: 14).

5.5.2 The Example of Radiocarbon Dating of the Bosnian Pyramids

In November 2008, the Bosnian Pyramid of the Sun Foundation announced in an online article that:

The first radio-carbon [*sic*] analysis of the organic material found above the megalithic blocks (within the conglomerate) revealed sensational results: The blocks with engraved symbols have been covered for more than 30,000 years! These analyses coincide with the ancient paintings in Northern Spain and South France. (Lascaux; 32,000 years). (The Archaeological Park: Bosnian Pyramid of the Sun Foundation 2008)

Next to the article is a photo of archaeologist Andrew Lawler,⁶⁵ wearing a hard hat, taking samples of organic material from the wall of one of the tunnels. This 'final product' report states in no uncertain terms that "engraved symbols" in these 'pyramid tunnels' were dated to 30,000 BP.⁶⁶

The importance of the radiocarbon sample is due to two major events: (1) this was the first organic material the project had come across that could qualify for radiocarbon testing, and (2) the piece of wood was found embedded in conglomerates inside one of the tunnels, only a few metres from a rock the team called the "T1 Megalith". Dr. Muris Osmanagić (Semir Osmanagić's father and a mining expert) has controversially claimed that this 'megalith' is engraved with 'proto-Bosnian script'. These carvings on the large rock in the tunnel have a dubious history. Multiple people assert that they saw the rock on earlier occasions without the 'script' carvings on it (Dowell 2008). This controversy sets up a dualistic scenario for the project: if the rock was previously observed without scripts on it, then the rock (and potentially the authority of their whole narrative) is a clear hoax created by the project or an enthusiastic supporter. But if the scripts are genuine, then the project could try to argue for 'ancient' human activity in the tunnels. Despite the controversy, the Foundation proceeded to do radiocarbon sampling on the assumption that the scripts were ancient. They argued that the organic material they found was encased by the conglomerates covering the 'T1 Megalith'. Therefore, if radiocarbon dated, this organic material in would give an accurate date of the 'megalith' sealed by the conglomerate, indicating the years of 'pyramid activity' (Irna 2008c; Lawler 2008).

⁶⁵ Lawler holds a B.A. in archaeology from the University of Cambridge. See Section 5.3.5.

⁶⁶ Later, the Foundation even published a 'guide to understanding radiocarbon dating' for the public on their official website to further the apparent transparency and importance of the radiocarbon dating process (Smart 2009).

Archaeologist Andrew Lawler, while he was still working for the Foundation, took the first samples of organic material and sent it to two radiocarbon laboratories: (1) the Research Laboratory for Archaeology and the History of Art at the University of Oxford in the United Kingdom and (2) Leibniz-Laboratory for Radiometric Dating and Stable Isotope Research at Christian-Albrechts University in Kiel, Germany. Another sample (3) was taken by the untrained Dr. Muris Osmanagić (PhD in Mining) and was later sent to the radiocarbon laboratory at the Silesian University of Technology in Gliwice, Poland. Oxford refused to return a result on the sample. In their report, they state:

The small graphite sample was measured on our AMS system, but produced a very low target current (4.19mA) and poor reproducibility. These factors together resulted in our decision to fail the sample because any result would, in our opinion, be inaccurate and potentially misleading.

Our conclusion is that the sample delivered to our lab is not wood, but low carbon sediment. As such we do not think that we can attach any archaeological significance to its radiocarbon content. (Higham 2008)

However, the two other laboratories dated the material and returned relatively similar results. Kiel dated the conventional age to 30,600 +540/-510 BP. Gliwice dated the material to 34,800 +/- 1500. These results were first presented at the '1st International Conference of the Bosnian Pyramids' (ICBP) in August 2008, and they later appeared in press releases and in reports on the official Foundation website (Pazdur 2008). The following section identifies some of the interpretive issues involving epistemic and executive authority that emerge from this activity.

5.5.3 The 1st International Scientific Conference of the Bosnian Pyramids

These radiocarbon conclusions formed the centrepiece of the 1st International Scientific Conference of the Bosnian Pyramids (ICBP), which was held for five days in August 2008. The conference itself was an elaborate production put on the Bosnian Pyramid Foundation. No expense was spared in the conference materials, booklets, nametags and transportation [Figure 28], and many of the high-profile participants (PhD holders, mainly Egyptian) were financed for the duration of the conference. The first two days involved morning-to-evening guided tours of the 'pyramid complex', including the hills in Visoko, multiple tunnel sites, and other areas of interest, including Gornja Vratnica and Zadvidovici (where a supposed rock quarry and 'mysterious' stone balls were located, respectively). The last three days were comprised of all-day conference presentations. The conference presentations were held in the Hotel Grand in Sarajevo,

and the academic portion of the event was opened by local political dignitaries. The whole event was book-ended by public press



Figure 28: Various papers and booklets, as well as the official conference guest badge, given to participants in the ICBP conference, Sarajevo 2008. The conference was professional-looking and well organised. Photo by Tera Pruitt.

conferences. In appearance, the conference was streamlined and professional [Figure 30]. Most of the presenters had advanced degrees behind their names, and the format followed conventional scientific conferences around the world, such as the Annual Meeting of the European Association of Archaeologists. Before the actual event, the Foundation released public conference leaflets, brochures, radio broadcasts, television promotions and advertisements as large as motorway billboards, and they followed the conference with public press releases that promoted the ‘conference conclusions’ and ‘expert agreement’. The aim of the conference, officially promoted during and after the event, was to bring together experts and evidence so that discussion and debate could flourish—and so that the project could ostensibly legitimise itself through propaganda. However, it also became apparent during the conference that a primary aim of the event was to establish an *appearance* of authority, by drawing on institutions and systems of scientific accreditation to establish a sense of legitimacy.

The conference itself was a checkerboard of science and pseudoscience [Figure 31]. The majority of presentations by ‘accredited’ professors and researchers had nothing to do with the Bosnia Pyramid project or archaeology in Bosnia. Most of the Egyptian and Russian presenters, for example, discussed topics that were of interest to them and their own regions of work; for example, “The Ancient Library of Alexandria: Pioneering the Universal” or “The Step Pyramid at Saqqara: The Motive and Realization”. Some of the presentations that were listed in support of the Bosnian pyramid hypothesis actually derailed during the presentations, like that of Dr. Oleg Khavroshkin from the Schmidt Institute in Moscow, Russia, who drifted off-topic from Bosnian Pyramids to discuss extraterrestrial origins of life (see Section 5.4.3.3, above, for more discussion on Dr. Khavroshkin).

Two Chinese scholars from Xi’an⁶⁷ both attended the conference and gave rich and exciting presentations on genuine archaeological excavations of pyramidal tomb complexes in Xi’an China. Neither scholar spoke English; they wrote their abstracts and gave their presentations entirely in Chinese. The presentations were translated by an amateur Chinese translator living in Sarajevo, employed by the Foundation solely for the conference. During the first two days of conference tours, it became clear that both Chinese scholars were visibly confused by the (lack of) archaeology they saw at the ‘pyramid’ sites, and when they tried to explain this to Osmanagić and other participants in Chinese (with the translator trying to help), it was to no avail. At the various sites, Osmanagić would take them by the arm and show them his site stratigraphy, metaphorically patting them on the head, while they stood together shaking their heads, unconvinced [Figure 29]. The Chinese translator, on the other hand, was visibly moved by what she saw and heard at the conference; while she had no training or experience in archaeology, she did have PhD in an unrelated discipline, and she wrote a very strong letter of support for the pyramid project which she then printed and handed out to all of the conference participants. Importantly, when the dust had settled after the conference, these two Chinese scholars were mentioned as “supporters” in the official post-conference press releases, even though I suspect that they had little idea—to use the idiom—of what they were getting themselves into.

⁶⁷ Dr. Jiao Nanfeng, Director of the Archaeological Institute of Shaanxi Province, and Dr. Cao Fazhan, leading archaeologist in Han Yangling Mausoleum project.



Figure 29: Semir Osmanagić attempting to convince one of the sceptical Chinese scholars that these bedrocks are, in fact, 'pyramid blocks'. Photo by Tera Pruitt.

Alongside the credible presentations on Egypt and China, 'alternative' or 'fringe' papers were also given at the conference. Among these was a presentation by John Cowie, an alternative amateur and independent researcher living in the United Kingdom. His talk, which was a late inclusion in the conference and therefore did not appear in the original program, was based off of his self-published book *Silbury Dawning: The Alien Visitor Gene Theory*, the thesis of which is:

My theory is that the rapid evolution of our intelligence is due to the arrival on Earth of a highly intelligent extra-terrestrial being, or race of beings – which I will call the *Alien Visitor* throughout this book – that bred with, or somehow planted its genetic material and educated our *Homo sapiens* ancestors. (Cowie 2000: 2)

Another fringe presentation by the prolific New Age writer and alternative journalist Philip Coppens seems to have been given more weight by conference participants. Coppens gave a talk called “The New Fire Ceremony: kingship & renewal as a template for pyramid construction”, which he had previously given at a another conference and published online. In it, Coppens argues that the scientific establishment and “the old status quo” have not recognised the true importance and prevalence of pyramids throughout history:

The old status quo that it were but the ancient Egyptians and the Mayans that built pyramids has been upset and over the past decade, hardly a month seems

to have gone by without a pyramid being found; and almost each year, a gigantic pyramid or pyramid complex is found somewhere. Today, it is clear that massive pyramids are a feature of many civilisations, while the pyramids of Italy and Bosnia are not easily associated with any culture that is known to have either built such large remains or built pyramids. Over the past decade, the landscape of the pyramid debate has therefore radically changed and offers science a challenge. Today, I want to set out the challenge, as well as provide some of the answers that may be the key revelation of what the pyramids truly are. I hope that it will stimulate debate and can become a “foundation stone” of what I have termed “The New Pyramid Age”. [sic] (Coppens 2007)

His talk went on to describe how many new pyramids have been discovered in recent years and how ‘the establishment’ would soon have to agree with what ‘alternative amateurs’ have known all along—that pyramids are profound and central markers of human civilisation, and mysteriously culturally interconnected. While Cowie was taken to be a somewhat extreme personality at the conference, Coppens garnered authority and respect from other conference participants and became a central personality by the end of the week, even appearing in the final press conference and advising on the final outcomes and conclusions. Less than a year after the ICBP conference, Semir Osmanagić was invited to speak at Philip Coppens’s own alternative ‘Histories & Mysteries Conference’ in Edinburgh (Coppens 2008a), an event promoting fringe archaeology, highlighting the archaeological and mystical significance of the controversial Mitchell-Hedges Crystal Skull.

‘Alternative’ ideas and ‘establishment’ ideas seemed to meet halfway at the ICBP conference. While a number of ‘alternative’ presentations did appear at the conference, they were sandwiched between other presentations that did present ‘scientific’ data: the Chinese presentations of mound excavations in Xi’an, mentioned above, along with authoritative presentations on Egypt by participants like Dr. Mostafa El Abbadi on the Library of Alexandria Project. There was also a thorough lecture given by Chris Norman, a Development Control Manager of the West Lothian Council in Scotland. Norman’s lecture, titled *Tourism and the Cultural Heritage: Towards a Sustainable Approach*, which came out of a solid vein of heritage management policy in the United Kingdom. Norman addressed how the Bosnian Pyramid project’s potential for tourism could be maximised by planning and development, and he outlined important steps that could be taken to create a sustainable tourism industry in the region (Norman 2008). Norman’s presentation gave sound suggestions for improving tourist infrastructure and promotion—all useful suggestions that one might see in any policy consultation for heritage in the United Kingdom.

Finally, two individuals presented the genuine results from the radiocarbon testing at the ICBP conference: Andrew Lawler, who was the project’s Permanent

Archaeologist at the time and who, again, held a B.A. in archaeology from the University of Cambridge, and Dr. Anna Pazdur, a physicist and the head of the Department of Radioisotopes at the Gliwice Radiocarbon Laboratory. The Gliwice Radiocarbon Laboratory in Poland is described online as having “received the status of Centre of Excellence GADAM (Gliwice Absolute DAting Methods Centre)”, and Dr. Pazdur, who is head of the radioisotopes department, is listed as having:

[p]ublished more than 50 papers in international reviewed journals and more than 150 of other papers and reports, author or co-author of several chapters in monographs, author of one monograph, co-editor of one monograph. Editor-in-Chief of *Geochronometria: Journal on Methods and Applications of Absolute Chronology*. (ATIS 2010)

At the ICBP conference, Dr. Pazdur presented the radiocarbon results from her laboratory, and Lawler presented the findings from Kiel (Lawler 2008; Pazdur 2008). Dr. Pazdur’s presentation merely explained what radiocarbon dating methods were and how they operate, and she ran through the procedures that her laboratory took in order to reach the date of 34,000BP (or 42,000 BP calibrated).

Lawler’s presentation on the results from Kiel was more in-depth. He argued that the radiocarbon results were consistent with many different possible conclusions: (1) the carbonized wood and sediments might have been deposited in the time of the C-14 results, before the tunnels (and/or carvings) were made, and then the tunnel was used and abandoned before conglomerate collapsed onto the T1 Megalith ‘carvings’; (2) the tunnels/cave system might have existed in the pre-human Upper Miocene, then was later infilled during the C-14 dates by localized flooding from river or glacial melt water, after which the tunnels could have been used by humans but later abandoned; (3) the wood was embedded by humans for unknown reasons, possibly as a support or fixing, then carvings could have been made on large stones encountered in the sediments. Lawler presented all of these different potential scenarios, but implied that he thought the organic material was natural in origin and had little interpretive value. Semir Osmanagić and other conference organizers did not receive this ‘natural’ interpretation well—at one point Dr. Muris Osmanagić (Semir Osmanagić’s father) actually stood up and belittled Lawler in front of the conference audience. Lawler left his employed position with the Foundation soon after the conference, in part because of irreconcilable differences that had hit a tipping point at the conference (Lawler, personal communication 2008). Later, Lawler’s report appeared in modified form on the official Bosnian Pyramid website, and the modified document stressed the human origins of the material and downplayed Lawler’s original suggestions about the material’s natural origin (Irna 2008a; Lawler 2008).



Figure 30: Image from the ICBP Conference. *Photo by Tera Pruitt.*



Figure 31: Foundation volunteer proudly showing off a 'pyramid artefact' (which has been marked with a number for recording purposes). In reality, this is not an artefact, only a rock. *Photo by Tera Pruitt.*

5.5.4 Drawing on the Authority of Radiocarbon Methodology

I would argue that during the ICBP conference, the Bosnian Pyramid team drew heavily on the radiocarbon results because of the authority that *the method* holds in the field of archaeology and in the eyes of the popular media. Archaeologists have long recognised the importance of radiocarbon as a dating method. It was invented in the late 1940s by William Libby and it “revolutionized our understanding of prehistory...[providing] new, more reliable, and universally applicable techniques” for recording chronological sequences and ordering time (Trigger 1989: 384). Before C-14 dating, archaeological sequences and chronologies had to be created from rough typologies that were tediously correlated with historic references from ancient Egypt or other ancient societies. Radiocarbon dating revolutionized the field by allowing precise dates to be pinned down on specific stratigraphic layers and archaeological objects. Desmond Clark observed that without radiocarbon dating “we would still be foundering in a sea of imprecisions sometime bred of inspired guesswork but more often of imaginative speculation” (1979: 7). As Clark implies, radiocarbon dating is seen as very *scientific* and robust method, in that it observes the decay of atoms in the natural world and equates this to measurable time. When deep history and time is measurable by a scientific method, this is quite a powerful display of authority and promise.

Because of the importance of radiocarbon dating as a technology and a scientific method, the Bosnian Pyramid project has drawn heavily on the method for scientific presence and authority. The radiocarbon results were the centrepiece of the ICBP conference, and the results have been mentioned constantly in press releases ever since. For example, one headlining feature for Osmanagić’s induction in the Library of Alexandria Society stated that: “The Egyptian experts gave a special attention to the new radiocarbon results of the tested samples from the complex of the underground tunnels beneath the pyramids that point to a much older civilization than the Butmir Culture”. The article is titled, “Alexandrian Archaeologists Impressed By The Scientific Approach Of The Bosnian Pyramids Research” (The Archaeological Park: Bosnian Pyramid of the Sun Foundation 2009).

As a technology, the popular understanding of what a radiocarbon date does is relatively straightforward: you measure the rate of decay of carbon in an organic sample using the correct radiocarbon dating tools and technology, and you receive in return a reliable historical date for the material. The actual methodological process, however, involves many more diverse, complex and social steps: for example, there is complex

preparation of samples, accurate sampling by an expert who has received the appropriate sampling training, pre-treatment and avoiding of contamination, testing and results processing (Briant and Lawson 2008). The reality is that radiocarbon dating, like most methodological technologies, relies heavily on humans, their social methods and their ability to interact with and judge the final data output. Thus, the interpretation of seemingly objective data is heavily influenced by the *social* production of knowledge.

The meaning and authority behind the radiocarbon method might be compared to that of Magnetic Resonance Imaging (MRI) in the field of medicine. In an article called *Appealing Images: Magnetic Resonance Imaging and the Production of Authoritative Knowledge* (2005), Kelly Joyce writes that “popular accounts ‘black-box’ crucial decision and practices that shape the use and quality of MRI examinations in medical practice” (2005: 438). She argues that “broader cultural views that link mechanically produced pictures to the ‘revelation’ of the physical world and the production of truth, enhances the status of anatomical images, thereby increasing their significance in the construction and assertion of authoritative knowledge in contemporary medicine and culture” (2005: 439). Joyce is stressing that the power of the MRI as an ‘authoritative’ tool is behind the popular notion that the MRI process renders an ‘apparent’ image or ‘direct window’ into the body, simply ‘revealing truth’ about the bodily state. The popular idea is that the images produced by MRI machines simply ‘reveal’ these truths about the body’s condition, such as where tumours are located or what disease is ailing a person.

However, in reality, when professionals use and create MRI images, a great deal of imprecise social interpretation and practice goes into the construction of knowledge about the body. Doctors use these images cautiously, as mere tools for interpreting what may or may not be worthy of interest or further examination. Joyce explains that when doctors ‘read’ an MRI image, they heavily interpret what they see, as some of the fuzzy lines or blobs might represent a number of different realities about the body. Furthermore, even if the image has a clearly recognisable image, the doctor at hand is always socially interpreting the image and rendering meaning from it. The image itself does not ‘reveal’ truth; rather, truth about the body is constructed from social interactions in a network between the body, the machine, the image and the doctor. This reality of the technology’s interpretive and social aspect is ‘blackboxed’ in the popular understanding of MRI images, and—importantly—the authority and status of the MRI as a scientific method comes from this misconception of the method as being ‘relevatory’, a process of producing apparent truths.

Radiocarbon dating has a similar problem. A popular understanding of radiocarbon methods also presupposes that the technology is ‘relevatory’. The idea is

that an archaeologist simply inputs carbon samples of organic material into a machine, and then the data outputs ‘tell us’ the age of archaeological material. The authority and status of radiocarbon dating comes from this notion that the ‘scientific results’ in some way present us with ‘truths’ about the natural world. But in reality, radiocarbon dating presents results in a similar way to MRI testing: radiocarbon output charts must be interpreted by a (human) expert, the sampling process must be assessed for contamination, and the material’s original location and content must also be socially interpreted. The whole technology is based on a social construction of authoritative knowledge. The activity of sampling itself, of choosing what to sample, and of conducting or refusing to test organic material is inherently social and interpretive, a point that Joyce similarly argues in her study of MRI imaging (Joyce 2005).

In the case of the Bosnian Pyramids, the team’s decision to take radiocarbon samples emerged for socio-political reasons, as mentioned in the previous section; there was a great deal of social pressure for the project to provide a way to ‘reveal truth’ and produce ‘proofs’ about the ‘archaeological’ material under contestation. Radiocarbon dates provided the means for that revelation and authority for the project’s account of the past. Despite the fact that Oxford refused to participate, the dates that were returned from Kiel matched those that were returned from Gliwice—approximately 35,000 BP (uncalibrated). Trained experts, like the Cambridge-trained archaeologist Andrew Lawler and Dr. Anna Pazdur who is the Head of the Department of Radioisotopes at Gliwice, presented the radiocarbon results at the conference. The ICBP conference presentations by Lawler and Pazdur were straightforward, scientific and solid; they ran through their methods—the accurate and correct steps that were taken to sample and test the organic material from the ‘pyramid’ tunnel—as well as the results. The results in particular have been promoted on the official Bosnian Pyramid Foundation website, most notably the fancy output graphs and charts that show the calibration dates and ranges (ICBP 2008; Pazdur 2008).

In this instance, the data coming from the Foundation’s ‘final product’ account of the past was not a mere drawing upon or manipulation of institutions to seal the performance of scientific authority. The activity in question—radiocarbon dating and results presented by experts in the field—was arguably ‘real’ science taking place, not pseudoscience. However, the human activity was a taken-for-granted story. The final interpretations that appeared in public press releases and other social media headlined that: “the new radiocarbon results of the tested samples from the complex of the underground tunnels beneath the pyramids that point to a much older civilization than the Butmir Culture” (The Archaeological Park: Bosnian Pyramid of the Sun Foundation

2009). But there was no good evidence that the organic material under question had anything to do with human activity, and the script on the ‘megalith’ was of questionable provenance. The radiocarbon results, properly interpreted, have nothing to say about these crucial questions. Alone, they merely reveal that a certain lump of organic material likely dated to the ancient past. In reality, the organic matter—although scientifically tested by experts through a reliable method—was likely a piece of tree root or other organic matter that had washed into a natural cave system from flooding of glacial melts. But because of the popular understanding of radiocarbon dating as a reliable technology that ‘reveals truth’ about *past people* and not just *past organic matter*, the story of Palaeolithic pyramids sounded plausible to the public. The project’s use of the radiocarbon method and their appropriate performance of presenting the radiocarbon results was immensely successful at accumulating attention, prestige and a great deal of authority for the project in the eyes of the general public.

5.6 Chapter Conclusion: Authority in the Politics and Performing of Pyramids

This case study raises questions about what makes something *appear* authoritative different from something that *is* authoritative? Collins and Evans suggest that, “The problems of legitimacy and of extension arise because ‘the speed of politics is faster than the speed of science’” (2007: 125). Certainly this case study embodies such a scenario; the site has been lifted in authoritative status and popularity because of its politics, and because of the way scientific methods are being socially applied and performed to bolster pseudoscientific theories. The Bosnian Pyramid site’s context, and its ‘authority’ in relation to the science it performs, is complicated by the layering of social and scientific politics at play. The site is drawing its sense of legitimacy from performance by using select scientific methods and traditions that have been authorised by the scientific community. In a case like the Bosnian Pyramids, the lines between authoritative categories in science—authoritative, authorised, legitimate, and merely appearing authoritative—are blurred and nuanced, and such context in a field like archaeology raises larger questions and conditions about what it means to have authority in a scientific discipline

This chapter argues that the Bosnian Pyramid project has accumulated authority for two main reasons. First, the public in this case study are actively *participating* in the

invention of the notion of pyramids. The pyramid project is deeply ingrained in national and ethnic Bosnian history. Director Semir Osmanagić is able to construct his vision of Bosnian archaeology, and continues to hold authority, only through the continued participation by a supportive audience who allows his ideas to gain momentum and security. A variety of interest groups attach different values and meanings to the pyramid narrative. To stress again, Eric Hobsbawm writes:

‘Invented traditions’ have significant social and political functions, and would neither come into existence nor establish themselves if they could not acquire them...the most successful examples of manipulation are those which exploit practices which clearly meet a felt—not necessarily a clearly understood—need among particular bodies of people. (1983b: 307)

Such a *need* for pyramids clearly exhibits itself at Visoko. Unlike the unsuccessful pseudoarchaeology site of Gabela, where another pseudoarchaeologist claimed to have found Troy,⁶⁸ Osmanagić’s pyramid site satisfies specific socio-political needs. It offers a world-class monument that outstands and out-sizes every other major national monument in the world, right there in ‘little Bosnia’. It offers politicians a diversion from unstable government problems and offers a campaign strategy. It gives a war-struck town a thriving economic boost. It fulfils serious social needs. Osmanagić presents a simulacrum and hyperreality, a ‘virtual’ story that overlays the ‘actual’ truth—but it is only through the full acceptance and participation in this vision that the site comes to fruition. This active, participatory inventing is exemplified in one quote by a Visoko resident, which bears repeating: “If they don’t find the pyramid, we’re going to make it during the night. But we’re not even thinking about that. There *are* pyramids and there *will be* pyramids” (quoted in Foer 2007). This is exactly what the participating public, media, and Osmanagić are doing: they are constructing pyramids through their participation.

Secondly, the project is constructing and maintaining authority through their *performance* of authority. This argument has several facets. In Section 5.4 of this chapter, I refer to the performative process by which Osmanagić is inventing a site and a sense of authority by acting the role of amateur archaeologist, creating the appearance of serious academic project. To explain more deeply—in the book, *How to Do Things With Words*, J.L. Austin distinguishes between ‘statements,’ which are utterances that simply describe something, and ‘performative language,’ which are neither true nor false statements, but rather utterances which perform certain kinds of action. When you utter performative language, and when the circumstances are appropriate, the language does not describe

⁶⁸ See Section 5.3.6.

something, but rather *does* something (for instance, saying “I name this ship the *Queen Elizabeth*” in the appropriate circumstances will perform the action as it is said) (Austin 1962). Although Austin was certainly discussing more narrow and specific utterances, the general idea can be applied to the performances occurring at Visoko. By repeatedly saying that there are pyramids, and by describing an inexistent site as existent in what appears to be authoritative circumstances, Osmanagić is *creating* pyramids. By saying on ABC television, for example, that “If a person could look back and just visualize this place as you see it, eight thousand, ten thousand years ago, they would see a massive stone city” (ABC 2006), he is uttering performative language. He is not describing the faux city, because it does not exist. It is through the verbal narration of this city—and through the appropriate circumstances that give him authority (namely authoritative media)—that the city *is being invented*.

Another facet in this project’s performance rests on its reliance on science as a master discourse. The Foundation’s performative language and mimicry of scientific documents are, I would argue, quite literally inventing a heritage site. This point is perhaps best driven home in regards to the physical site excavation. When visitors approach the Pyramid of the Moon, they find large-scale excavations of monumental steps leading up the mountain. Visitors like Joshua Foer exclaim, “Suddenly it dawns on me—and I’m shocked that it has taken me so long to figure this out—that Osmanagić is *carving* pyramids out of these pyramid-shaped hills” (2007, emphasis added). Osmanagić has chipped away at the mountainside until it physically resembles pyramid steps. This behaviour is performative: Osmanagić is playing the part, constructing (quite literally) the right image, and thus inventing heritage. This last point is particularly relevant, because Osmanagić’s work at the site is an enormously complex operation, and it relies on structures of authority that are embedded not only in the discipline of archaeology, but also in popular conceptions of what it is to do archaeological research—which, perhaps unsurprisingly, affect popular and professional receptions of archaeological interpretations. The contestation behind this case study questions the underlying practices of legitimation that we use in our own practices in disciplinary archaeology, and it addresses the ethical use and abuse of authority in archaeological or amateur research.

A final consideration in this chapter concerns the way archaeological authority is driven by public and academic confusion over the nonhuman actors, technology and methods involved in the production of knowledge. The physicality, materiality and technicality of the Bosnian Pyramid project play critical roles in the creation and sustenance of authority. Like in the case of Çatalhöyük, the case of the Bosnian Pyramids

shows that authority is an accumulation or outcome of many different translations and negotiations by many different actors in a given social network.

Human motivations—political desires and social desires, like a wish for popularity or personal attention—do drive much of the authority in this case. However, I would argue that the material and nonhuman actors are even more important in this scenario. On the one hand, it is the physicality and materiality of this case that has had such an impact on its successful reception as an authoritative site. The reason the pyramid story is so well-received by the public is that it offers a very tangible symbol for Bosnian nationalism. This national symbol is derived not only from the monumental presence of pyramids in the landscape—which are very striking physical markers that can be deliberately pointed to as something ‘there’ and ‘important’ looming over the town—but also it is a symbol that can be easily inscribed. The most obvious example is the use of the pyramidal shape in the official Foundation logo, which inscribes this pyramid into the Bosnian National Flag, creating a mobile, powerful and very tangible symbol of nationalism and pride. Such an inscription becomes an agent itself, reinforcing the authority of the project and its pyramids through its very visible connection to nationalism and socio-politics. The project has also had a very real, material impact on the landscape and region. Much of the success of this project involves the way various people—locals, politicians, volunteers, interested visitors—can get physically involved in the project and see very real, material economic returns. There is no confusion over the positive economic impact, or the material and psychological gains, that members of the public have felt.

But confusion does emerge when the ‘science’ and ontological significance of the project is examined more closely. Professional archaeologists who have opposed the project have highlighted the fact that the Foundation’s claims for scientific accuracy are unsupported, and they are right. For a few members of the public that I interviewed, the accuracy of the project was a non-issue: they were purely interested in the economic and material benefits the project could bring. However, it was far more common for people to express a sense of support for the project because they thought it was a genuine, scientific archaeological site. This means Semir Osmanagić has successfully *performed* the role of a scientist or academic archaeologist, engaging in the appropriate mannerisms and behaviours, collecting the right credentials and stereotypical logos and brands of an archaeologist (like his Indiana Jones hat), without having the ontological evidence to back up his claims. Many members of the public have not been privy to the lack of evidence and contestation around the site, and have only seen the façade of scientific activity.

Osmanagić has been mobilising the appropriate nonhuman actors and methods—like experts and radiocarbon dates and conference badges—but he denies them the necessary public scrutiny to give them the authority of facts. All of the nonhuman actors in the case of the Bosnian Pyramids are mobilised, but remain mere performances and methods, never evolving the necessary stability and consensus to turn into ‘facts’. Authority in this case manifests in a theatre of ‘doing science’, where the nonhuman actors have no agency of their own, for they are employed to play very specific roles set up by Osmanagic and his team. For example, objects like rocks (such as the one held by the very eager volunteer in Figure 31) have been mobilised by the pyramid team to represent “pyramid artefacts”. These objects appear to appropriately perform their roles in the pyramid story, until they are examined further and the details become contestable. In the act of further scrutiny, the true ontological state of being ‘just a rock’ becomes clear, and the authority of the pyramid story starts to unravel. In this case, when the surface façade is scratched and the physical ‘smoke and mirrors’ behind the performances are examined in more detail, then the ‘evidences’ and ‘proofs’ of the project fall apart, and their actual roles in support of the narrative become far less clear. At some point, authority fails to accumulate when the ontological and material evidence runs out and can no longer be mobilized.

Authority is very strongly based in the appropriate performances of roles and categories. Socio-politics and institutions can dramatically affect the reception of certain accounts of the past. However, the ontological world plays a very significant role in the overall stabilisation and maintenance of scientific authority and the production of authoritative knowledge. This case illustrates how active participation by both knowledge producers and knowledge consumers is inherent in the construction and maintenance of authority. Nonhuman and human actors, performances and participation, institutions and individuals are always interlinked and essential to the role of sustained authority in the production of knowledge. They are accumulative, and each must necessarily feed back into each to establish an authoritative vision of the past.

CHAPTER SIX:

Conclusion: Authority in the Production of Archaeological Knowledge

"Science, if it can deliver truth, cannot deliver it at the speed of politics." (Collins and Evans 2007: 1)

"...science rests, in the long run, on the consensus of scientists, not on the authority of any on individual, no matter how outstanding." (Goldstein and Goldstein 1978: 255)

6.1 Introduction and Summary

This thesis began by questioning: what is authority in archaeological practice? What contexts and conditions lie behind the creation and maintenance of archaeological authority? This thesis addressed the problem that, while the field of archaeology has seemed ready to engage with issues of authority and power rights in communities of practice, rarely has the root conceptual understanding of what authority is, and how it manifests in the first place, ever been explicitly discussed. Chapter Two of this thesis deconstructed the concept of authority in relation to the production of archaeological knowledge. It analysed the term 'authority' in existing literature and observed how formal accounts and representations of the past rely on the underlying notion of authority: personal and institutional, epistemic and executive. Chapter Three outlined the methodology used to examine two case studies, which illustrate the development and mobilisation of authority in actual archaeological practice. Chapter Four introduced and analysed the case study of Çatalhöyük; it demonstrated how authority is embedded, used, networked and translated—structurally, conceptually and spatially—in the production of archaeological accounts of the past. Chapter Five used the case study of the Bosnian Pyramids to illustrate how authority can be drawn from socio-politics and science as a master discourse, and it argued that performance and participation are integral to the way archaeological 'final product' interpretations are successfully received by the general public.

The following sections of this conclusion chapter examine the main arguments that can be drawn from this study. The first section offers a considered summary of the two major case studies, addressing the similarities and differences between them and

their significance. The second section of this chapter revisits the argument that authority begins in dividing practices, in the activity of defining boundaries and categories, in setting up a sense of alterity. The next section argues for the importance of recognizing authority as a cumulative process. The active processes of translation and stabilisation, as well as the important role of nonhuman actors in the production of knowledge, are critical in the creation and maintenance of authority in the discipline of archaeology. The following section defines the importance of epistemic dependence, the concept that all knowledge is built upon indirect evidentiary support, in the trust in experts and the notion of expertise. These aspects of knowledge production sit alongside, and are directly impacted by, ontological evidence in the creation and production of authority. This chapter concludes by asking how we might deal with authority in the field of archaeology, suggesting future research in this area.

6.2 Comparison and Significance of the Case Studies

6.2.1 Introduction: Summarising Case Studies

This thesis has been intentionally structured around two case studies, Çatalhöyük and the Bosnian Pyramids, analysed in Chapters Four and Five. As explained in Section 3.3.5, these case studies were chosen to be compatible, so that when brought together in a discussion, remarks about their operation would provide meaningful conclusions in an analysis of 'authority'. These case studies were not examined simply to compare and contrast two different case studies, as Çatalhöyük and the Bosnian Pyramids are not directly comparable and equal sites. Rather, these case studies were explicitly chosen as compatible examples that *illustrate* solid examples of how authority manifests and operates in the production of archaeological accounts of the past. These case studies demonstrate key points of this thesis: that authority is an accumulative, material and social phenomenon (see Section 6.3, below). The following sections briefly discuss the results from the two case studies of this dissertation, in order to integrate the demonstrable qualities of these studies into the final arguments on authority that make up the rest of this concluding chapter.

6.2.2 Differing Research Results and the Successes and Failures of the Two Case Studies Used in This Thesis

As explained in Section 3.1.2 and 3.3.1 of this dissertation, I chose to study one case of professional archaeology (Çatalhöyük) and one case of alternative archaeology (Bosnian Pyramids), since both projects produce their own 'authoritative' accounts of the past through their practices, publications and public presentations. At the end of this study, I find that my results have yielded different outcomes, with different successes and failures.

The differences in research outcomes are due to the variable amount of time I spent conducting fieldwork at each of my case studies, as well as the nature of the studies themselves. With the Bosnian Pyramids, I had a very long and familiar relationship with the project's development. I followed its progress from the earliest news coverage in 2005. In 2006, I began studying the Bosnian Pyramids in depth for my 2007 Master's dissertation on the socio-politics of the project (Pruitt 2007). For my doctoral work, I continued to research the site through 2009, taking multiple short fieldwork trips to Bosnia over five years (intervals from 2006-2011), with an extended stay in the country through the summer of 2008. Because of my familiarity with the project's history and the socio-politics that sustain it, I believe I have had much greater success in using the Bosnian Pyramids as a case study in this thesis. Drawing from my case study in Bosnia, this thesis provides a comprehensive look at how authority can be drawn from socio-politics and science as a master discourse, comprehensively arguing that performance and participation are integral to the way archaeological 'final product' interpretations are successfully received by the general public (see Chapter 5).

I spent a much shorter duration of time conducting fieldwork at Çatalhöyük: just five weeks in 2009, late into the project's history and development. Because of this, I think that alongside my successes, I have also had some noteworthy failures in using this case study in this thesis. In Section 3.3.2.2, I explain how I chose to conduct fieldwork for five weeks at Çatalhöyük in the summer in 2009. Since I felt I was extending ethnographic research at an already much-studied archaeological site, I deliberately designed my fieldwork to mirror previous Çatalhöyük ethnographies of similarly short lengths—notably those of Hamilton in the 1996 season, Rountree in the 2003 season, and Erdur in the 2006 season (Hamilton 2000; Rountree 2007; Erdur 2008). Because I reviewed so much literature about the project in advance, my initial aim for on-site fieldwork was just to gain familiarity with the site structure and to have the opportunity to talk with the archaeological team and members of the visiting public. But I discovered that project structure and methods on site were far more complex and

interesting than I had initially assumed (and had read about), and my dissertation in turn focused more on my own fieldwork than originally planned. As discussed in more depth in Section 4.5.2, the final outcome of this approach has led to some failures as well as successes. For example, I find that some of my results may have simply contributed another ethnography to an already almost-toppling 'pile', and some of my results may have been compromised by an overly wary and 'too studied' project team (see Section 4.5.2 for a detailed discussion on these failures and limitations). Despite the insights I gained about authority in disciplinary practice, my limited time at the site has led to a less comprehensive study on authority at the Çatalhöyük site itself than envisaged. The Çatalhöyük project is multi-layered, chaotic and complex, and any comprehensive study of authority and the production of knowledge at this project must rely on an extensive familiarity with the site, which I was unable to obtain in the limited time and space I had available for doctoral work (see Section 4.5.2).

This thesis has, however, successfully employed both the Çatalhöyük and Bosnian Pyramids case studies to illustrate the original argument that authority in the production of knowledge is a *messy, mangled and material* affair. Despite the very different backgrounds of these case studies, both demonstrate how authority in the discipline rests on the stabilizing of material performances and on the complex material interactions of things and people. The following sections, starting in 6.2.3 and continuing through the rest of this chapter, discuss the overall conclusions that these two studies offer on the significance of material evidence in producing authority in archaeology.

6.2.3 Case Studies Comparison and Significance: Contribution to Understanding Authority and the Importance of Material Evidence in Archaeological Practice

The Çatalhöyük and the Bosnian Pyramids case studies sit on opposite sides of the 'demarcation line', and their complementary use in this thesis has maximised demonstrative value of the argument that authority is an accumulative, performative and contextual social process. As explained in Section 3.3.1, the 'demarcation line' in philosophy of science studies refers to the academic attempt to demarcate authorised or 'real' science from non-scientific or pseudoscientific enterprises (Curd and Cover 1998: 2). Çatalhöyük is a professionally organised and empirically thorough archaeological project, and it has provided a sound case for how authority can operate within standardised, professional boundaries. Chapter Four of this thesis targeted the physical, spatial, temporal and social aspects of the Çatalhöyük project, outlining the way human

and nonhuman actors within the project produce knowledge at the Çatalhöyük mounds and dig house. Chapter Five of this thesis examined the Bosnian Pyramids project, highlighting the way the authority of this pseudoarchaeological project rests on social performance and participation. Despite the very different approaches and the very different ontological value of these sites, both projects 'have authority' in certain contextual arenas. Furthermore, both of these projects and the contributing archaeologists involved in the production of knowledge arguably lose or undermine some of their own authority because of continued misuse of ontological evidence, and because of confusion over the nonhuman actors that are necessary for the continued stabilisation and accumulation of authority (see Section 6.3.2 below, for detailed discussion on this point).

As explained in Chapter Four, the Bosnian Pyramid project's executive and epistemic authority is apparent in how it has been given full permissions and political support by the national government, has been treated as authentic and authoritative by many media outlets, has the support of many people with authoritative credentials and institutions behind their names, and has been directed by a man who a majority of the Bosnian public considers to be an authority about the past due to his credentials and performance as an archaeologist. Director Semir Osmanagić has been treated as an expert authority on archaeology in Bosnia by the media and public, as well as by professional institutions like the University of Sarajevo and the Library of Alexandria. Similarly, as explained in Chapter Five, Çatalhöyük is also an authoritative site, supported by the national government as well as by numerous political and social institutions, and it is acknowledged by the entire professional archaeological community. Furthermore, a majority of media, the profession and the public also treat director Ian Hodder as an authority about the past because of his strong empirical program and novel ideas implemented at Çatalhöyük. This thesis, using two sites on opposite sides of the demarcation line, which are both creating 'authoritative' accounts of the past, has examined the fundamental tensions behind what makes someone an authorised authority and what makes an account of the past authoritative.

Sometimes a picture can be worth a thousand words, so I refer to the images in Figure 32, which represent some the similarities and differences between these two studies. Both of these sites are large earth-moving operations, and both have a diverse team, with credentials from reputable institutions, who claim passion for finding a kind of 'truth' about the prehistoric past. Both projects have figureheads who exude a knowledgeable presence, who staunchly argue for 'correct approaches' and the empirical or scientific validity of their claims, and who strongly argue for the voices of



Figure 32: Photographs of two ‘authorised’ archaeological ‘authorities’—the top photo is of Semir Osmanagić lecturing to a public crowd in front of the public and media; the bottom is Ian Hodder lecturing to members of the public on tour. *Photos by Tera Pruitt.*

subaltern groups to be heard. Both projects are highly valued by the media and by the many people whose lives are directly touched and improved upon—socially, economically, nationally, professionally—by the existence of the sites in the first place, and by the archaeological interpretations which develop from the teams' activity. In both projects (perhaps most clearly seen in these images), the orientation of humans in relation to the material and physical space is heavily controlled at the archaeological sites by the archaeologists in their respective teams. This physical control directly enables and limits the power hierarchies and the authority of individuals who interact with archaeological material, and this directly impacts the authority of claims about the past. At both of these sites, individual and institutional authority is entirely dependent upon the physical and material world, as well as the human and nonhuman actors who enable and constrain the interpretive value of accounts, directly resisting and accommodating authority. This discussion is expanded upon in Section 6.3, below.

It is important to revisit critical points addressed in previous chapters of this dissertation. Major differences exist in the way these two case studies operate: in how they treat the nonhuman actors involved in their archaeologies, in the way their empirical authority operates, and in the sustainability of their authority. As argued in Chapter Five, Semir Osmanagić's site in Bosnia relies upon what I call 'smoke and mirrors' performance and participation. There is no true ontological evidence of prehistoric pyramids in Visoko; there is no material evidence of an ancient Bosnian supercivilisation. The site has gained its authority primarily through the *performance* and outsourcing of science by the key players involved, such as Osmanagić himself and many of his team. Furthermore, I argue the site is critically supported by the public and many credentialed 'experts' because they actively want to *participate* in the construction of meaning, value and national symbolism in a post-war country. While the economic and social benefit of the pyramids project is very much real, the authority that lies behind this claim of pyramids—and behind the people who insist upon it—is ultimately unsustainable. As argued in the conclusion of Chapter Five, people like Semir Osmanagić are forcing the nonhuman actors in this site to play very specific roles in a theatre of 'doing science'. Objects like rocks are being inappropriately mobilised to represent 'pyramid artefacts'. When these objects are no longer mobilised by participatory actors, when they lose their significance in a narrative of post-war Bosnian social reconstruction, then they will lose all authority. Authority is a cumulative process, and in the case of the Bosnian Pyramids, that accumulation will run out of steam at a certain point of time. This site demonstrates how authority is strongly based in the appropriate performances of pre-authorized roles, categories and institutions (like

‘science’ or ‘radiocarbon dating’); performance within established socio-politics and institutions can dramatically affect the reception of certain accounts of the past. Ultimately, however, the ontological world intrudes upon, stabilizes, maintains or disrupts scientific authority in the production of authoritative knowledge. It is in this respect that the authority of the Bosnian Pyramids case study fails over time.

Chapter Four demonstrated exactly *how* this process of stabilization works in detail, by deconstructing the way authority operates in human and nonhuman interactions at the site of Çatalhöyük. As the conclusion of that chapter argued, authority is an outcome of complex social and physical factors. Nonhuman actors, as well as processes like inscription and translation, play critical roles in creating and maintaining authority in the production of archaeological knowledge. This case study demonstrated how physical and temporal factors—such as the layout and territoriality of dig house space, along with personal familiarity with repetitive archaeological material over long periods of time—can lead to personal and institutional authority. This chapter demonstrated how the most influential actors in knowledge production are nonhuman actors, along with the methods and programs of inscription and translation that create both stabilities and authorities. Unlike in Bosnia, the team at Çatalhöyük have been actively establishing stability and familiarity with material at the site, accumulating a great deal of empirical authority based on continued agreement about the material evidence, stabilising a sense of ontological reality. However, like in Bosnia, the project has not fully addressed the way authority is *actually* operating, and how it is ultimately reliant upon its nonhuman actors and the processes of inscription and translation. Director Ian Hodder has arguably begun to undermine his project’s own authority by continually insisting that instability is key to the construction of more valid realities or accounts of the past. In reality, this authority is formed from continued familiarity or stability with repetitive material culture, and the consensus formed from peer review and from multiple voices leading to stabilization.

The important similarities of these case studies rest in the way both projects seem to misunderstand the active role that nonhuman actors, as well as processes like inscription and translation, play in the construction and maintenance of authority. The important differences in these case studies rest in the ultimate direction of the two projects, in the exact way this misunderstanding affects their authority. In Bosnia, the entire project’s premise and future is at stake, as the lack of ontological reality to back its claims will make the project’s public support collapse, or perhaps reduce its authority to merely a fringe following. In Çatalhöyük, the project’s role as a cutting-edge archaeological project or an influential model of archaeological method is at stake, as

any new instability that is artificially forced upon this empirically based study of the past will simply stabilise once again in the future, as various team members develop greater familiarity with recognisable and repetitive archaeological material. The following section of this chapter offers the concluding arguments of this thesis regarding the significance of the findings from these two case studies and the role of authority in the production of knowledge in archaeology.

6.3 Deconstructing Authority in the Production of Archaeological Knowledge

6.3.1 Authority in Dividing Practices, Categories and Alterity

A major way authority operates in the production of archaeological knowledge is in the solidification, definition and categorization of what it is to be ‘appropriately archaeological’. In any discipline, a great deal of power and authority is vested in both the state of being classified and in who has the power to name or choose categories. ‘Dividing practices’ (c.f. Foucault 1965; Rabinow 1984: 8-11) are both physical and intellectual and have an essential power/knowledge relationship. The act of classifying people and things creates relationships of asymmetric power, through practices of inclusion/exclusion. To repeat from Bowker and Star: “to classify is human...a classification is a spatial, temporal, or spatio-temporal segmentation of the world” (1999: 1-11). As humans, we classify the world, often tacitly, by sorting activities and materials into categories. By doing so, we create social and moral order out of the world we experience, and we construct self-identities that exist against categories of what we see as ‘other’. In an academic discipline, the very nature of classifying objects and acts creates greater and lesser authority by those who are dividing and being divided. This thesis examined two specific case studies that illustrate how dividing practices in the discipline of archaeology can construct categories through a sense of validity and alterity—groupings we distinguish as ‘us’ versus ‘them’, ‘archaeological’ versus ‘not archaeological’, ‘authorised’ versus ‘unauthorised’. Dividing practices impact our method and interpretation in archaeology, and impact our understanding of authority.

The photos in Figure 32, above, represent some of the issues in categorisation and alterity. These photos from both the Çatalhöyük and the Bosnian Pyramids project show how archaeological (and pseudoarchaeological) spaces can be physically divided

by place, people, actors and materials, all of which tacitly operate within a social hierarchy of access and authority. In both photos, the leading representative authorities—Semir Osmanagić in Bosnia and Ian Hodder in Çatalhöyük—stand in a position of intimate access to remains from the past.⁶⁹ Both men hold PhDs and other credentials from recognised universities. Both hold requisite government permits to access archaeology. Both voice their desire to engage in a dialogue of transparency and scrutiny. Both have the highest level of executive authority and access in their respective archaeological sites and projects. In both photos, members of the public stand on platforms above on ground level, looking on while they are lectured to by the authorities below them; they are shown what is worth seeing and what information is valuable enough to be interpreted and narrated. In both pictures, the interpretations and accounts of the past being narrated by the authorities are also being mediated in a way that further elevates their accounts and accountability—in the case of Bosnia, television crews capture and relay the interpretations by Semir Osmanagić, and in the case of Çatalhöyük, public display signage lines the site and supplements Ian Hodder's presentation with information that has been chosen represent the most stable and authorised information about the Neolithic past. These two photographs illustrate how the divisions we create in physical and intellectual space promote a sense of authority through alterity. Dividing practices are one of the most fundamental ways that archaeology operates as a social science. Our science and our methods are what set us apart from 'the others'; our divisions of space and place set our teams apart from the general public; the nature of division creates social asymmetries, elevating some to positions of authority, and others to subaltern roles.

As addressed in Chapter Four, during my fieldwork at the site of Çatalhöyük in 2009, a great deal of site activity and interpretation emerged through such social categories: spatial, temporal, interpretive and inscriptive. Laboratory spaces in the dig house, for example, were arranged according to artefact types, indicative of the way the profession has developed around specialties that focus on materials such as obsidian or faunal remains. This arrangement of 'pod-like' laboratory cultures very physically affected the division of material remains in the dig house. It also socially impacted groupings of people and practices, which directly affected interpretation and the production of knowledge, based on the way such groupings physically enabled or constrained how individuals could build their own social and epistemic authority. During my visit in 2009, people and spaces at Çatalhöyük were arranged and controlled

⁶⁹ Or in the case of the Bosnian Pyramids, presumed remains from the past.

according to executive hierarchy in the site social structure. For example, certain rooms were tacitly restricted to only certain specialties or individuals, unless permissions were obtained by the appropriate authorities or representatives, and the whole dig house was tacitly controlled by the narrowing or consent of access. More spaces were accessible to those who held more executive access, due to their strong social and temporal ties to the site.

On a disciplinary/public level, scholars like Reba N. Soffer have argued that, “in the long run, the success of a discipline is not determined by its powers of protection or patronage”, but rather “successful professions have maintained a monopoly over a special body of knowledge and skills...of a real benefit to the public” (1982: 801). When certain individuals own or possess the physical arenas of knowledge production, like archaeological sites, they owe a great deal of responsibility to the other stakeholders who may wish to have access. At Çatalhöyük, Hodder and his team have tried to accommodate multiple stakeholders and voices by allowing them greater access to more private areas of the dig house and less accessible materials. However, alterity and authority are still staunchly (and in some ways, necessarily) maintained at Çatalhöyük. While Hodder has previously argued that “Subordinate groups who want to be involved in archaeological interpretation need to be provided with the means and mechanisms for interacting with the archaeological past in different ways” (Hodder 1992: 186), the very sentence structure of this comment allows that Hodder and his team are in the authoritative position of *providing* subordinate groups with ‘means and mechanisms’, while subordinate groups are at the receiving and disadvantaged end of this process, dealing with whatever means or mechanisms they are allowed or allotted. While the team’s intent to empower members of subordinate groups in this case is highly motivated with a real desire to allow greater accessibility and freedom to archaeology, and while I do think subordinate groups have been empowered in many ways through their collaboration with the site, it must still be recognised that this empowerment is always controlled by those who are higher in the social hierarchy of archaeology. Any subaltern empowerment has been necessarily portioned out with the aim and understanding that, by giving away site access and authority to subordinate groups, it should never undermine any benefit to archaeologists themselves. This defence of ensuring the boundaries from what is ‘authorised’ from what is ‘other’ (the public, Goddess Community, local communities, etc.) is highly motivated by the status of archaeology as a discipline, where archaeologists are factually constructed through their appropriate practice and familiarity of behaviours within that discipline, and they need

to secure their own positions in an intellectual and professional arena by defending their own sense of self, practice and careers.

It should also be recognised that professional authority of access and territoriality is arguably not always a bad thing, as a case like the amateur Bosnian Pyramid project may illuminate. Many professional archaeologists have criticised this project for its pseudoarchaeology. It has damaged genuine archaeological remains and threatened historical accounts of the Bosnian past. Nevertheless, this case critically shows that there is fundamental power to be had in the control of physical access and epistemological space. The amateur Bosnian Pyramid project has created and maintained authority through its control over the physical landscape, and its ability to successfully define, label and alter physical and intellectual space. It has acquired the requisite permits from a supportive government, developed status and attention through its influence on popular media, and manipulated the landscape to appear archaeological. However, unlike the case of Çatalhöyük, the Bosnian project's control over physical space involves only a careful *performance* of scientific authority, heavily controlling only an image of an authoritative account of the past. Its claims have little ontological significance. In a case like Çatalhöyük, Hodder and his team control and defend their epistemological space through the translation of evidentiary support that they accumulate from the ontological world. This highlights an important distinction in the construction of authority in archaeology: nonhuman actors actively enable, constrain and limit how authority can be sustained over time. This point is expanded in the following section.

6.3.2 Authority in Translation, Stabilisation and the Agency of Nonhuman Actors

One of the most important arguments that has emerged from this research is that, in science, authority is inherently rooted in the act of constructing things recognized as 'facts'. In the production of knowledge, the construction of facts is very different from the mere production of accounts or narratives. The case studies of Çatalhöyük and the Bosnian Pyramids critically address this point, for in both cases, the authority of certain individuals and their theoretical programs are undermined by confusions and misrepresentations of the roles of nonhuman actors. This thesis argues that the active role of the nonhuman processes and objects involved in the production of knowledge are critically important to the authority of facts and 'final product' accounts of the past.

Chapter Four of this thesis used the case study of Çatalhöyük to demonstrate the importance of translation and inscription in the production of knowledge. The chapter used Latour's 'translation model' (Latour 1986: 266-269; also see Section 2.2.4 in this dissertation) to show how executive and epistemic authority accumulates through the translations, negotiations and interactions of many different actors in a given network. Chapter Five used the case of the Bosnian Pyramids to illustrate that, while human socio-political desires are main contributing factors to the executive authority and popularity of an archaeological project, the authority of a 'final product' archaeological account fundamentally rests on the material and ontological significance of its evidence. In projects claiming scientific roots, the authority and agency of the ontological world will eventually win out over the performances and politics that might lend immediate authority to the site. Previous literary discussions about authority in archaeological practice have focused on the presence and impact of human actors—a great deal of debate has surrounded issues of gender, site control, the power and voice local publics, as well as individual rights over interpretation. However, this thesis argues that authority is a complex process that accumulates from the interactions of *both* human and nonhuman actors. It should be recognised that the ontological world has as much impact, and places as much constraint upon, authoritative interpretation as the humans that interact with it.

At Çatalhöyük, Ian Hodder has long recognised the importance of authority in the archaeological process, but he has mis-conceptualised archaeology as a practice where the primary actors are human. Hodder has vigorously promoted the nonhuman actors at his site through a very strong program of empirical practice, with at-hand specialists in the field and unprecedented attention to scientific detail. However, he has paradoxically promoted a theory of practice where interpretation and fact-construction are a human-centric affair. He continues to promote the idea that instability in human presence at an archaeological site will bring better interpretation to the archaeological accounts of the past produced there. The idea is that humans will better think through the material they handle if they are forced to continually contest their relationship with it. However, as I argue in Chapter Four, this continuous instability neglects the essential authority of the ontological world by denying the agency and constraints that nonhuman actors place upon human interpretations. The stability that Ian Hodder tries to resist is, in fact, precisely where his empirical authority is rooted: in the familiarity, repetition and stability of evidence. Physical space, landscapes, material objects, artefacts and tools, methods and practices are all rooted in physicality and materiality. They go

beyond playing the role of being mere data or objects. They practically enable, constrain, resist and accommodate the way we engage with the world, and they limit our interpretive authority. They impact the way we can say with greater or lesser certainty what is a 'fact' versus what is a mere account or narrative. The reason an archaeologist can 'become an archaeologist' and gain authority in that role is through the performance of the appropriate behaviours of an archaeologist—which are rooted in physical practices that promote familiarity with repetitive and accumulative ontological evidence. And the reason authority accumulates through expertise (gaining greater familiarity with a site or specific type of archaeological material category), is that there is a process of active stabilisation as material becomes more recognisable and repetitive.

It is easy to contrast Çatalhöyük with the Bosnian Pyramids project, where confusion also arises over the role of nonhuman actors and methods. In Bosnia the project is only performing authority by drawing on the institution of science as a master discourse. The nonhuman actors upon which that performance rests—objects like rocks that they claim are artefacts, and methods like radiocarbon dating that are misinterpreted—lack the public scrutiny of 'facts'. The Bosnian Pyramids team mobilises objects and methods to play roles in a theatrical story for the public; these things are simply a way for the team to prove that they are 'doing science'. The lack of ontological significance in their material—which breaks down under further scrutiny and lacks the requisite familiarity, repetition and stability of inquiry—is the reason why the authority of the Bosnian Pyramid project is intellectually unsustainable. Both Çatalhöyük and the Bosnian Pyramid project ultimately demonstrate that authority is an outcome of complex social and physical factors, that nonhuman actors and processes play a critical role in stabilizing and establishing that authority, and that this sense of stability is central to the maintenance of authority over time.

6.3.3 Authority in Epistemic Dependence

6.3.3.1 Defining Epistemic Dependence

One of the major questions that emerges from this research relates to the concept of epistemic dependence: how does one become 'an authority' or an expert in archaeological practice? Why do we trust some accounts over others? Why do we come to depend on or trust certain epistemic authorities, lending them executive authority over physical and intellectual space? Fundamental underlying issues about authority in

archaeological practice centre upon the ideas of trust, certainty, expertise and epistemic dependence.

'Epistemic dependence' is the idea that knowledge—particularly scientific knowledge—depends on indirect evidentiary support for that knowledge. In many cases, people believe something to be true but do not possess evidence for that belief; instead, they trust and rely upon the intellectual authority of experts who assert that they have the necessary evidence for that belief (Hardwig 1985). As philosopher John Hardwig notes, the amount of knowledge in the world is essentially infinite, and each individual is finite. Most scientific knowledge is built upon the work of multiple people, experiments and arguments. In most cases, an individual researcher or member of the public may not have the time, resources or sometimes even the capability to replicate or test the previous results from which her own scientific knowledge relies upon. Philosopher Michel Blais explains further that "[w]e must trust the evidential reports of others, simply because physically we cannot start from scratch. Whatever worth science may have, it requires this trust; for it is by and large a cumulative enterprise and no one individual can shoulder the evidential load" (Blais 1987: 369). Our reliance on epistemic dependence is a critical part of our everyday practical lives, and informs the way we think and approach anything from driving a car or following a map, to developing new theories in scientific research. In the practice of archaeology, epistemic dependence impacts how archaeologists build upon their scientific methods and theories, and it impacts the way the public receives archaeological accounts of the past that are constructed by others.

Conceptually, authority in scientific (and archaeological) practice heavily depends on epistemic dependence, creating two issues of note. First, epistemic dependence results in chains or ranks of authority and status, which can be followed back and linked to any given knowledge proposition. Secondly, as this thesis has demonstrated using the case studies of Çatalhöyük and the Bosnian Pyramids, the actual practice of epistemic dependence is a messy affair where social cues operate alongside tacit and tangible realities. This "mangle of practice" (Pickering 1995) directly affects epistemic dependence and thus, the acceptance and authority of any given knowledge proposition. To elaborate on the first point, knowledge generally relies on the abstract leap between what we 'know' from first-hand evidence experienced with our own senses, and what we 'know' from second-hand accounts told to us by others who claim to have first-hand evidentiary support. This creates a unique context of epistemic authority, and in many cases, of executive authority as well: on the one hand, first-hand and witnessed evidence for a given knowledge proposition is fundamentally more

valuable and useful than second-hand, received evidence. Therefore, persons who witness first-hand evidence for certain knowledge propositions usually have higher status and authority than others who must rely on second-hand evidence to build upon or contest that knowledge.

For example, a doctor who personally conducts a study on the affects of smoking on the human body, who personally tests and observes evidence that smoking causes cancer, generally has more authority and status on the subject than a second doctor who uses this evidence to tell a patient about the cancer risks in smoking.⁷⁰ Furthermore, if this patient then advises her friend about the new knowledge about smoking that she has learned from her doctor, she too is drawing on epistemic dependence. This exemplifies how epistemic dependence can result in chains or ranks of authority and status, which can be followed back and linked to any given knowledge proposition. If one were to 'rank' the authority of epistemic dependence in this scenario, the human and nonhuman agents involved would result in a complex matrix of what is considered to be expert and lay expertise, higher or lower epistemic authority.

'Expertise', as regards epistemic dependence, is a dialectic of trust and deference between two or more parties. The first doctor in the example above arguably has the greatest epistemic authority due to his exposure to first-hand evidence. In other words, he can be defined as an 'expert' because of the valuable knowledge he has accumulated, and in the way he translates that knowledge as authoritative to others. The second doctor operates the complex role of being both a lay person as well as an expert, regarding this specific knowledge proposition about smoking and cancer. He is not the most authoritative expert because he himself has not witnessed the evidentiary link between smoking and cancer first-hand. However, he is a secondary expert, in that he presumably has required the appropriate training and expertise that allows him to recognise and critically discern what makes for a solid medical experiment. This understanding of epistemic dependence involving the second doctor creates a complex relationship between the knowledge proposition and the idea of what constitutes expertise and 'knowing' something, an inherently complicated and messy reality. Finally, the third person—the patient—in this scenario begins as a lay person. After receiving the knowledge proposition about smoking from the second doctor, the patient trusts the doctor's expertise because of the context in which it was given to her and, therefore, she

⁷⁰ The real study is in: Doll, R. c. and A. Bradford Hill (1950). "Smoking and Carcinoma of the Lung." *British Medical Journal* 2: 739-748. I use this example because this quantitative epidemiological research, along with other related studies, forcefully and notoriously established the epistemic authority of both the scientific study itself and the research finding that 'smoking causes cancer'.

trusts the doctor's epistemic authority on this matter. Here, *context* is a crucial key to the authority of the knowledge proposition, the ease of its reception, and to the acceptance of epistemic dependence and expert authority. This will be discussed further below.

This matter of context brings us back to the second issue of epistemic dependence and epistemic authority: in actual practice, epistemic dependence is a messy reality that operates through tacit and tangible social cues, which directly affect the acceptance and authority of any given knowledge proposition. For any given knowledge proposition, we trust and accept the testimony of experts based on their performance and acceptance of social cues, which we draw from a host of social institutions—establishments, rules, mores, standards, accreditations—and then we immediately assess the viability of a knowledge proposition from those cues. Credentials, institutions, qualifications, authoritative logos and brandings, speech acts and so forth impact how we negotiate and judge testimony from experts. Therefore, the way knowledge is *presented* and *performed* is important to how epistemic dependence and scientific authority are established.

To refer back to the example of the doctors and the proposed link between smoking and cancer, the patient in this scenario trusts the second doctor's advice not only because he is a doctor, but because he is an 'authorised' authority. The patient regards the second doctor as an expert because she presumes the doctor has the relevant training to recognise valid secondary evidence. This presumption might be drawn from the way he is behaving or performing as a doctor (wearing the appropriate white lab jacket, sitting in a chair opposite to the patient and not on the patient table, wearing a stethoscope, referencing the appropriate medical journals, etc.), and from the way he is inhabiting the physical space of a doctor, like working in an appropriate office with an appropriate staff. It also might come from institutional credentials that 'authorise' him to behave like a doctor (for example, the patient might see an appropriate medical degree from an institution, which presumably determined whether he is competent in medical affairs, and which is physically hanging on his office wall or is listed alongside his name in a book). Furthermore, the doctor must also have authorisation by the state, with license to practice in his physical space; if he is practicing medicine without the state's permission, he will eventually be forcibly stopped.

Significantly, the patient is also constantly testing the doctor's competence, and hence his authority, by judging the ontological world—in other words, she judges the success or failure of the doctor's advice and diagnoses. If the doctor gives a wrong diagnosis—for example, if he wrongly declares her cough to be due to an allergic

reaction and not to her smoking—then the patient may doubt his authority and expertise if she sees no benefit from his treatment. On the other hand, the patient is also constantly testing the doctor’s competence on things that might be purely based on social context—unrelated to the ontological world at all. If, in the scenario above, the patient thought that she herself possessed evidence that contradicted the doctor’s assertion that smoking causes cancer (personal knowledge of a long-lived and cancer-free uncle who smoked his entire life, for instance), then she may disregard the doctor’s expertise or authority on this matter, despite its ontological significance. To further complicate things, the patient may trust or accept her doctor’s advice based on a long personal history or relationship with him as a family doctor or a family friend—something which may or may not relate to his medical expertise, credentials, nor the ontological world at all, but rather a social justification of loyalty or a personal sense of trust. Related to this, the patient may base her judgement of the doctor’s assertions purely on his social reputation as a reliable, famous, knowledgeable or authoritative medical practitioner. Again, epistemic dependence and a person’s acceptance of a given knowledge proposition may have no immediate association or relation to the evidentiary support at all; though, I should stress, there is always ontological significance behind knowledge claims. In actual practice, epistemic dependence is a messy and hermeneutic affair, entirely dependent on an ongoing negotiation between a complex network that includes: individuals, materials and evidence; the institutions that authorise them; the practices and performances of accountability and expertise; and the ontological world.

6.3.3.2 Epistemic Dependence in Archaeological Consumption, Validation and Fidelity

In archaeology, epistemic dependence is doubly important, because a defining characteristic of archaeological knowledge is the awareness that any true and exact validation of archaeological data is rarely—if ever—possible (c.f. Read 1989). When we study the past, we may deduce and construct a ‘hard’ understanding of material properties of certain things. For example, we can answer some of the ‘how’ or ‘what’ questions about the past (i.e., How was a pot fired? What temperature was required to set the wet clay? How did a skeleton appear in a pit underneath a house foundation?). But we can only inductively reach answers to ‘why’ questions to create ‘soft’ holistic narratives about what happened in the past (i.e., Why was a woman buried with a plaster skull under a house foundation? Who made that pot?). This conundrum of having

only partial evidence and partial understandings is at odds with the fact that our ultimate 'products' in archaeology are holistic and stable accounts of the past.

Thus, the discipline of archaeology as a knowledge-producing culture rests on a system of epistemic dependence that heavily relies on individuals and institutions acting as intellectual authorities, whose role is to suggest which specific artefacts, sites and final interpretations have 'fidelity' to the past. The term 'fidelity' comes from the Latin word *fidelitas*, meaning 'faithfulness', and it references how accurate a copy or simulation is to an original (OED 1989). This notion of loyalty or fidelity to historical accuracy is the ultimate aim of a re/constructed narrative of the past, and it is what an authoritative account aims to prove. There are three important points on this subject to consider further.

First, authoritative accounts in 'final product' form—such as textual accounts in reports, media stories, museum displays and historic reconstructions—are usually the first point of contact for most people outside of the core scientific community with access to the original material. This point of contact with archaeology relies heavily on the consumption and context of authority. Like the example of the doctor and the patient in the previous section, the first point of contact with 'final products' usually relies on the authority of performance and the acceptance of that performance for meaningful contextual reasons. A patient who walks into a doctor's office initially negotiates the authority of her doctor by his appropriate appearance and performance of a doctor; then she negotiates and accepts his promotion and record of credentials; and only later does she negotiate and judge a kind of ontological validation of her experience with his advice. Similarly in archaeology, most members of the public and the broader scientific community, outside of the 'core' team members who are able to access material, rely heavily on the authority and epistemic dependence of archaeological performances. In preparing their displays, texts and other 'final product' accounts of the past, archaeologists operate with a tacit understanding about the best way to present their ideas coherently and authoritatively. They stabilise and solidify all of their messy social interactions that led to their conclusions, and then 'black box' their output in solid, clear and simple accounts meant for public consumption. They follow institutionalised formats for their target audience—whether for text meant for scholarly journal articles, or creative images for public museum displays—which use the appropriate context, language and presentation that will maximise the appearance of their authority and advertise their fidelity to the past. In this production of 'products' meant for 'consumption', archaeology itself becomes "a mixture of humans and non-humans, texts, and financial products that have been put together in a precisely co-ordinated sequence"

(Callon 1991: 139). In archaeology, the aim is to create a valuable, consumable product, to sell to the public and sell to ourselves. As the case of the Bosnian Pyramids illustrates, for many members of the public, this product can be seen as valuable simply if it positively contributes to the socio-political climate and economy, and this is opposed to how many members of the scientific community may see a product's value, where an account must also contribute a 'faithful' record for our ontological understanding of the past.

As Harry Collins and Robert Evans write, "As with language, so with the expertises analogous to language—coming to 'know what you are talking about' implies *successful* embedding within the social group that embodies the expertise" (Collins and Evans 2007: 7). As this thesis has argued, 'successful embedding'—epistemic and executive authority of any given archaeological project, person or account—is an accumulated effect. Authority from 'final product' inscriptions is drawn from the manipulation of asymmetric power relations (politics) and from appropriate performances which legitimize and authorize social arenas of practice. In academic arenas like archaeology, authority can be strategically constructed by using science as a master discourse. As the case of the Bosnian Pyramids illustrates, by drawing on institutions and recognised categories of practice in archaeology, one can construct a means to archaeological authority. Semir Osmanagić's pyramid project is particularly successful because of what I call the 'outsourcing' of ethics and authority. For example, Osmanagić's Foundation has employed accredited scientists to use real scientific methods on genuine ancient material to come up with genuine prehistoric dates for material in his site, like the radiocarbon dating the team performed on organic material yielding a reliable date of +/-35,000 BP. However, the 'final product' interpretation of "35,000 year-old manmade pyramids" is a gross leap in logic, because the ontological significance of the dated organic matter does not lend authority to this interpretation.

A second important consideration in this thesis, which expands upon this latter point, is that even for most members of the general public who rely on context, ontological validation for epistemic dependence is still important and central to the idea of what 'authority' is in the production of knowledge. Epistemic dependence, and thus authority, in archaeology is anchored to a product's ontological worth. In the case of the Bosnian Pyramids, for example, some members of the public (like aspiring politicians and café owners in Visoko) may support the account of Palaeolithic pyramids purely because of the money the project brings to the economy. However, the vast majority of supporters actually think that the project has ontological worth, because they have been convinced by clever media manipulation and performances that Osmanagić's account of

pyramids is defended with evidentiary support. They believe Osmanagić and his team are ‘doing archaeology’, based on their judgement of his performance as an archaeologist, and by the appearance of things that the pyramid team has unearthed that are seemingly ‘verified’ through scientific conferences. If Osmanagić and his team did not maintain their public performance through one-step-removed media, or if their façade was broken by closer examination, then the site would lose public authority and status—which is what is ultimately happening when professional archaeologists are looking at the project more closely and opposing its claims.

It is unlikely that the Bosnian Pyramid project’s authority is sustainable, because as more people gain greater understanding of the context and actual evidence, the ontological world that contradicts the team’s findings will intrude upon its performance. This case study also shows that authority and acts of legitimation are employed and distributed through the medium of science, and they need to be actively performed in order to acquire and maintain status. Executive access also plays a critical role in this performance. For example, with the Bosnian Pyramids site, individuals like Semir Osmanagić sit in key positions as ‘gatekeepers’ in the ‘interpretive gap’ between all of the scientific outsourced practice and the final interpretations; thus they affect and control the outcome of the accounts and interpretations that the team produces. Osmanagić holds ultimate control over the final accounts that are presented in ‘final product’ form on the project’s websites, reports and books, and which intentionally black-box all of the messy activity that went into their production. Archaeological accounts may be stabilised and authorised through scientific practices, but people gain authority through positions as gatekeepers and their executive control over aspects of the knowledge production process.

This consideration leads to a third important point in this thesis: that the process of ‘gatekeeping’ and access control is present and central to the way authority operates in the professional discipline. As the case of Çatalhöyük illustrates, ‘gatekeepers’ (like Ian Hodder as site director and like the field excavator in the case of the plastered skull burial) can hold influence over interpretation through their executive control over key access points in physical and intellectual space. In the case of Çatalhöyük, despite a desire or intent to allow multivocal interpretive access to flourish in a postprocessual theoretical program, specific team members on site have had more or less authority and authoritative presence based on their personality or ‘charismatic authority’ (c.f. Weber 1978),⁷¹ as well as their relative position on a site executive

⁷¹ Charisma is defined in the Oxford English Dictionary (1989) as: “compelling attractiveness or charm that can inspire devotion in others”.

hierarchy. In 2009, team members with more executive authority on the site were allowed more physical access to material and space. Their authority was complicated by the fact that their epistemic and executive presence was built from years of experience with the geographical region, demonstrated knowledge of the Neolithic past, as well as personal duration and experience with the Çatalhöyük dig house space itself. Importantly, in order to gain epistemic and executive authority, an individual had to have closer access to and experience with material, which accumulated as a representation of their familiarity with the material and translated into expertise. A dialectic of “resistance and accommodation” (Pickering 1995) ensues in such a case, where facts gain authority and are socially constructed through their ontological boundaries and their social translation by certain people. Equally, certain people gain authority and become “socially constructed as factual agents” (Van Reybrouck and Jacobs 2006) through their interaction with archaeological material, bounded by the material and physical properties of the objects and space that they interact with. As noted previously, “Excavation seems not so much a process of *salvaging* but of *solidifying*” (Van Reybrouck and Jacobs 2006: 34, emphasis in original). It is through the repetition, familiarity and stabilising of spaces, time and fluid practices that authority is built and translated by many different people, and narrowed by those who have more social power and positionality. But it is always constrained by the material nature of its context and the ontological world.

6.4 Dealing with Authority: Suggestions for Further Research

This thesis has argued for the circumstances under which we make given authoritative interpretations, explanations or predictions in the production of archaeological knowledge. However, I conclude by asserting that this process still only “explains why we make them—but leaves untouched the question of our license for making them” (Goodman 1983: 60-61). A yet underlying issue on this subject is the question of what ethically gives ‘us’ the right to access material remains, while ‘they’ have no such right? What gives a professional the authority or right to account for the past and control access to archaeological materials? This discussion of territory rights,

ownership and multiplicity has been a referenced debate in postmodern theory in archaeology over the last twenty years. Yet one of the most provocative questions from this discussion has yet to be fully answered: is it our professional, ethical obligation to actively encourage multiple interpretations from within our own disciplinary boundaries? Archaeological sites are space and territory—both physically and intellectually—and one individual or group always has more right or control over access. When two or more things compete for intellectual or physical space, there is almost always an asymmetry of power. Further research is needed to address some of the ethical concerns that emerge this study on the nature of authority: if archaeological interpretation “begins at the trowel’s edge”, then it is important to continually address who is holding the trowel. How does the physical access of archaeological remains directly impact intellectual access and the epistemic authority of interpretations? By understanding the exact nature of the way we construct authority, what does that nature imply about the ethics and accountability of the discipline?

Another fundamental line of research that would greatly benefit from further examination is the exact nature and role of the nonhuman, ontological actors in the production of archaeological knowledge. This thesis has established that they enable and constrain our archaeological authority and the validity of our interpretations; however, due to constraints of space, it lacks further study that traces the exact impact of specific types of inscriptions or different technologies that are critical to scientific output. A particular type of technology that this research addresses is the science of radiocarbon dating, which has had a very powerful and important role in the history and development of knowledge in the discipline. Further exploration into the authority and power of technologies like radiocarbon dating—as ideas embedded in popular culture, as well as critical scientific methods in the field—is warranted. Further interesting questions have also emerged during this research regarding the authority and agency of specific *types* of archaeological products and technologies. For example, how does authority and reception of knowledge differ by the production and consumption of different types of archaeological representation: archaeological images, diagrams, physical reconstructions, museum displays? How might inscriptions like archaeological photographs relate to the concepts of building trust and expertise, or ‘active witnessing’ by the public? How do our inscription practices materially create public and professional trust? Although some new research is beginning to address some of the implications and affectations of archaeological images and witnessing, further exploration is needed in deconstructing exactly *how* these practices operate within disciplinary practice (Moshenska 2009; Perry 2009).

Finally, a broader historical examination of the nature and role of authority in archaeological throughout the professional history of the discipline would greatly benefit the field. How has authority in the discipline of archaeology changed or developed through time? Archaeology began as an amateur activity before it professionalised and became 'scientific', and in this process a great deal of weight has shifted to the burden of validation and authority, and in the materiality of this process of authorising interpretations. A detailed historiographic study which specifically traces the role of authority—and the impact of nonhuman as well as human aspects of this process—in disciplinary development would be of critical interest. By continuing to develop such lines of well-informed and ethically aware self-study on authority, we can contribute to better practices and a more humane world.

Appendix A

Çatalhöyük Database Entry: Feature 1517 (plastered skull burial).
Publicly available on the Çatalhöyük Project website: <http://www.catalhoyuk.com/>



Image Placeholder

Appendix B

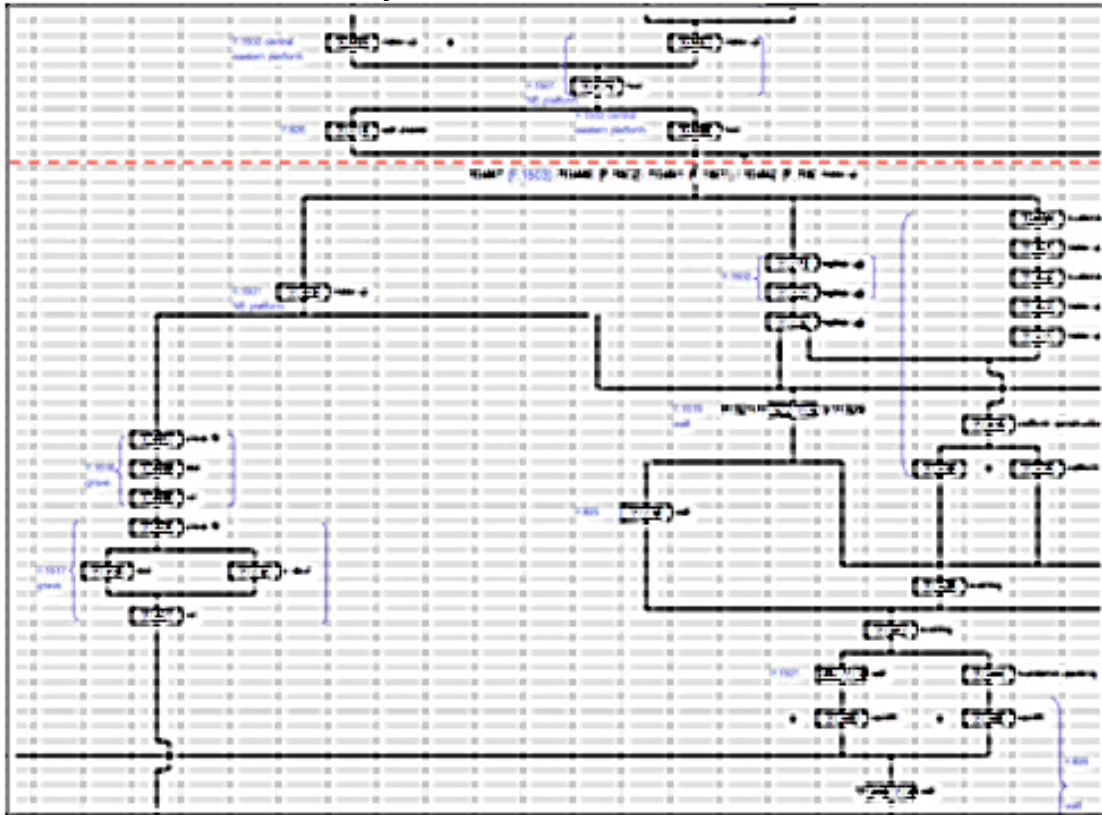
Çatalhöyük Diary Entry: 30/07/2005.

Publicly available on the Çatalhöyük Project website: <http://www.catalhoyuk.com/>

Image Placeholder

Appendix C

Sample of a Harris Matrix Chart.



Appendix D

Article and comments by Michael Baltar at ScienceInsider.

Publicly available online: <http://news.sciencemag.org/scienceinsider/2010/09/hodder-cleans-house-at-famed-ata.html>

Image Placeholder

Original image can be found online at:
<http://news.sciencemag.org/scienceinsider/2010/09/hodder-cleans-house-at-famed-ata.html>

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Original image can be found online at:
[http://news.sciencemag.org/
scienceinsider/2010/09/hodder-cleans
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-house-at-famed-ata.html](http://news.sciencemag.org/scienceinsider/2010/09/hodder-cleans-house-at-famed-ata.html)

Appendix E

Email Interview with Semir Osmanagić

Date: 21 March 2007

Q: I know you have worked with archaeologists and other experts in the past. Are you planning to consult any more this season?

A: OF COURSE. THIS PROJECT IS OPEN FOR EVERYONE. BUT, ARCHAEOLOGICAL WORK DOES NOT BELONG ONLY TO ARCHAEOLOGISTS NO MORE. WE'RE GETING INVOLVED A NUMBER OF GEOLOGISTS, GEOPHYSICISTS, GEODETIC AND MINING EXPERTS, ASTRONOMERS, PALEONTOLOGISTS, ETC.

Q: Have you found any artifacts or material culture yet? If you find artifacts, what do you do with them? Theoretically, if you accidentally find artifacts from a non-pyramid time period (Illyrian, Roman, Medieval, etc.), what is your plan of action?

A: WE HAVEN'T FOUND ANY ARTIFACTS THAT BELONGED TO THE ORIGINAL BUILDERS. ACCORDING TO THE LAW, WE'RE OBLIGATED TO SEND ALL ARTIFACTS, NO MATTER WHAT PERIOD, TO THE LOCAL MUSEUM.

Q: The people who built the pyramids must have lived somewhere; where do you believe archaeologists will find these settlements?

A: AS SOON AS THIS COMING MAY WE'LL BE DOING SOME DIGGING IN VILLAGE GORNJA VRATNICA 4 KM FROM BOSNIAN PYRAMID OF THE SUN. WE MIGHT FIND SOME BURIAL SITES OVERTHERE.

Q: How many people are employed by your Bosnian Pyramid Foundation?

A: DURING THE SUMMER WE GO UP TO 85 FULL-TIME EMPLOYEES. IN THE WINTER WE HAVE APPROX. 35 EMPLOYEES.

Q: I hear you are currently working on your PhD entitled 'The Maya Civilisation.' What is your thesis? Does it also research pyramids? What university are you researching under?

A: THE THESIS HAS BEEN RESEARCHING UNDER UNIVERSITY OF SARAJEVO AND IT DEALS WITH THE MAYAN CIVILIZATION. I'VE COMPLETED THE WRITING AFTER VISITED MORE THAN 50 MAYAN RUINS IN MEXICO, EL SALVADOR, GUATEMALA, BELIZE AND HONDURAS. AND YES, EVERY MAYAN CITY USED TO HAVE PYRAMIDS.

Q: How do you feel the broad professional archaeological community feels about your project?

A: EVERY NEW IDEA HAS OPONENTS IN THE BEGINNING. THE BIGGER THE IDEA, MORE AGRESSIVE THE OPONENTS. BUT, IT DOES NOT INFLUENCE MY GOALS AND DETERMINATION FOR AN INCH.

Q: I understand that you have recently been working on a documentary, which sounds exciting. What is it about, what TV network is it with, what language will it be covered in, and how does it tie into your BiH pyramid site?

A: BOSNIAN TV IS DOING A 12-EPIISODE DOCUMENTARY CALLED "SEARCH FOR THE LOST CIVILIZATIONS" BASED ON MY BOOK "CIVILIZATIONS BEFORE THE OFFICIAL HISTORY BEGAN" (PUBLISHED IN SARAJEVO). WE'VE ALREADY FILMED AT THE FOLLOWING LOCATIONS: EASTER ISLAND, BOLIVIA, PERU, MEXICO, COSTA RICA, UK, FRANCE, GERMANY, MALTA, EGYPT... CURRENTLY I'M IN JORDAN. I GOT LEFT LEBANON, CROATIA, MONTENEGRO AND BOSNIA. WE WANT TO SHOW THAT BOSNIAN PYRAMIDS, STONE SPHERES AND MEGALITHIC WALLS IS NOT EXCEPTION BUT VERY COMMON ON FOUR CONTINENTS.

Q: You have had a lot of media attention (I am originally from Houston and saw a broadcast there, as well as other broadcasts through the web). Do you generally contact TV organizations, do they contact you, or is it a mixture of both directions?

A: IN MOST CASES, TV OUTLETS CONTACT US. OF COURSE, INITIALLY, IT ALL STARTED FROM OUR SIDE.

Q: What type of media do you believe has been most influential in spreading information about your site? Internet, television, newspapers, etc?

A: TV

Appendix F

Email Interview 1 with Andrew Lawler

Date: 24 November 2009

Q: *What was your official title and position with the Visoko team? What was your job like on a daily basis - a 'day in the life' excavating at the Moon site?*

A: My official title was initially 'Archaeological assistant. Within about 6 weeks of me arriving, the 'Permanent Archaeologist' Raffaella Cattaneo (who had dubious qualifications, including an apparent PhD from Bristol in 'Minoic'(!) Archaeology) resigned due to perceived sexism, and I was promoted to this position. However, not wanting to become heavily entangled in their project, my official title according to my contract was 'Excavation Coordinator'.

Essentially, my job involved the overseeing of 5-6 labourers at a time, as well as excavating myself, recording all trenches, taking samples of any organic deposits, and training a photographer how to photograph in a manner that is archaeologically acceptable. As time progressed, my main concern shifted to protecting preserved field/drainage systems apparent in Sonda (trench) 28, Sector F, Grid 1, (spreading in a Southeasterly direction) which were heavily truncated by quarrying and a wartime trench, but had small quantities of burnt stone throughout the fill. This was uncovered in late September 2007, as I recall. The labourers up on this site could understand the basics of stratigraphy, and excavated/stripped at 2.5m intervals, so the sediment profiles could be photographed and recorded before carrying on. Other parts of the 'Moon Pyramid' I had no control over. One man (Dzeno?- owner of 'cool shadow') hacked away haphazardly, but it was his own land, so I let him do as he pleased, with the caveat that if he dug without telling me, I wouldn't attempt to record it. There were also volunteers digging on the slopes in early summer 2008, but I was not informed of it until after they left.

On the 'Moon' summit, a typical day would be:
Arrive 9.15-10, depending on weather/any kit required.
Coffee until 10-10.30.

Overview of work done after I'd last visited (I only went up there 2-3 times a week, as I was trying to make sense of artefacts (real and fantasy) found in 2006 excavations on Visocica, to give them archive numbers and as good an explanation as possible of where they were found from any descriptions with them). Jasmin would then photograph any profiles, and I'd fill in the paperwork.

Then, I'd excavate alongside the workers until lunch (12.30ish). After lunch, we'd start back around 1.30. The general idea was that I'd dig alongside them, unless they came across any dark sediments (generally manganese) or anything unusual, which I'd record, photograph in situ, and bag up, assigning a field number. At about 3.30, I'd discuss with Amidza (the land owner and foreman) in order to lay out any new trenches Semir wanted in a way so that they wouldn't affect his crops or the excavation method (which was focused upon neatness, to impress visitors), then leave to file any profile/grid sheets filled in, turn the field numbers into catalogue numbers, and put any samples taken into storage, while Jasmin put all photographs on the computer, separating archaeological ones from touristic/promotional ones, so their file names &

locations would not be changed.

Q: I know you started to implement some archaeological structure while you were there, like inventories and recording. What kind of recording did they do on site before and after you came?

A: None whatsoever, from what I can tell. I was told there was a 2006 report, but never saw it. It was obvious from looking at the 2006 arefacts that someone with excavational experience had worked on the digs, as they had often put as much detail as possible onto scraps of paper included in the sandwich bag (these varied from notebook pages to cigarette cartons). I never found out who this person was.

Q: What kinds of artefacts did you find, and what was the stratigraphy like? In retrospect, how do you interpret what was going on archaeologically at the site? What periods and types of material were you digging?

A: In my time there, the preserved wood in Ravne was found (later destroyed by Muris Osmanagic), as well as several carbonised samples taken from Sonda 28, Sector C (various profiles). Apart from that, nothing predating the recent war (ration packs, bullets and cases, patches of burning) was found except the burnt stones already mentioned. There was a lot of material found in KTK Tunnel, but I refused to work in there without first seeing a safety report. One of the workers told me he'd been instructed to throw away anything under 200 years old. I managed to convince him to keep a small sample of material recovered once a week (in order to provide an approximate stratigraphy of the tunnel infill), but he quit the job about 3 weeks later, so all we had was a piece of metal plate (which later got lost) and an industrial ceramic tile (kind of like kiln lining, but a finer matrix). If I were to hazard a guess at the date of the field system uncovered in Sonda 28, Sector F, I'd say Iron Age, but that's more guesswork than anything else.

[...]

Q: How many employees vs. volunteers were there? Were most of the volunteers local?

A: The number of labouring employees varied hugely. At an estimate, I'd say in July 2007, there were 25 at Vratnica, 15 at Ravne, 10 at KTK, 6 on the Moon summit, another 10 or 12 around the rest of the moon, plus Zombi's itinerant team of 4-8. There was a steady, but low, flow of volunteers, with I'd say 4-10 at any given point throughout the summer. By May 2008, this had dropped to an unknown amount (less than 10) at Vratnica, none bar Dzeno(?) on the Moon side, 4 on the Summit (who quit and barred Semir from going up there a few days after my resignation), none at KTK after work finished there in mid-Sept 2007, and Zombi's team had joined up with the remaining workers in Ravne, to make a total of 9, I think, there. During summer 2008 there were a total of 13 volunteers working at any point (several lasting only a few days). These were- a young couple from Slovenia, a Bosnian diaspora archaeology student (who left and worked in Sarajevo museum for the remainder of his stay), a retired Australian guy who lived in the town, a Canadian museum conservationist, the 2 unknowns working on the Moon slopes, and 6 students from KU Leuven (part of the reason I ended up here, although they came after I'd resigned, but was still living in Visoko). There were no other volunteers in 2008, except locals working the odd day or 2, but this was essentially to help out their friends who were employees.

Q: How much attention did the site receive when you were there? What kind of attention? From the public, Bosnian politicians, international politicians, artists, schools, academics?

A: There was a lot of attention for the first couple of months- local TV crews & national newspapers more than once a week, and journalists arriving from abroad roughly once a week. Most of the attention was focused towards Semir and the Egyptians (whilst they were there), and most of it was off-site. Nabil Swelim and his entourage spent under 3 hours visiting sites altogether. I met a few politicians on a National scale at the start, although later on, this dwindled to essentially local interest, and caused a minor problem, as I befriended Asmir Hodzic, SDP Mayoral candidate, slightly to the vexation of Munib Alibegovic, incumbent mayor, and pyramid supporter. This was quite well known in the town, and I got the feeling in the run-up to the October 2008 elections (which began while I was still working for the Foundation), that this was frowned upon by Alibegovic.

Q: Where do you think the project is now? Is there still the same kind of hype now, as opposed to three or four years ago? Where do you think the project is headed?

A: The Foundation have apparently just announced a summer camp for 2010. However, I know 'opponents' of the project are planning to launch a campaign highlighting the lack of safety reports for the tunnels, carcinogenic molds and fungi growing in them in abundance, the fact that nobody will actually be excavating the 'Sun Pyramid', as the Foundation lack permits, and actual volunteer numbers for the past few years, and raising questions about insurance for volunteers.

There is nowhere near the same hype now as in 2006. Even in 2007 businesses within Visoko were redirecting their focus away from 'Pyramids', and the only evidence I saw in spring this year (2009) of the initial 'Pyramidomania' was the leftover tat being sold in bric-a-brac stores near to the market. The 'Srcem za Piramide' festival in April (the official opening of 'digging season') did not extend past 3 or 4pm (2008's had gone on until well after 10pm, although had been poorly attended, and badly reviewed) and had no mention of plans for the coming 2009 season, instead focusing on cultural events, such as a fun run and rafting gala. In fact, as far as I know, there has been no archaeologist employed or consulted by the Foundation since my resignation in August 2008.

Appendix G

Email Interview 2 with Andrew Lawler

Date: 9 July 2010

Q: Other than your own work, what kind of professional research has happened at the pyramid sites while you were there and before you came? Have you heard of anything after you left?

A: While I was there, there was a conservationist who came for a few days. She was Canadian, but married to a Bosnian, and came to volunteer for a few days whilst they were visiting family in Sarajevo. Apart from her, the Russian scientists and the Egyptians were the only researchers who 'worked' there that I saw. Apparently, a man undertaking core drilling also came, but I never met him. When I arrived, an archaeologist called Rafaella Cattaneo was also working there, but her qualifications and experience were dubious, to say the least.

After I left, nothing has been done to my knowledge. According to friends, and what I can gather from the occasional press release I read, there's been no archaeologist working there since I left. The person meant to be leading excavations at the moment is a Croatian guy, who, from what I know, is an art historian whose previous work has been on the history of woodblock printing.

[...]

Q. What has been the role of the Egyptians at the site? Do you know why the Egyptians - particularly Swelim - are so supportive?

A: Apart from Aly Barakat, their role was little more than that of tourists. I know that some, particularly Mona (Fouad Ali? I'm not sure, but it wasn't Mona Haggag- I met her for the first time at the conference) was disappointed in this, as they felt they were being used as promotional tools. Swelim, on the other hand, thrived on this. He's ex-military, and is used to entertaining, very comfortable with the media etc. I seriously doubt his credentials as a serious archaeologist though. He's never held an academic tenure, and received one of his PhDs from a Hungarian university very shortly after his retirement from the army. I think it's pretty odd that an Egyptian would choose to study Egyptology in Hungary after a relatively prominent military career, and wonder whether the award of this may have been politically motivated, especially considering the 'report' he wrote after spending under 2 hours on Visocica.

The Egyptian ambassador to Bosnia is heavily involved with the Foundation (as is the Malaysian embassy), and I assume it's his influence that got the Egyptians over.

[...]

Q: Do you think the authority of these scientific institutions carry a lot of weight with the public? Or is the public more disinterested now?

A: I honestly couldn't say. The thing is, since the whole debacle with Oxford (twice- if you don't know the details on this, I'll be happy to fill you in), they seem to have been a bit more careful on the use of names of institutions. However, Osmanagic still gets away with claiming that he's a member of the Russian Academy of Natural Sciences- alongside many Nobel

Laureates. In reality, it's the Russian Academy of Sciences to which the Nobel winners belong, and he's a member of something totally unaffiliated, and founded in the 1990s. The whole does he/doesn't he conundrum with Osmanagic's PhD is still unclear, and many people in Visoko see him as a charlatan.

The fact that the Foundation continuously change their agenda (the conference was supposed to be biennial, if you remember), and have tried to mess the town around to suit them has lost them a lot of face with organizations who were previously more than happy to help. For example, 'Srcem za Piramide', the official opening celebration, always used to happen in April, with a rafting exhibition by the local club. This year, they moved it to June, and advertised that the rafting was going to take place as normal, without asking the club. However, the river is too low in June to raft safely, so they kind of pissed off the club with that blunder. I'm not sure how it all panned out, as I haven't spoken to anyone about it since.

Q: You mentioned the students from KU Leuven came to work at the site while you were there. How did they hear about the excavation, and why did they choose Visoko to excavate? Do you know of any other university groups that came to work at the site? What was their impression of the excavation?

A: I think they heard about the excavation on the news or the internet. They organized it as a group themselves independently from the university, fully in the knowledge that Leuven would not give them accreditation for their digging as part of their compulsory undergraduate work.

No universities have excavated at the site, as none have recognized it as a bona fide dig. Instead, individual students have gone there out of interest. Any belief or impression that this gives the Foundation's work official recognition from a university or other institution is wrong. During my tenure, there was an archaeology student from Trieste with family friends in Visoko, the conservator mentioned above, and a Bosnian-French architecture student, who used the excavation as his compulsory internship for Lyon (possibly Lille) university.

Q: The actual pyramid hypothesis is a bit fuzzy to me, so I'm hoping you can help clear things up: According to the Foundation, are the pyramids supposed to be made of artificial blocks covering a natural hill, or is the entire hill supposed to be man-made from blocks?

A: This changes continuously. The Foundation's primary stance is that it is 100% man-made, unless a supporter of theirs is proposing an alternative explanation at the time, which they then say is plausible, and use this as a means to justify 'further research'. However, the chronologies suggested by the foundation contradict each other- they claim the 'megaliths' were put in place and carved prior to their burial by sediment, which forms the base of the pyramid. It was then shaped and covered with blocks, and then the tunnels were dug. It's supposed to be a block-covered man-made hill, in other words. This is what Swelim supports. Barakat suggests it is a natural hill which is artificially shaped.

Q: Do you know what period the Foundation says the pyramids are supposed to be from - Illyrian, paleolithic, etc.? How does the radiocarbon dating play into the team's hypothesis?

A: Definitely pre-Illyrian. Some have tried to connect it with the Butmir culture, particularly the nearby site of Okoliste. The general claim is that they were built before the last ice age. The older the better.

The radiocarbon dating is just one thing that helps. Getting dates of 40k years from 2 laboratories was great for them, as they could present that without showing the caveats (primarily that the dates are on the edge of the C14 limit). When Oxford refused to give the date as 'definitive', they released a statement implying this was some form of conspiracy.

Q: What does the team say about the people/settlements/human activity they think was happening at the site? Do they care more about the concept of pyramids, or are they genuinely interested in studying the prehistoric people who supposedly made the pyramids?

A: It's all about pyramids to Osmanagic, and also to many of the tourists that he attracts. There has been no effort at all to interpret the 'pyramids' in the context of the landscape, ancient river patterns etc. It's all about patterns and perfect geometry. They have no interest in more recent cultures (for instance the medieval town, or Neolithic settlement on visocica), and workers were told to throw away anything under 200 years old that they found.

However, some visitors and 'independent researchers' are very interested in the pyramids as monuments to lost civilizations, as opposed to being purely pyramids. The Hungarian and Bulgarian supremacists that come over present it as evidence of both their countries' power and influence in the past, for instance.

Q: What artefacts and structures did you and the team find that you think were genuine, and which do you think were more fantasy?

A: Nearly everything was fantasy during my time there. Only the burnt stones from the Moon pyramid were real and older than the war. At KTK tunnel, an abundance of 19th and 20th century stuff was coming out, but most of it disappeared, and I guess since I left the rest has been disposed of. No work was carried out on Visocica while I was there, and nothing was found in Ravne or on Vratnice that was real.

When I reorganized the artifact store, about 10% of what was in there was real. The rest was fossils or 'pretty stones'. There was some Neolithic and medieval pottery, a flintlock, an iron knife (presumably medieval) some nails and glass, and 10-20 animal bones, as well as some bone fragments.

Q: Previously you've mentioned carbonised material and the burnt stone you found at the site. There was also the metal mould and the stone building structure on the moon pyramid. What period would you guess this material is from? What kind of settlements/sites/material do you think this came from?

A: The carbonized material was indeterminate. It was sealed in well-stratified natural deposits at several locations. It was sampled correctly, and photos were taken of it in situ, as well as measurements. Unfortunately, the Foundation has all the paperwork. As to age and whether it's natural/man made, I can't say.

All the 'metal moulds' I was shown were natural rocks with odd indentations. Admittedly, one did look feasible as an artifact, but it had been so heavily cleaned, that nothing much could be said for it, except perhaps by a specialist. That is, of course, provided it is man-made in the first place.

The stone structure is odd. It has been speculated (<http://irna.lautre.net/Real-archaeology-in-Visoko.html>) that it's an iron age grave. However, a few things point to this not being the case. First, the soil underneath the structure is natural, and no body is in there. Second, I've

seen the nails that were found, and they're incredibly regular, suggesting an industrialized manufacturing process. Finally, the Foundation found this with incredible ease and accuracy, suggesting that either some of it was already protruding, or it had been in use in recent memory. I'd say it was the lower few courses of a storage shed of some kind, most likely the timbered ones that are found in that area, that dated from the 18th century or later.

Q: How does the New Age connection relate to the scientific activity happening at the site? Are they two separate spheres of people and activity, or are they intertwined?

A: I get the feeling they're intertwined. Ahmed Bosnic, on-off president of the Foundation earns his money selling spiritual trinkets, plus books on the paranormal and suchlike. Semir is heavily involved in New Age stuff in America, and his ghost-writer (Sharon or Karen, I think; possibly this one: <http://www.sharonprince.net/>) works with *Astraea* magazine, who do a lot of the Foundation's promotions and interview protagonists regularly.

The 'New Agers' seem to comprise the bulk of the tourists. They include the Bulgarian and Hungarian supremacists, who send regular tour parties, and the cult of Damanhur, as well as more independent New Agers who make their own way to the town, or come with Semir. It seems as if, as the Foundation has lost many sources of funding, they aim to appease these people as they are their last viable cash flow. To the media, Semir attempts to distance himself from these people, but in reality, they are pretty close to him, and some hold him in pretty high regard, being literally unable to speak in his presence (I saw this with my own eyes once with a group of Hungarians- the party leader turned bright red and was visibly very, very nervous when he arrived at the motel unannounced).

New age science is employed a lot- I think I've told you the whole Harry Oldfield story before, and the Russians' research is definitely undertaken without regard for archaeological principles, and the science they claim to apply can't be interpreted by anyone except themselves.

Q: Do you think the project is sustainable - in an intellectual sense, as well as a practical sense? Do you think the project will be around for years to come? Do you think the project can continue to adapt their hypotheses and practices to meet public demand/interest? Or do you think the project is unsustainable in the long run?

A: I have mixed feelings on this- there's the whole 2012 hypothesis to take into account, and to what extent Semir, funders and tourists actually believe in this.

I don't think the project is financially sustainable- one look at the staff turnover and continual relocation of administrative and archaeological premises tells you this. The fact they have limited archaeological equipment shows they do not have a serious approach to excavations, and promotional literature is always vastly over- or under-ordered, which suggests that people aren't employed for the right reasons.

I think the Foundation is hoping that the recession is the reason for its downturn in financial income, or possibly hoping that other people will believe this is the reason for it. For this reason, I can see them holding out by hook or by crook until 2012, then after that, who knows. They are running out of media which has an interest in the site, and are limited to recycling old storylines (ie 'new pyramid possibly found' 'famous person visits' 'scientific analysis supports hypothesis') which don't have the same impact as the first time they were used. Anyone who will ever visit the pyramids within Bosnia already has, and therefore the Foundation need to look abroad to attract more tourists and funding. Unfortunately for them, they're old news now.

Appendix H

Sample page from the Bosnian Pyramid's "Scientific Evidence" Booklet.

Image Placeholder

Original image can be found online at:
[http://www.bosnianpyramid.com/
images/pdf/Bosnian%20pyramids
ScientificReportMay2006.pdf](http://www.bosnianpyramid.com/images/pdf/Bosnian%20pyramids%20ScientificReportMay2006.pdf)

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